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JOHN G. GURLEY, Managing Editor
JANE LADD, Assistant

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BOOK REVIEWS

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BOOK REVIEWS

OTHER DEPARTMENTS

The American Economic Review

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ECONOMIC EDUCATION: ASPIRATIONS AND ACHIEVEMENTS

By G. L. BACH AND PHILLIP SAUNDERS*

In recent years, widespread concern has been expressed over the low level of economic literacy of the general public. Since apparently only a small portion (perhaps 10-20 per cent) of all future citizen-voters will ever take as much as one economics course in college,[1] attention has been increasingly focused on the possibility of developing at least a modicum of economic understanding through teaching in the high schools.

Concern for this problem has developed gradually within the economics profession, culminating in the policy statement adopted by the AEA's Executive Committee in March, 1964 and the establishment of a new action-oriented Committee on Economic Education [9, p. 565]. Prior to this action the Association's previous Committee on Economic Education had for some years attempted to stimulate economists to help high school teachers and administrators with this task, mainly through attention to the problem at annual AEA meetings. During the past five years, however, a more concerted effort has developed, including at least four major projects, two of which have involved AEA participation or sponsorship.

This article summarizes briefly these recent developments and then presents the findings of recent investigations of what economics is actually being taught in the high schools and by whom, and of the effec-

* The authors are, respectively, professor and assistant professor of economics at Carnegie Institute of Technology. The former is chairman of the AEA Committee on Education, and this article represents, in part, a report to the profession on the results of several special undertakings the Association has sponsored or encouraged. The authors wish to thank especially their colleagues, Michael Lovell, Lester Lave, and Leonard Rapping, for statistical advice at a number of points; and Mrs. Ann Brunswick of the National Opinion Research Center who supervised the NORC study on which a substantial portion of this report is based. NORC has a separate detailed report on its findings which includes extensive information on high school teachers and their backgrounds not reported here [2].

[1] Less than 50 per cent of all present high school students will enter college, and apparently only about one-fourth, certainly less than one-half, of these will take a course in economics.

tiveness of recent AEA-sponsored steps to improve economic understanding. The major sections are: I. Recent Steps to Improve Economic Understanding; II. What Economics Do Students and Teachers Know?; III. Current Materials and Teaching Practices; IV. The Teachers; V. Who Watched "The American Economy"?; VI. Effectiveness of "The American Economy"; and VII. Some Implications. For those who want a quick overview, Tables 1 and 6, plus the concluding "Implications," may be helpful.

I. *Recent Steps to Improve Economic Understanding*

In 1959 the AEA Committee on Economic Education appointed a 13-man Textbook Study Committee to analyze the economic content of the textbooks being used in high school social studies courses (in economics, problems of democracy, and American history). Paul Olson served as the chairman. This committee reported to the profession in "Economics in the Schools," published as a supplement to the *American Economic Review* in 1963 [11].

In 1960 the Association appointed a National Task Force on Economic Education, an independent group of well-known economists, to describe for high school administrators and teachers a minimum core of economic understanding fundamental to good citizenship and reasonably attainable by most high school students. The need for such a statement from the profession had been widely voiced by school teachers and administrators, school boards, and leading citizens. The report of the Task Force, *Economic Education in the Schools*, was published in 1961 [12]. Some 250,000 copies of this report have been distributed in full or in summary form to laymen and to secondary school administrators and teachers throughout the United States.[2]

In 1961 the AEA agreed to serve as co-sponsor of a new year-long national television course on economics, called "The American Economy," and offered in "The College of the Air" series carried by 182 CBS stations and virtually all the educational television stations in the United States during 1962-63 (and rebroadcast by many in 1963-64).

[2] Members of the Task Force were G. L. Bach, Chairman, Lester Chandler, R. A. Gordon, Ben Lewis, and Paul Samuelson from the profession; Arno Bellack and M. L. Frankel from secondary education; and Floyd Bond, Executive Secretary. The report is available from the Committee for Economic Development, 711 Fifth Ave., New York 22, N.Y., which provided funds to finance the Task Force's work and published the report, with no control over, or responsibility for, the conclusions reached. The CED also played a leading role in stimulation action on "The American Economy" and the "Test of Economic Understanding" described below, and in securing funds to finance these projects, but in each case with no control over the project itself or the findings of the economists and other professionals involved. The CED's policy statement, *Economic Literacy for Americans* [13], provides a summary of its role, and its own position on some of the substantive issues involved.

Professor John R. Coleman of Carnegie Institute of Technology was the national teacher; some 40 leading economists participated as guest lecturers; and the members of the National Task Force, at the request of the AEA, served as a policy and advisory committee in planning the series. The course was aimed to help the general public, students, and especially school teachers with inadequate backgrounds in economics. It was based substantially on the report of the National Task Force as to content and approach.[8] "The American Economy" had an average daily audience of over one million viewers (according to a standard Neilsen survey and reports from the participating educational television stations), including some 15,000 high school teachers and about 5,200 students and teachers who registered for college credit at one of the 361 cooperating colleges and universities. The cost of the program was approximately $1.5 million, which was provided by The Ford Foundation and a large number of leading business firms; in addition CBS contributed free air time. This course, and the AEA sponsorship of it with the Joint Council on Economic Education and Learning Resources Institute (the producer), followed the general patterns successfully used by the professional associations in physics, chemistry, and mathematics in preceding years.

Following completion of "The American Economy," the National Task Force and the Learning Resources Institute commissioned the National Opinion Research Center, affiliated with the University of Chicago, to conduct a major national study of what economics is being taught in the high schools and by whom, and to analyze the effectiveness of "The American Economy" in raising the level of economic understanding among high school social studies teachers, one prime target of the program. The study was outlined by the National Task Force and conducted by NORC on the basis of a national stratified cluster sample. A cross section of high schools in the United States was selected. Teachers in those schools were questioned and they took a special test on economic understanding. The 2,791 actual questionnaire responses were then weighted to represent 4,677 high school social studies teachers, out of an estimated total of approximately 60,000-65,000 such teachers in the nation. The teachers thus surveyed completed detailed questionnaires on their backgrounds, their teaching practices and attitudes, and the content of their courses. In addition, each teacher took a 25-question version of the nationally standardized "Test of Economic Understanding" described below. All questionnaires and tests were administered by NORC personnel. The results of this study

[8] A course outline, including the names of participating economists, was reported in the *AER* [10] and is available in more detail in the special guide prepared for the course [4]. Professor Coleman has also presented his philosophy of the course in [3].

provide the first detailed picture, based on a scientifically designed sample of all high school social studies teachers, of who the teachers of economics are in the high schools, what their backgrounds are, what they are now teaching, and how much they learned from "The American Economy."

Simultaneously, in 1962-64, a new standardized high school level "Test of Economic Understanding" was developed by a related group of leading economists, educational psychologists, and high school educators to help school administrators and teachers evaluate how much economics their students know and are learning, using a large national norm group for comparison. This step, financed by the CED through the Joint Council on Economic Education, but entirely under the control of the experts involved, was an attempt to meet the widespread demand for such a test that would be professionally competent and free from the suspicion of bias often leveled against tests devised by representatives of business or other economic groups.[4] The test committee, under the chairmanship of Dr. John Stalnaker, President of the National Merit Scholarship Corporation, spent approximately a year devising a "Test of Economic Understanding" for use with high school students at the 9th to 12th grade level, using the report of the National Task Force on Economic Education as a rough guide as to the concepts and areas to be tested. The test was designed for students with or without separate courses in economics, and dual forms were prepared so the test could be given to students on a before-and-after basis. Because the test was designed for use by thousands, even hundreds of thousands, of students in widely varying schools and areas, the use of "objective" type questions was mandatory. All questions are multiple choice, with four alternative answers given to each.

Questions cover the basic areas of micro- and macroeconomics, plus some questions on "applied" fields such as international economics and comparative systems, but they omit all technical detail beyond such simple concepts as supply and demand. There are a few factual questions, but most are focused on the understanding of basic "concepts" and ability to handle "problem" or "application" situations. A few questions call for analysis of major policy issues (such as monetary and fiscal policy), and three require reading and interpreting graphs of economic time series.[5] Roughly, one-third of each test covers micro-

[4] The economists on this committee were G. L. Bach (Carnegie Tech), E. O. Edwards (Rice), J. A. Kershaw (Williams), Ben Lewis (Oberlin), and Lewis Wagner (University of Iowa).

[5] Typical questions are:
Factual:

1. In large business corporations common stockholders generally do not:
   a. own the business

economics, one-third macroeconomics, and one-third special topics and applications, such as the farm problem, international trade, comparative economic systems, and the like. Each question was pretested on thousands of students, many were extensively revised, and the test was carefully balanced for coverage, concepts involved, degree of difficulty, and kinds of understanding—all within the practical limits imposed by the need for a mass-testing instrument. The final test is extremely elementary. Every economist would probably devise a somewhat different one for the purpose; the constraints imposed by test experts and the realities of mass use in high schools were considerable.

Beyond these major recent moves, culminating in the AEA policy statement in June, 1964 [9], many other groups have worked steadily to improve economics in the schools. The nonpartisan Joint Council on Economic Education has perhaps worked most closely with professional economists, and the AEA has long nominated economists to be members of the JCEE Board of Trustees. Important though these efforts may have been, however, it is impossible to detail them here.

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b. receive a share of the profits
c. vote for the board of directors
d. manage the day-to-day business

Concept Understanding:

1. Under a private enterprise economy the function of competition is to:
   a. eliminate wasteful advertising
   b. eliminate interest and profits
   c. prevent large firms from driving small ones out of business
   d. force prices to the lowest level consistent with a reasonable profit
2. A rise in the price of which product would be likely to increase the demand for butter?
   a. butter
   b. oleomargarine
   c. bread
   d. any of the above

Problem Analysis:

1. If, when there is full employment, the federal government increases its spending without increasing its tax revenues, generally:
   a. a serious depression will occur
   b. an increase in unemployment will occur
   c. the national debt will decrease
   d. inflation will occur
2. In a basically private enterprise economy, which tax is likely to alter most the pattern of consumer choices among alternative products?
   a. a general sales tax
   b. a personal income tax
   c. an excise tax on particular products
   d. a tax on business profits

Copies of the full questionnaire are available from Science Research Associates, 259 East Erie Street, Chicago, Illinois, which handles distribution of the questionnaire. SRA also has complete information on norm groups, plus an item analysis of all questions included.

II. *What Economics Do High School Students and Teachers Know?*

How much economics do high school students and their teachers know? And how does their economic understanding compare with that of other groups, such as college students?

Table 1 provides a summary picture, using test scores on the very elementary "Test of Economic Understanding" described above. While this test provides only a very limited measure of economic understand-

TABLE 1—LEVEL OF ECONOMIC UNDERSTANDING OF HIGH SCHOOL SENIORS AND OTHERS[a]

	Test Scores	Standard Deviation	Significance of Difference from Preceding Group[b]
High school seniors:[c]			
No econ. courses (*n*=4601)	24.2	6.67	
One econ. course (*n*=1834)	29.7	8.19	.001
High school social studies teachers:[d]			
No college econ. courses (*n*=717)	32.0	8.48	
1–2 college econ. courses (*n*=1859)	32.8	8.42	N.S.
3–4 college econ. courses (*n*=1132)	35.5	8.94	.001
5+ college econ. courses (*n*=931)	37.2	8.48	.001
Watched "The American Economy" 3 or more times a week (*n*=110)	41.2	8.40	.001
College sophomores after econ. course:[e] (*n*=167)	40.7	4.20	
Industrial employees and managers:[f]			
Foremen and 1st line supervisors (*n*=319)	34.2	8.61	
Middle management (*n*=313)	36.3	8.13	.001
Staff: engineers, accountants, etc. (*n*=96)	36.6	7.11	N.S.
Top management (*n*=9)	42.7	8.4	.001

[a] All test scores are on the standardized 50-item, objective "Test of Economic Understanding," except test scores for teachers are converted from a special 25-item form of the test.

[b] Differences significant at .001 level where indicated. Differences marked N.S. were not significant at the .05 level.

[c] Data from Science Research Associates on total national "norm group" for the "Test of Economic Understanding." Widely repersentative sample of about 6,500 students.

[d] Data from NORC national stratified cluster sample of high school social studies teachers, described in text. Score adjusted to statistically comparable 50-item basis from 25-item test used by NORC.

[e] Data from studies of Carnegie Tech and University of Nebraska students, reported by Phillip Saunders [8] and by C. R. McConnell and J. R. Felton [6], respectively. Standard deviation is for Carnegie Tech students only.

[f] Data on special group of "industrial employees and managers" in 14 large, national companies tested by SRA for comparison purposes. Not necessarily typical of all companies. Data on top management shown merely for benchmark purposes.

ing, and while a more discriminating regression analysis controlling for such related variables as previous courses in economics is presented in Section VI, the basic scores are interesting, even striking for some categories.

The top two lines show the mean scores of some 6,500 high school seniors, divided between those who had taken no course in economics and those who had completed a one-semester course. The range of scores was from 8 to 48 on the 50-item test. While the students were not a scientifically drawn sample of all high school seniors, they were chosen to form a representative national "norm group" for users of the "Test of Economic Understanding." They comprise a roughly representative cross section of high school seniors, from large and small schools and cities, different sections of the country, and varying income areas.

As Table 1 indicates, even without a course in economics, high school seniors did roughly twice as well as the 12.5 score one would expect by chance, and taking a separate course in economics added 5.5 to the mean score of 24.2. This difference, which represents over a 20 per cent improvement, is statistically significant far above the .001 level, but it must be remembered that other variables (for example the possibility that brighter students tend to take separate courses in economics and the fact that economics courses are primarily offered in the "better" schools) may account for some of the difference. Even allowing for such special factors, however, it seems clear that a high school course in economics significantly increases students' ability to answer questions like those included on the "test of Economic Understanding."[6]

The next five lines show the test scores of the large NORC sample of high school social studies teachers. About 30 per cent of these teachers teach significant amounts of "economics" in separate courses or in courses in "problems of democracy"; the others teach primarily history, civics, and other social studies courses.

The mean score for all teachers was 34.1, with a range from 6 to 50 on a 50-item test basis.[7] But this composite hides interesting differences. For example, high school social studies teachers who have never taken a separate course in economics (about one-sixth of the total) achieved a mean score of slightly over 32 out of 50. This was significantly above the score of high school seniors with no economics

[6] For the results of a promising experiment with "programmed" teaching for high school students, see R. Attiyeh and K. Lumsden [1].

[7] Although actually a shorter 25-item form of the test was used, this form was item-analyzed and was found to give virtually identical results with the longer version when the 25-item scores were converted to the 50-item basis.

but less than three questions better than high school seniors after they had taken a single course in economics.

Teachers with one or two previous one-semester college courses in economics (about 40 per cent of all social studies teachers) scored slightly higher, but, strikingly, the difference was not statistically significant. More than two college economics courses raised the test scores in the following groups. But five or more college courses were required to raise the teachers' mean score by five points, the amount one recent high school course raised the high school students' scores from a lower base. Those teachers who regularly watched the national television course, "The American Economy," three times or more a week averaged 41.2, considerably higher than teachers who had had five or more previous economics courses. But most of the regular watchers had also had three or more previous college courses in economics. One must remember also that the national television course was fresher in their minds (completed about nine months before the test was given), and other variables may have significantly affected the test scores. Section VI reports the results of a multiple regression analysis to isolate the impact of the TV course on test scores, holding other presumably relevant factors constant.

The bottom sections of Table 1 show the scores of Carnegie Tech and University of Nebraska sophomores who had just completed a regular course in economics or the national TV course, as reported by McConnell-Felton [6] and Saunders [8]. The mean score of approximately 41 was virtually identical for students in both schools, and for those who took a regular lecture-discussion course or the national TV course. Last, data are shown for samples of industrial supervisors, staff workers, and managers. The sample, drawn from 14 large companies, is not necessarily representative of all industrial firms, but it appears to be a roughly representative cross section of larger, reasonably well-known companies. The 42.7 mean score for top managers rests on a number of responses far too small to be reliable, but it is included for its casual interest. The other scores rest on respectable samples. Again, other variables would need to be analyzed to ascertain the precise significance of the scores, but they do present a crude basis for comparison with the scores for other groups shown.

Beyond these summary measures, detailed analyses of student and teacher performance on individual questions are available. By and large, students without economics training in high school missed questions indiscriminately, with no clear pattern as between subject matter areas or factual versus concept versus problem and application questions. Questions on monetary policy and operations, international economics, and comparisons between the Soviet and the U.S. economic

system were heavily missed, although teachers report (see below) that they consider comparative systems the most important area in economics to teach. Students generally got the central notions of consumer sovereignty and the role of competition in our system, and did surprisingly well on simple supply, demand, and price questions even without formal economics training.

There was no clear pattern in the improvements induced by studying a course in economics in high school. Scores improved on all types of questions, but especially on factual questions involving general magnitudes, on the comparison between the U.S. and the Soviet economies, and somewhat more on micro questions than on macro. The questions still missed after a course in economics tended to be the "harder" ones by analytical standards. But this was not a clear pattern. Even after a course in economics, nearly the same proportion still missed a simple question asking who is hurt most by inflation (farmers, debtors, government bond holders, or businessmen), which was missed by 76 per cent of the students. The balance of payments and monetary policy continued to be enigmas to a large share of the students.[8]

High school teachers, like their students, did better on micro than on macro questions, and did better on factual than on analytical questions. Beyond these general observations, no clear pattern emerged from the item analysis. As with the students, teachers having formal courses in economics showed general improvement, especially on "concept" and analytical questions. But the pattern of improvement was not a clear one.

III. *Current Courses and Teaching Practices*

A. *Courses and Textbooks*

Of the 12 million students in high school this year in the United States, somewhere between 10 and 20 per cent will take a separate course in economics before graduation. No completely reliable data exist on the figure, but three independent estimates based on large samples all fall within this range.[9] Put the other way round, 80 to 90

[8] Tables showing a complete item analysis of the 100 questions on the two test forms are available on request.

[9] A U.S. Office of Education total count for 1961-62 shows about 290,000 students registered in separate courses of economics during the year, of a total of about 2 million seniors. About 220,000 were in required courses, the balance in electives. But this questionnaire information leaves a substantial question as to just what is reported as a course in "economics." For example, "consumer economics" is probably included by many schools.

The Joint Council on Economic Education obtained substantially complete information on courses in economics in the 130 largest school systems in the United States and estimated on this sample (with adjustments for smaller systems based on uncertain evidence) that nearly 20 per cent of all seniors are taking a separate course in economics. Our estimate, making use of the NORC and other data, is more like 15 per cent.

per cent will graduate without having any formal instruction in economics per se.

In the 130 largest school systems in the country, approximately nine out of ten offer a separate course in economics of at least one semester, and over a quarter (including New York City) *require* a one-semester course in economics of all graduates. Nationwide, however, only about 40 per cent of all public high schools offer a separate course in economics, indicating that such courses are rare in the smaller schools outside major metropolitan areas. But there seems to be a clear trend in the direction of more required work in economics, in separate economics courses, and in other courses like problems of democracy and civics. Pennsylvania, for example, has recently mandated a requirement of 36 class hours of economics for every high school graduate in the state. About four-fifths of the 130 largest school systems reported curriculum revisions within the last three years increasing the amount of economics taught in high school, and the overwhelming majority of the teachers in the NORC study also reported plans to increase the emphasis on economics. Detailed information is presented below on the content of such courses and their success in raising the level of student understanding.

Beyond separate courses called "economics," apparently 15-20 per cent of all high school seniors take a one-semester or one-year course in "problems of democracy," in which there is usually at least one large separate unit on "economics" or some economic problem such as social security or natural resources. Few of these go beyond description of institutions and information on government legislation to deal with the problems faced. Nearly all states have a mandated course in "civics" or American government; many of these courses have units in "economics," mainly of a descriptive-institutional nature. On his way through high school, virtually every student must take a course in American history, and many schools claim that some economics is taught in American history courses. However, there is little evidence to back up this claim, unless one counts the fact that the student hears something about the growth of economic institutions such as business firms and the westward movement of the frontier. Most of the history books mention bimetallism, the Granger Acts, tariff legislation, and the like, as well as the development of agriculture and industry as part of the historical sweep which the student traverses, but there is little that the economist would recognize as economics. Lastly, many students in "business education" programs take a required course in "economics." Generally, this is heavily weighted with elementary personal finance, bookkeeping, office practice, and the like, although some broader courses are appearing.

Extensive field studies, textbook analyses, and reports of teachers generally confirm the picture of high school economics teaching. In a few schools, particularly in big cities and upper-class suburban areas, some very good high school economics is apparently being taught. But even in courses called "economics," the coverage is generally descriptive and nonanalytical. Much space is given to economic institutions, descriptions of natural resources, laws, governmental regulations, and the like. In the last few years, some texts have introduced some attention to problems of aggregate economics. Sometimes elementary economic concepts are introduced (for example, supply, demand, and price), but these are seldom used in application to the problems considered in the following chapters. At least to us, most of the texts seem taxonomical and probably dull for typical high school youngsters. As the AEA's Textbook Study Committee wrote: "Perhaps the most alarming characteristic of textbooks in all three courses [economics, social studies, and U.S. history] is the dominance of description over analysis in the treatment of those economic topics selected for discussion" [11, p. x].

B. *Areas of Economics Taught in High Schools*

High school teachers in the NORC survey described above were asked to select from a list of 11 subject matter areas in economics those which they considered to be "very important" to teach and those to which they actually devote at least six classroom sessions each year. Table 2 shows the results separately for those teaching courses in economics or problems of democracy (about 30 per cent of the total sample) and for all others teaching some economic topics, usually in civics, history, or general social studies courses. In reading Table 2 it should be remembered that, since most high school students do not take a course in economics, the attitudes and practices of teachers outside economics and problems of democracy courses may be more important on these issues than those of instructors teaching separate courses in economics.

Comparative economic systems (capitalism, communism, etc.) is considered far and away the most important area of economics to teach by both groups. About 90 per cent of all teachers considered it "very important," and about half report spending six or more periods on the subject.[10] On the other hand, "the role of markets, prices, and

[10] These high figures for comparative systems may reflect partly a tendency to put into this category ("capitalism, socialism, and communism") general treatments of the U.S. economy ("capitalism") which don't fit into any of the other categories, even when little attention is paid to other systems. However, a substantial portion of the states now *require* a course, or some minimum number of days, on comparative political, economic, and social systems, often with specific mention of communism and the USSR.

TABLE 2—AREAS OF ECONOMICS EMPHASIZED BY HIGH SCHOOL TEACHERS*

Area	Rated "Very Important" by Teachers in: (per cent)		Plan to Devote at Least Six Classes to: (per cent)	
	Econ. & P.O.D.**	All Others	Econ. & P.O.D.**	All Others
1. Comparative systems (capitalism, communism, etc.)	91	87	53	47
2. Government finance, taxes, etc.	75	68	37	26
3. Labor unions, distribution of incomes	75	67	44	30
4. Development of economic institutions	70	64	41	31
5. Role of markets, prices, and profits	70	55	38	20
6. Booms, depressions, inflation, etc.	67	69	37	24
7. Government regulation of business	67	65	21	20
8. Consumer economics, personal finance	58	58	37	24
9. Money and banking	55	49	36	20
10. International economic problems	46	45	33	32
11. Underdeveloped economies	41	42	29	33

* Data from NORC. Table includes only teachers who reported that they teach something about economics, about 80 per cent of the total sample.

** Problems of Democracy.

profits" and "booms, depressions, and inflation" ranked well down the list among most social studies teachers, but, encouragingly, higher among those teaching economics per se. Interestingly, the distribution of income (labor unions, wages, social security, etc.) rated high among both groups, in contrast to what appears to be a tendency in the profession at the university level to play down this area in elementary courses. University economists will be interested in the place given the topic, "consumer economics, personal finance, etc." Although it ranked well down the list for both groups, it was still considered "very important" by more teachers than were money, international economics, and the underdeveloped economies.

C. *Teaching Approaches and Attitudes*

All teachers who cover any economics in their courses were presented three concrete topics (labor unions, the farm problem, and booms and depressions) and alternative ways of approaching each. They were asked to indicate which of three approaches they predominately use in teaching about each. One alternative stressed the historical approach, another the descriptive-institutional, and the third the development of economic concepts and their use in analysis of the situation. After this question was answered, teachers were asked specifically which of the three approaches they generally use the most.

TABLE 3—TEACHING APPROACHES*

Approach	Economics and P.O.D. Teachers	All Other Teachers
Descriptive-institutional	6.3	6.3
Concept-analytical	5.8	5.4
Historical	4.8	5.2

* Based on NORC study. Each index is based on responses of that group on the teaching approach to each of the three problems presented, where 10 would represent use of that approach by all teachers on all three issues.

Not surprisingly, most teachers reported some use of all three approaches. Table 3 shows the relative stress placed on each of the three by teachers of economics and P.O.D., and by all others who teach anything about economics in their courses. The index number reported for each approach represents a weighted average of the number of first, second, and third choices each approach received relative to the others, where 10 would represent use of that approach by all teachers on all three topics.

As might be expected, the descriptive-institutional approach leads. More surprising is how closely the concept-analytical approach followed. But a warning is in order. All questionnaires were completely anonymous, and the questions on the treatment of labor unions, the farm problem, and economic growth and fluctuations carefully avoided such colored terms as "analytical" and "descriptive," so there should be little bias. But use of the "concept-analytical" approach in most cases implies only development of the simplest of economic concepts and their use in only the most elementary way. Other evidence, e.g., the AEA's textbook study quoted above, suggests little use of what economists would call "analytical" approaches to economic issues, no matter what the teachers replied on this part of the NORC study.

IV. *The Teachers*

Most of the economics taught in the high schools is offered in courses in "economics" or "problems of democracy." Table 4 presents information on the teachers of these courses and compares them with all other social studies teachers.

The table indicates that nearly all economics and P.O.D. teachers have had at least one college course in economics and that 58 per cent have had three or more.[11] A quarter of all economics and P.O.D. teachers have had five or more courses in economics, and 4 per cent

[11] It will be remembered from Table 1 that there was no significant difference on economics test scores for teachers with zero or one-two college courses in economics.

TABLE 4—HIGH SCHOOL TEACHERS OF ECONOMICS AND PROBLEMS OF DEMOCRACY[a]

	Economics and P.O.D. Teachers	All Other Social Studies Teachers
	(per cent)	
1. Number of college economics courses:		
None	10	18
1–2	32	43
3–4	32	21
5 or more	26	18
2. Major in:[b]		
Economics	6	3
Other social science (inc. history)	84	79
Other	38	37
3. Rank in college class:		
Top 10 per cent	26	18
Top quarter, not top 10 per cent	33	37
Second quarter	29	33
Bottom half	12	12
4. When took last economics course:		
Last year	11	5
2–5 years ago	32	30
5–10 years ago	22	30
More than 10 years ago	35	35
5. Score on "Test of Economic Understanding"[c]	36.9	32.9

[a] Data from NORC survey.
[b] Totals more than 100 per cent because some teachers reported more than one major, including both undergraduate and graduate levels.
[c] Mean score on SRA test. For comparative data on other groups, see Table 1.

were majors in economics. Other social studies teachers have had considerably less economics.

High school social studies teachers as a group came from the top half of their college classes, only 12 per cent from the bottom half (though about two-thirds of the teachers obtained their grades through majors in "education"). Economics and P.O.D. teachers stood somewhat higher in their college classes than did other social studies teachers—26 per cent were in the top 10 per cent of their college classes. And over two-fifths of them were reasonably up to date, if recency of completing the last course in economics is a measure. Conversely, as indicated by item 4, nearly 60 per cent have not had a course in economics in the last five years, and a third not in the last ten years. The last line of the table shows that economics and P.O.D. teachers as a group did significantly better on the "Test of Economic Understanding" than did all other social studies teachers, as could be expected.

However, this comparison includes many other variables which influence test scores; a more thorough analysis, using a multiple regression technique, is provided in Section VI.

The NORC study also provides a large amount of additional information on high school social studies teachers. For example, 80 per cent of all such teachers were men. About one-third were under 30 years of age, another third between 30 and 44, and the rest older; the median age was 33.5. Interestingly, the proportion of these reporting no course in college economics, or only one to two such courses, was appreciably higher in the youngest age group than in the middle age groups. This suggests that the portion of potential social studies teachers taking courses in economics is lower now than it has been in the past, although it also reflects the fact that some teachers take economics courses toward advanced degrees after they begin teaching. About 40 per cent of all social studies teachers have earned some degree beyond the Bachelor's, and about 40 per cent are currently working toward some academic degree. Eleven per cent of all social studies teachers reported their college major was physical education.

Apparently about one-tenth of all social studies teachers are hired new each year. About a third have been teaching less than five years, while 43 per cent have been teaching at least 10 years. About 70 per cent report that all of their teaching is in the area of the social studies, with history the dominant area. About two-thirds taught at least one course in history, while about 13 per cent reported teaching a separate course in economics or economic institutions.

The median annual income of all social studies teachers in 1962-63 was $6,150. About 25 per cent reported incomes under $5,000 and 21 per cent reported $7,500 or over. Their family backgrounds, as measured by father's occupation, conformed closely to the composition of the general population, except that more social studies teachers came from professional and fewer from farm families than in the general population. About 20 per cent of all social studies teachers reported reading the *New York Times* regularly; 5 per cent added the *Wall Street Journal*. Two per cent admitted to reading the *American Economic Review* regularly, while about 10-15 per cent reported regular reading of *Social Education*, *Social Studies*, or similar publications.

V. *Who Watched "The American Economy?"*

We turn now to an evaluation of the success of "The American Economy," the nationwide television course sponsored by the AEA in 1962-63. It was the most widely watched educational television course in history. Its total audience, averaging over one million persons daily, was apparently about twice as large as the highest previous audience

for a comparable national TV course, which was the course on probability and statistics broadcast in the preceding year. The some 5,200 viewers enrolled for credit at participating colleges was also the largest on record. Over 45,000 TV study guides for "The American Economy" were sold, and the National Educational Television Center reports that the sound films made from the TV tapes are the most widely used of any educational TV series ever produced—3,346 rentals and 531 sales of films in the course had been made as of November 30, 1964.

About 20 per cent of the 65,000 high school social studies teachers in the country watched the program at one time or another in 1961-63 or 1963-64. The NORC national sample survey, however, indicates that only about 5 per cent watched the program at least once a week throughout the 1962-63 year. A separate survey conducted by the National Association of Secondary School Principals of five large states (California, Connecticut, Illinois, Minnesota, and New York) indicated that approximately 15 per cent of the social studies and business education teachers in those states were watching the series "on a regular basis." Thus, it is clear that a substantial proportion of all social studies and business education teachers watched at least some of "The American Economy," but it seems probable that not more than 5-10 per cent of them (perhaps 3,000-6,000) were serious, regular viewers.

Some 245 colleges and universities offering a credit course based on "The American Economy" reported 5,200 students signed up for credit as of March, 1963. A subsequent postcard survey by the authors of this article indicates that approximately 85 per cent of these people (some 4,400) successfully completed the course for college credit. Of those completing the course, slightly over 40 per cent (some 1,800) were reported as school teachers. The remainder were regular undergraduate students taking "The American Economy" as their introductory economics course or other persons taking the course for credit.

Other surveys by the National Association of Secondary School Principals (nationwide) and the Committee for Economic Development (New Jersey), together with the NORC data, indicate that certainly over 1,000, and perhaps as many as 1,500, high school social studies and business education teachers successfully completed "The American Economy" for college credit.

A. *Teachers Who Watched*

What do we know about the high school social studies teachers who watched "The American Economy" regularly? Table 5 summarizes the answer, and compares these watchers with high school social studies teachers who were not regular viewers.

Perhaps the most striking finding is that two-thirds of all the high

school social studies teachers who watched "The American Economy" regularly at least once a week had previously had three or more courses in economics. Sixty-two per cent reported at least one graduate-level course in economics, and 44 per cent reported two or more such graduate courses (sometimes Schools of Education give graduate education credit for elementary work in economics when supplemented by advanced work in teaching methods). Only 14 per cent reported no course in economics. On the other hand, only 4 per cent reported that economics was the major field of their last academic degree. Item 3 confirms the related fact that the watchers were mainly people who were actively concerned with economics; more than half of all regular viewers were currently teaching a course in economics or problems of democracy.

This finding that half or more of the regular viewers had already had a substantial amount of economics accords with previous experi-

TABLE 5—HIGH SCHOOL SOCIAL STUDIES TEACHERS—VIEWERS AND NONVIEWERS[a]

	Regular Viewers[b]	All Other H.S.S.S. Teachers
	(per cent in each category)	
1. Previous economics training:		
No economics courses	14	16
3 or more economics courses	67	42
Economics major for last degree	4	2.5
2. Advanced degree (beyond A.B.)	61	37
3. Teach a course in economics or P.O.D.	53	13
4. Standing in college graduating class		
Upper 10 per cent	27	20
Upper 25 per cent	65	56
5. Degree of professional activity[c]	14.4	10.1
6. Median age	43.8	33.4
7. Median years teaching	14.3	8.3
8. Sex (per cent male)	73	81

[a] Based on NORC sample. Per cent in each case shows percentage of all teachers specified by the column heading, except for items 5–8, which are actual numbers.

[b] Those who reported watching the program regularly once or more weekly throughout the 1962–63 year.

[c] Weighted index of four measures of professional activity, including (a) the number of professional organizations to which the teacher belonged; (b) the number of times he has held office in a professional organization; (c) the number of professional and academic meetings attended over the past year; and (d) the number of professional and technical periodicals which the teacher reads regularly. Performance on each measure was coded 0–6, and possible scores on the index run from 0 to 24.

ence with educational television and related mass media. Previous studies have found that the role of such educational media is more one of reinforcing and supporting existing attitudes and interests than in developing new ones. People select from their environment stimuli that are meaningful to them in terms of previous experiences. Furthermore, recent studies in the field of adult education and of audiences for educational television in other fields show that those who participate in such programs and watch educational TV are more likely to be those who start with higher educational levels [5, pp. 80 and 136], [7, p. 57].

Lines 4 and 5 of Table 5 suggest that regular watchers ranked somewhat higher than other high school social studies teachers in academic standing, and that, as might be expected, they were generally more active in professional activities. Regular viewers were older and more experienced than were other teachers, and women comprised a substantially higher proportion than of all social studies teachers.

VI. *Effectiveness of "The American Economy"*

A. *Economic Understanding*

How effective was "The American Economy" in adding to the economic understanding of its viewers? The NORC study asked all of the social studies teachers who viewed the program at all whether it "added a great deal," "added somewhat," "added a little," or "didn't add anything." About 40 per cent of regular viewers reported that the program added a great deal to their understanding, and another 45 per cent that it added somewhat. Conversely, only 15 per cent considered that the program added little or nothing. Somewhat surprisingly, regular watchers who had already had three or more courses in economics felt that the program added just as much as did those who had had fewer or no courses at all in economics.

Such general reactions, however, are suspect as evidence of the actual learning that occurred from watching. As part of the NORC study, therefore, each social studies teacher in the sample was given a shortened, 25-question version of the "Test of Economic Understanding" described above. Performance on this test may serve as a rough measure of the economic understanding of each teacher. Therefore a multiple regression analysis was run to isolate the relative importance of watching the TV course and of some eight other variables in explaining performance on the test.[12] The major variables used in the

[12] As was explained above, scores on the 25-item test were converted to a statistically identical 50-item basis by multiplying by 2, to maintain comparability with the other scores shown in Table 1.

analysis were: watching the TV course; taking it for credit; previous training in college economics; teacher's standing in college graduating class; whether or not respondent teaches a separate course in economics or P.O.D.; teacher's professional motivation; and the personal characteristics of age and sex. The regression equation was of the usual linear form, $Y = a + bX_1 + bX_2 \ldots bX_{14} + u$, where u is a random disturbance term assumed to have the usual simplifying properties.

Table 6 presents the results of the regression analysis. To provide extra information several of the main independent variables were subdivided into additive subvariables. For example, regular watchers of "The American Economy" were divided into "one or more times a week" and "three or more times a week," where all of the second group is included in the first. Thus, the coefficient in column 1 of Table 6 for "watched one or more times a week" is to be interpreted in the usual fashion as the effect of this variable, holding all others constant. The coefficient for "watched 3 or more times a week" also shows the effect of this variable, holding all others constant; thus it shows the *marginal* effect of watching 3 or more times over 1-2 times a week. To obtain the full impact of watching 3 or more times a week, we must add the two coefficients (.64 + 7.24), which gives us 7.88. Since this marginal analysis is applied for all of the first four major variables, column 2 has been added to show the full (summed) effect of the final subvariable in each group.

Table 6 indicates that watching "The American Economy" regularly three or more times weekly was far and away the most important variable in raising teachers' performance on the test of economic understanding. Its coefficient of 7.88 was more than twice as large as that of about 3.6 for having taken five or more college courses in economics or for graduating in the top 10 per cent of one's college class (the best proxy we had for intelligence, though one which also includes other factors such as motivation). It was much more powerful than taking one or more, or even three or more, college courses in economics. No other variable approached these in positive explanatory power.

The R^2 for the multiple regression is .152. For economists used to working with time series, this will seem extremely low. However, it is roughly in line with the R^2's obtained in many other cross-section studies, for example of consumption behavior, and the F-test shows it is significant beyond the .001 level. The low R^2 may occur because major variables have been completely omitted—although it is hard to see what they might be. More likely, it is because of the large amount of random noise in such a large sample, and because the proxies used

TABLE 6—MULTIPLE REGRESSION ANALYSIS OF RELATIVE INFLUENCE OF SELECTED VARIABLES ON TEACHERS' TEST SCORES[a]
$R_2 = .152$

Explanatory Variable	Marginal Subgroup Coefficients[b]	Coefficients[b]
Watched "The American Economy"		
X_1 = one or more times a week	.64	
X_2 = three or more times a week	7.24**	7.88
Took "The American Economy" for credit[c]		
X_3 = in-service credit or college credit	−8.90**	
X_4 = college credit only	3.10	−5.80
College economics training		
X_5 = one or more courses	.26	
X_6 = three or more courses	.88*	
X_7 = five or more courses	2.44**	3.58
College class standing		
X_8 = top 25 per cent	.12	
X_9 = top 10 per cent	3.48**	3.60
Teach high school economics or P.O.D.		2.36**
X_{10} = teach such a course		
Professional motivation		
X_{11} = activity in professional organization		−.12**
X_{12} = has or is working for advanced degree		1.16**
Personal characteristics		
X_{13} = sex (male)		2.22**
X_{14} = age		−.04**

[a] Based on NORC data for 3,966 teachers; some responses could not be used because of incomplete data. See text for description of test of economic understanding used.

[b] Using one-tailed t-test, * = significant at .01; ** = significant at .001. For difference between two columns, see text. All variables are dichotomous (0–1) except for age and for professional motivation (a continuous variable described in Table 5.)

[c] "In-service" credit is usually offered directly by high school system toward salary increases; amount of work varies widely. "College" credit is formal credit, usually involving some on-campus review in teaching sessions plus formal examinations.

for important explanatory variables (for example, for intelligence and motivation) are imperfect; we comment below especially on what we suspect are inadequate measures of motivation. Similarly, the equation weights any college economics course as the equivalent of any other. But the proxies all appear to be reasonably sensible, and at least to give a tentative answer as to the relative importance of watching "The American Economy" as against other obvious possibilities for explaining performance on the test used.

The large negative coefficient for taking the TV course "for credit"

is one major surprise of the data. One might suppose that taking the course for credit would reflect high motivation, in addition to providing some additional education through supplementary on-campus or in-service classes. But taking the TV course for "in-service credit" subtracted nearly 9 points on the test score, holding all the other variables constant. Those taking the course for college credit did 3.10 points better than "in-service" credit-takers, but this difference was not statistically significant and, as indicated in column 2 of Table 6, it still left them with a coefficient of -5.80.

This finding strongly suggests that the teachers taking "The American Economy" for credit differed markedly from other teachers on some other variables not included in our regression; otherwise one must conclude that the supplementary college teaching and examinations usually required for students taking the course for college credit actually confused the teachers and detracted from what they would have learned from just watching the TV course.

To investigate this unexpected result further, we examined directly all the cases of teachers taking the course for credit, only 42 individuals in the sample, of whom 23 were college-credit viewers and the others in-service credit viewers. This examination throws grave doubt on the superior motivation hypothesis for these watchers; about 25 per cent of the credit-takers reported watching less than three times a week. Indeed, it seems probable that many credit-takers may have wanted the credit more than the knowledge and did as little as possible to get by. A strong bimodal distribution of test scores among credit-takers supports this hypothesis. Several of the low scores came from the irregular watchers just noted, and the weighting system implied by the sampling procedure happened to give substantial weight to a few of these low test scores in producing the results shown. Clearly the "professional motivation" variables (X_{11} and X_{12}) are weak surrogates for the real motivational differences that may have existed. And one might reasonably hypothesize that a strong unmeasured motivation factor for regular viewers as a group is a major explanatory variable not picked up by the present regression equation.

There is additional evidence on the impact of the TV course, as measured by the same test. McConnell-Felton [6] and Saunders [8], using independent data in Pennsylvania and Nebraska, recently found that regular college students taking a regular year-long on-campus sophomore college course in economics scored about the same on the "Test of Economic Understanding" as did students taking "The American Economy" for college credit at the same institutions. McConnell-Felton reported, however, that on a more sophisticated test,

requiring more advanced technical tools from economics, regular college students taking a typical sophomore course outperformed the TV students by a significant margin. In further work, not yet published, Saunders reports a clear marginal improvement in the test performance of teachers who have taken the TV course and have supplemented this by on-campus classroom work with regular university instructors at some institutions. These results suggest the importance of high-quality instruction if classroom experience is to have any value in adding to a good TV presentation.[13]

B. *Residue from Previous College Economics Courses*

The coefficients for X_5 and X_6 in the regression equation indicate that one or two previous courses in college economics made no significant contribution to performance on the test of economic understanding, while three or four courses added very little to test performance. Five or more college courses added 3.58 to the test score, but even this was only about half the contribution made by watching "The American Economy" regularly during 1962-63.

This result may be interpreted by some as a devastating commentary on the effectiveness of our elementary economics courses or indeed on college economics as a whole. At least, it deserves further consideration.

First, on the average, teachers have been out of college about eight years, and the absence of any residue from college economics courses may merely reflect the well-known phenomenon of forgetting. Indeed, psychologists have shown that retention of most learned material is very short unless the material is deemed relevant by the learner and is used or otherwise reinforced periodically.[14] Perhaps students retain virtually nothing from their other college courses either. But this is slight consolation if we measure the value of our courses by what lasts after the student escapes the final exam. The fact that we *may* do as well as other disciplines is hardly a happy defense. Nor does the fact that economics is not "used" or "reinforced" provide much of a rationalization, since everyone in this sample is an active teacher in the social studies, of which economics is surely one important component. Indeed, over 20 per cent are currently teaching a course in economics or problems of democracy. As is indicated by the coefficient for X_{10}, this current involvement with economics helps appreciably to raise

[13] Preliminary findings from Saunders' further studies also show that this superior understanding of well-trained high school teachers is directly reflected in superior test performance of their high school students, as compared to control groups of students with other teachers.

[14] See, e.g., Carl Hovland on "Learning," in *Handbook of Experimental Psychology* [14].

those teachers' test scores, but not enough to change the general picture significantly. It would have been remarkable had a college economics course taken (on the average) several years ago showed as much effect as the recent comparable TV course, and some allowance for this fact is required in assessing the relative success of the TV experiment; the McConnell-Felton and Saunders studies cited above provide direct evidence on this point. But this does not alter the basic finding of *no* significant residue from even a year of college economics.

Second, the absence of a residue from basic college courses may reflect the fact that the test used is a bad measure of economic understanding or that it measures the wrong things. Readers are invited to review the sample questions above and form their own judgments. Certainly the test is extremely elementary, since it was constructed to test basic, though nontechnical, aspects of economic understanding. It certainly will not discriminate effectively among people who know a good deal of economics—though a look back at Table 1 shows that mean scores for all the relevant groups are well below 100 per cent accuracy. Moreover, since it was designed to avoid rewarding mere acquaintance with technical textbook terms, this test may not show as strong an advantage for formal education types of information and understanding as would some other tests.

To obtain a further evaluation of the reasonableness of the "Test of Economic Understanding" for this purpose, the department chairmen of 30 leading universities and about 30 other leading economists especially interested in basic economics were asked to take the test and to write a brief impressionistic evaluation of it for the purpose for which it was designed. About half did so. Every reply stated that the test seemed at least "satisfactory" for the purpose indicated; most stated that it seemed "good," "highly appropriate," or "very good," although a number expressed reservations about individual questions. But of course the test may be a "good" one for its primary purpose and still not be satisfactory for evaluating desired lasting effects of our college courses.

Third, perhaps the minor carry-forward from college courses may reflect the fact that these teachers took unusually "poor" or "weak" undergraduate courses, and that students who took better courses would have performed significantly better on the test given. Since respondents' forms indicate where they studied as undergraduates, we have underway a supplementary analysis of this possibility. Preliminary data indicate that of about 4,200 teachers for whom information is available, about 1,300 (somewhat less than one-third) attended "teachers colleges." About 900 attended a group of 120 top "prestige" or very well-known universities and liberal arts colleges. About 2,000

(just under half the total) attended other colleges and universities. W also have preliminary information on how many majored in "educ tion"; such majors accounted for about two-thirds of the tot sample.

Economists who mistrust the educational standards of departmen and schools of education may suspect that these data go far to expla the results reported above. Very preliminary analysis suggests that, l and large, noneducation majors did somewhat better on the test th their education counterparts in schools of comparable stature, and th high school teachers from the better-known schools did substantial better than those from other schools. But this, of course, may mere mirror differences in basic student abilities, and careful study holdi such other variables constant will be required to judge whether in fa different types and sizes of programs and institutions appear to achie significantly different lasting effects from their economics courses. W hope to report to the profession separately on this analysis in the ne future. But while this further analysis may show significant differenc in the success stories for different types of programs and institution the preliminary data suggest little reason to suppose it will change t basic picture presented here of generally low carry-forward from bas courses in college economics.

C. *Plans for Change and Teaching Approaches*

Teachers in the NORC sample also reported on plans to change t teaching time they will devote to different areas of economics. Nearl twice as many regular watchers of "The American Economy" (define as those who watched three or more times a week) reported plans t increase the time spent on half or more of these areas next year as di nonwatchers. For example, 33 per cent of all regular watchers plan t increase the amount of time spent on six or more of the 11 areas c economics listed in Table 2, as compared to only 15 per cent of th nonwatchers. It is not, of course, permissible to attribute th difference solely to watching "The American Economy." Watche may have been the ones who were inclined to put more time on ec nomics in any case. However, the results are consistent with the hop that "The American Economy" would stimulate more attention to ec nomics in high school social studies teaching.

Subject to the same reservation about causation, it is interestin that regular watchers show especially large increases in time spent o the core materials of macro- and microeconomics, compared to othe teachers. For example, 39 per cent of all regular viewers are spendin significantly more time on "economic stability and growth" than be fore the course, compared to only 22 per cent of other teachers. Simi

larly, 37 per cent of the regular watchers now spend significantly mor time on "the role of markets, prices, and profits," compared to 23 pe cent of other teachers. Roughly comparable results were reported o "the development of modern economic institutions." In other areas o economics, the differences between the plans of watchers and non watchers were much less marked, except that 33 per cent of the regula viewers reported plans to spend *less* time on consumer economics, com pared to much smaller changes for other teachers. These results ar consistent with the hope that "The American Economy" would develo more understanding of the central analytical core of economics and th way it can be used in thinking about economic problems.

Similarly, regular viewers reported a much greater emphasis on "ar alytic" (as contrasted to "descriptive" and "historical") teaching ap proaches than did occasional or nonviewers. On the three situatior given (see III C), 24 per cent of regular viewers chose the analytic approach in teaching on all three, as compared to only 9 per cent (the other teachers. And 43 per cent of regular viewers chose an analyt cal approach to two of the three situations, as compared to only 27 pe cent of the occasional or nonviewers. Again, the results are consistei with the hopes of "The American Economy" to stimulate a more an lytical approach to economic issues, although they certainly cannot b attributed solely to that course.

This evaluation of the effectiveness of "The American Economy does not include a large amount of *ad hoc* evidence reported by teac ers, school administrators, and others interested in economic educatio from around the United States. These reports, almost without excep tion, agree that "The American Economy" was a widely watche popular, effective TV program and course in economics. An abbrev ated 60-film condensation of the course has been widely used in "in-se vice" teacher training programs and is now being used as the founda tion for teacher development in 50 major school systems, under a ne program by the Joint Council on Economic Education to help impro economic teaching in the schools. Experiments are also under way fo the direct use of the films in high school courses. College teachers ar using selected films widely, and industrial firms are using parts or a of the course for in-company development of middle- and lower-ma agement people.

VII. *Some Implications*

In conclusion, we suggest the following as some implications of th findings.

1. If we want most of our future citizens to have any formal trai ing in economics, it must be given in the high schools, barring an eno

mous change in the national educational pattern beyond even the large increases in college enrollment currently expected. Thus, unless the profession wishes to wash its hands of responsibility for economic understanding of the citizenry, it must take a strong, active interest in the teaching of economics in the high schools.

2. It is possible to teach a substantial amount of economic understanding to average students in the high schools. Even with present inadequate high school courses in economics, students taking such courses showed large improvements in average test scores on the simple "Test of Economic Understanding." Still unpublished experiments in particular localities confirm this possibility, and indicate that with *well-trained* high school economics teachers or with effective "programmed learning" the improvement can be much more dramatic than shown in Table 1.

3. Better-trained high school teachers are critical in improving economic understanding provided by the schools. The small test margin of the mass of social studies teachers over average high school students who have had merely a weak one-semester course in economics is dramatic evidence on this point. Superintendents and other school administrators repeatedly stress the importance of improving the basic economic understanding of their social studies teachers if real improvement is to be made in their teaching. As indicated above, recent experiments confirm this strongly. Intensive work with competent, interested, and understanding university economists, followed up by in-service help, can dramatically improve the understanding of average high school teachers, their ability to teach effectively, and the performance of their students. However, merely taking more courses in economics or going through weakly taught summer institutes or in-service programs apparently does little good for high school teachers; quality of instruction and teaching materials appear to be crucial.

4. Unless the results reported above are grossly misleading, it is clear that present (or previous) college courses in economics don't do an effective job of preparing school teachers to teach economics, even recognizing the reservations indicated above. Whatever our students do on the final exam, the several-years-after test shows little residue, even for high school teachers for whom economic issues provide a part of their day-to-day teaching responsibilities. These findings emphasize again the well-known psychological principle that "learning" unsupported by motivation and reinforcement through repeated use or other means has a very short half-life. If our college courses don't develop student interest in economics for the years to come and if the analysis we teach isn't usable *and used* by students on their own after college,

there is little reason to expect much to last, however elegant the analysis or important the descriptive material in the course.

5. Since the average age of high school social studies teachers is only 33, and since about one-third of all teachers have been teaching less than five years and nearly two-thirds less than 10 years, improvement in the economic training provided in the colleges and universities could have a rapid impact on teaching in the high schools.

6. Improved textbooks and other teaching materials are critically needed as a foundation for improved teaching of economics in the schools. This includes not only materials for special courses in economics, but at least equally better materials for courses in problems of democracy, civics, American history, and the like. It is essential to remember that the great bulk of students get their exposure to economic issues in such courses. The economic preparation of the teachers in such courses is particularly weak, and such teachers badly need the best teaching materials.

7. If we want to get more economic analysis and points of view into history, problems of democracy, and civics courses in the schools, growing experience suggests that such teaching materials must be fitted into the patterns of those courses. For example, simply preparing booklets on economic analysis or description of economic institutions to be included in courses in American history or civics is unlikely to have much influence. Conversely, carefully developed materials which fit into the pattern of the American history course and develop important economic concepts and ways of using those concepts within the flow of the history course have been found valuable by history teachers.

8. Over all, there is little likelihood that economic understanding in the high schools will improve greatly unless school administrators and teachers get more sympathetic and active aid from professional economists than they have had to date.

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BIASED EFFICIENCY GROWTH AND CAPITAL-LABOR SUBSTITUTION IN THE U.S., 1899–1960

By PAUL A. DAVID AND TH. VAN DE KLUNDERT*

It is by now generally conceded that the presence of a large residual element in the growth of aggregate output, an element that is not accounted for by the growth of inputs of capital and labor measured in a more or less conventional manner, is aesthetically unsatisfying in explanations of the supply side of economic growth. Moreover, the simple labeling of that residual element as the consequence of "technical progress," or an equivalently broad and imperfectly understood phenomenon, does not prove practically helpful in guiding decisions about policies aimed at influencing the aggregate growth rate. So long as the residual is no more than "a measure of our ignorance," a substantial portion of the observed rate of growth of output presents no handles for control.

In this situation it is hardly surprising that the notion that there is some sense in which "inputs" just equal output should acquire strong appeal. Indeed, this has been the line taken by much of the recent interesting work with aggregate production functions. The "residual" has been treated as the consequence of the mismeasurement of the inputs; conventional measures of inputs of labor and capital are regarded as inadequate because they fail to reflect alterations in the economic quality of physical units of the factors of production. As this approach is currently being pursued, the object of the game is to make the offending residual disappear by contriving new (and more appropriate) measures of the growth of labor and capital inputs which will, between them, fully account for the observed growth of output. (Cf. e.g., Denison [7], Domar [8], Griliches [12].) It leads to the "embodying" of "technical change" in capital inputs (cf., e.g., Solow [32] [34]), on the one hand, and, on the other, to the "embodying" of ostensibly superior technical

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knowledge and skill in the labor force through the agency of education.[1] Since everyone can make his own inputs and the only restriction imposed on a player is that he shall not personally overexhaust the growth of output, this is generally considered to be a particularly good game; it affords considerable opportunity for the exercise of ingenuity and offers a wide variety of results, each of which is at least internally quite consistent.

While it might be appropriate at some levels of discussion to raise objections to specific devices employed in carrying through the embodying operations, and to attempt to suggest refinements, it does seem more important to ask first how useful it is to continue playing the game under the present rules. Granting that conventional measures of inputs may be inappropriate because they fail to capture quality changes or, to put it differently, because one suspects that a broad array of secular developments has had "input-augmenting" effects, would it not be preferable to begin by establishing the magnitude of those effects before attempting to identify them with specific secular developments? In other words, would it not be sensible to start by trying to identify *the form* which the growth of conventional input efficiency has taken and then proceed to tackle the intriguing, but quite distinct, question of the sources of such growth?

As it is now, the typical *modus operandi* implicitly involves an effort to dispose of both issues by a single stroke. The generalized embodiment approach attempts to account for the growth of the residual by hypothesizing that various factors, such as technological advance in the design of capital, increased formal education, shortened working hours, have been responsible for the augmentation of one or another of the conventional inputs. The nature and magnitude of factor augmentation derived from each source considered is tacitly specified in the course of adjusting the conventional measures of labor and capital inputs. For example, having postulated that the spread and lengthening of formal education has augmented labor (and not capital), one might follow Denison [7] in correcting a man-hour input series so that it reflects the movements of an index of the (weighted) educational level of the work force. Although a check on the procedure would be afforded by comparison of the results of such adjustments for change in labor quality with the actual increase occurring in the efficiency of man-hour inputs, the empirical question of the form taken by total conventional factor-efficiency growth is suppressed; ultimate vindication of the hypotheses underlying the refashioned input measures is left to turn on their performance, as a combined package, in explaining the growth of output.

[1] Richard Nelson [26] has given neat expression to the formal symmetry between improvements in the quality of the capital stock, as a consequence of technical advances embodied in the design of new capital and alterations in the age distribution of the capital stock, and improvements in the quality of the labor force.

So long as there is no way of telling how increases in conventional total factor efficiency actually have been distributed between labor-augmentation and capital-augmentation, the present approach seems reasonable. But, as shall be seen, *it is possible to infer the rate of labor-augmentation and the rate of capital-augmentation from conventional measures of inputs and output.* Once such information is available, it may be used to place prior (not a priori) restrictions upon attempts at empirical identification of the sources of factor-efficiency growth. This would, at the very least, have the virtue of stabilizing the distribution of the residual between labor-associated and capital-associated improvements in factor efficiency and so prevent the disquieting pronounced shifts in the imputation of the residual to various causes proposed by each new statistical study.

With this providing one source of motivation, the present paper ventures an initial investigation of aggregate production-function relationships allowing for the possibility that the growth of the efficiency of conventional inputs may be "nonneutral," in the sense that the marginal productivities of those inputs do not increase at the same rate through time. Factor-efficiency growth so conceived may be nonneutral because technical innovations have a labor-augmenting or a capital-augmenting bias, or because unmeasured quality improvements in one of the inputs have taken place with relative rapidity. For the moment, however, the reasons will have to remain a subsidiary concern, and we shall, for the sake of simplicity, adopt the convention of regarding labor and capital inputs as being augmented by "technical change." Such contribution to the understanding of aggregate productivity growth as we can hope to make here will be confined simply to establishing the form taken by factor-efficiency growth in the private domestic sector of the U.S. economy during the present century, rather than identifying the sources from which it has flowed.

Of course, the implications of the form in which technical change has occurred extend beyond the sphere of current preoccupation with the sources of conventional productivity increase. In the context of the analysis of growth models the nature of technical change carries significance for the existence of an equilibrium growth path (cf. Solow [30], Uzawa [37], Amano [2]), while for students of the inventive and innovative process the question of the historical bias of technological advances has formed a subject of no little concern. (Cf., e.g., [25], especially paper by W. Fellner; Habakkuk [14].) Thus, an answer to the question of whether there has been any bias in the direction of technical change, and a measurement of such bias as has been experienced in the United States since the beginning of the twentieth century, are of interest in their own right.

Yet another matter of interest falls within the purview of this

paper, in part because it is one upon which the question of the neutrality of technical change has some bearing. Much of the research into productivity growth has been concerned with the algebraic form of the production function, especially with the magnitude of the elasticity of substitution between labor and capital.[2] In addition, substitution possibilities are an important determinant of the properties of dynamic two-factor models and are of significance in the operation of the price mechanism in free-market economies.[3] The assumption that technical change is Hicks-neutral (leaving relative marginal productivities of the factors undisturbed) is usually explicitly invoked by studies undertaking the estimation of the elasticity of substitution from time-series data.[4] While this assumption is not required for proper estimation of the substitution parameter, some estimation methods that have been used (cf., e.g., Kendrick and Sato [19], Kravis [21]) do demand Hicks-neutrality and are, consequently, misleading when technical change happens to be biased towards saving either labor or capital. The present study makes use of a general aggregate production function of the now familiar constant elasticity of substitution (CES) form, doing so in a manner that permits estimation of the substitution parameter while allowing for the possibility of nonneutral technical change.

The principal conclusions reached via this route may be anticipated briefly here. It is found that during the twentieth century technical change in the U.S. Private Domestic Economy has not been Hicks-neutral, nor, for that matter, has it been Harrod-neutral; although both labor-augmentation and capital-augmentation have been going on since 1900, these changes have been biased in a labor-saving direction. It is estimated that over this period capital-augmentation has proceeded at the rate of approximately 1.5 per cent per annum, while the annual rate of labor-augmentation has exceeded that by roughly 0.7 of a percentage point. Initial abandonment of the assumption that technical change was Hicks-neutral also leads to an estimated long-run elasticity of substitution in the neighborhood of 0.32, a value that casts very serious doubt

[2] This question may be regarded as one which involves the determination of the appropriate form for indexes of total factor input employed in the construction of measures of output per unit of total input. It is, then, in *principle* closely connected with the broad issue of the measurement and explanation of conventional total *productivity* change. As a practical matter, however, the form of the production function does not appear to make a great deal of difference in the calculation of total productivity indexes; within the range of variation of estimates that have been secured for the elasticity of substitution, productivity indexes are found to be rather insensitive to corresponding variations in the weighting schemes used to combine labor and capital inputs. Cf., e.g., Nelson [26, pp. 577–78].

[3] On two-factor growth models, cf., Solow [30], Eisner [9], Pitchford [29]. For other implications of the curvature of production-function isoquants, cf. ACMS [3].

[4] Hicks-neutrality is also commonly assumed for the purpose of estimating the rate of shift of the aggregate production function through time, even when the function is specified as being of the Cobb-Douglas form.

on the appropriateness of the (unitary elasticity of substitution) Cobb-Douglas form for aggregate production function analyses of the U.S. economy. The latter finding obviously heightens the attractiveness of the unrestricted input-augmentation concept adopted here, for if the production function were actually of the Cobb-Douglas form, Hicks-neutral technical change would not be less general than input-augmentation.[5]

I. *An Aggregate Production Function with Labor- and Capital-Augmenting Technical Change*

We begin by assuming that the aggregate production function is homogeneous of the first degree in inputs of capital and labor measured in efficiency terms, rather than in conventional units, and that it is characterized by constant elasticity of substitution between the "inputs" thus defined.

$$V = [(E_L L)^{-\rho} + (E_K K)^{-\rho}]^{-1/\rho} \tag{1}$$

fulfills these requirements, and the first and second derivatives of the volume of output, V, with respect to the inputs of labor and capital services ($E_L L$ and $E_K K$, respectively) obey all the normal conditions for a production function. In this notation, L and K represent conventional measures of the physical flow of labor and capital inputs, although in using the model we shall follow the common procedure of taking a conventional measure of the capital stock as a proxy for the flow of constant efficiency services it renders.[6] The coefficients E_L and E_K then represent the levels of efficiency of the conventional inputs of labor (measured as man-hours employed) and capital (measured, in principle, in terms of machines of a constant kind).[7] Alterations in E_L and E_K through time are to be interpreted as labor-augmenting and capital-augmenting "technical changes," although this says nothing about the sources of such efficiency growth. An increase in E_L is designated as labor-augmenting change, or labor-associated efficiency growth, despite the fact that it may be a consequence of the introduction of better machines.

The remaining parameter in the production function, ρ, is related to σ,

[5] Cf. Section I, below.

[6] If the durability of the stock has been declining, a conventional stock measure of capital in terms of resource costs in some base year will understate the rise in the services yielded by that stock, even if there has been no increase in the efficiency of the physical units comprising the stock. While such changes in durability may be quite important in short intervals of time (cf. Nelson [26, p. 578, n. 7] for reference to unpublished empirical work by Zvi Griliches on this question), it is difficult to believe that over the course of the six decades we shall consider this will be a serious source of error.

[7] Cf. Appendix C, Section 1.a, for amplification of the concept underlying the capital stock measure.

the elasticity of substitution; $\rho=(1-\sigma)/\sigma$, as is shown by Arrow, Chenery, Minhas, and Solow (ACMS) [3]. Since the elasticity parameter itself must be positive, the function given in equation (1) is only defined for $\rho \geq -1$. We assume, as has already been stated, that the substitution parameter, and hence ρ, is constant through time.[8]

The concept of factor-augmenting technical changes defined in this model (and the corollary notion of biases in the direction taken by technical change toward either relative labor-augmentation or relative capital-augmentation) can be related to the more familiar Hicksian concepts of neutral, labor-saving, and capital-saving technological progress. Following Hicks's [15, p. 122] definitions, "inventions" are to be classified as labor-saving, neutral, or capital-saving according to whether—given a constant capital-labor ratio—they lower the marginal productivity of labor relative to the marginal productivity of capital, leave the relative marginal productivities unaltered, or raise the marginal productivity of labor relative to that of capital. The equality of the rates of growth of labor and capital efficiency in the present model is exactly equivalent to neutrality in Hicks's sense. However, what we designate as a labor-augmenting bias in technical change ($\dot{E}_L/E_L > \dot{E}_K/E_K$) amounts to the same thing as a labor-saving innovation if, and only if, the elasticity of substitution is less than unity. Similarly, for a capital-augmenting bias in technical change to satisfy Hicks's definition of capital-saving, the elasticity of substitution must be less than unity.[9]

[8] Cf. Brown and De Cani [5] [6], for an attempt to allow for variations in the elasticity of substitution by the contrivance of defining "technological epochs."

[9] Cf. Solow [31]. To derive the foregoing set of equivalences, (1) may first be differentiated partially with respect to L and K, which yields:

$$(2)\qquad m_L = \frac{\partial V}{\partial L} = E_L^{-\rho}\left(\frac{V}{L}\right)^{1+\rho}$$

and

$$(3)\qquad m_K = \frac{\partial V}{\partial K} = E_K^{-\rho}\left(\frac{V}{K}\right)^{1+\rho}.$$

Now, differentiating (1), (2), and (3) with respect to time, we obtain

$$(1a)\qquad \frac{\dot{V}}{V} = \alpha\left(\frac{\dot{L}}{L}+\frac{\dot{E}_L}{E_L}\right)+\beta\left(\frac{\dot{K}}{K}+\frac{\dot{E}_K}{E_K}\right)$$

where α and β represent the elasticities of output, V, with respect to L and K, respectively. We also have

$$(2a)\qquad \frac{\dot{m}_L}{m_L} = \frac{1}{\sigma}\left(\frac{\dot{V}}{V}-\frac{\dot{L}}{L}\right)+\frac{\sigma-1}{\sigma}\frac{\dot{E}_L}{E_L},$$

and

Establishing these correspondent relationships not only serves to connect the concept of factor-augmentation with older and more familiar approaches to the problem of bias in technical change, it also has the virtue of simplifying our terminology. In anticipation of the finding that the elasticity of substitution is less than unity, we shall feel free to speak of technical change as labor-saving where $\dot{E}_L/E_K > \dot{E}_K/E_K$, and as capital-saving where the reverse is the case.

In order to utilize the production model which has been set forth here in empirical work, we assume perfect competition in all markets and, further, hypothesize that the observable relationships among V, L, and K can be regarded as the result of profit maximization subject to the constraint of the function given by (1). With this justification, the real wage, w, may be taken as equal to the marginal product of labor, m_L; the real rate of return on capital, r, can be set equal to m_K; and in place of the elasticity of output with respect to L, (α), and the elasticity of output with respect to K, (β), we may write the share of total output received by labor π_L and the share going to capital π_K, respectively. Dividing equation (2) by equation (3)—these equations are given in footnote 9—and making the appropriate substitutions for the marginal product terms, leads to the expression for the capital-labor ratio,

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$$\frac{\dot{m}_K}{m_K} = \frac{1}{\sigma}\left(\frac{\dot{V}}{V} - \frac{\dot{K}}{K}\right) + \frac{\sigma - 1}{\sigma}\frac{\dot{E}_K}{E_K}. \tag{3a}$$

Substituting from expression (1a) into (2a) and (3a) leads to the following relationships:

$$\frac{\dot{m}_L}{m_L} = \frac{\beta}{\sigma}\left(\frac{\dot{K}}{K} - \frac{\dot{L}}{L}\right) + \frac{1}{\sigma}\frac{\dot{E}}{E} + \frac{\sigma - 1}{\sigma}\frac{\dot{E}_L}{E_L}; \tag{2b}$$

$$\frac{\dot{m}_K}{m_K} = -\frac{\alpha}{\sigma}\left(\frac{\dot{K}}{K} - \frac{\dot{L}}{L}\right) + \frac{1}{\sigma}\frac{\dot{E}}{E} + \frac{\sigma - 1}{\sigma}\frac{\dot{E}_K}{E_K}, \tag{3b}$$

where the total efficiency growth rate, $\dot{E}/E$, is defined as

$$\frac{\dot{E}}{E} = \alpha\frac{\dot{E}_L}{E_L} + \beta\frac{\dot{E}_K}{E_K}.$$

From the last two equations it can readily be seen that if $\sigma < 1$, the Hicksian definition of a labor-saving change in technology,

$$\left(\frac{\dot{m}_L}{m_L} - \frac{\dot{m}_K}{m_K}\right) = \frac{\sigma - 1}{\sigma}\left(\frac{\dot{E}_L}{E_L} - \frac{\dot{E}_K}{E_K}\right) < 0,$$

is satisfied when $\dot{E}_L/E_L > \dot{E}_K/E_K$, and the Hicksian definition of a capital-saving change is satisfied when $\dot{E}_L/E_L < \dot{E}_K/E_K$. The absence of any difference in the rates of labor and capital augmentation $\dot{E}_L/E_L = \dot{E}_K/E_K$ meets the requirement for Hicks-neutrality $\dot{m}_L/m_L = \dot{m}_K/m_K$ with all nonnegative values of σ.

$$\frac{K}{L} = \left(\frac{w}{r}\right)^{\sigma}\left(\frac{E_L}{E_K}\right)^{1-\sigma}, \tag{4}$$

which, upon differentiation with respect to time, yields

$$\frac{\dot{K}}{K} - \frac{\dot{L}}{L} = \sigma\left(\frac{\dot{w}}{w} - \frac{\dot{r}}{r}\right) + (1-\sigma)\left(\frac{\dot{E}_L}{E_L} - \frac{\dot{E}_K}{E_K}\right), \tag{4a}$$

or, simply rearranging the terms,

$$\frac{\dot{w}}{w} - \frac{\dot{r}}{r} = \frac{1}{\sigma}\left(\frac{\dot{K}}{K} - \frac{\dot{L}}{L}\right) - \left(\frac{1-\sigma}{\sigma}\right)\left(\frac{\dot{E}_L}{E_L} - \frac{\dot{E}_K}{E_K}\right). \tag{4b}$$

Equation (4b) suggests two reasons for alterations in relative rates of factor remuneration: should capital services become more abundant than labor services, either because physical capital of a constant kind increases more rapidly than man-hour inputs or because capital-saving technical change renders labor services measured in efficiency terms comparatively less abundant, relative wage rates (per man-hour) will tend to rise. In contrast to this macroeconomic view, the form of equation (4a) suggests an essentially microeconomic explanation of the connections among these variables: increases in the wage rate relative to the return on capital induce substitution for labor, producing a rise in the capital-labor ratio expressed in conventional terms, while technical change of a labor-saving sort has precisely the same influence.

It will readily be seen that by multiplying both sides of equation (4) by $(L/K)^{\sigma}$, we obtain an expression relating the capital-labor ratio to the relative factor shares (π_L/π_K) and the ratio of the efficiency levels of labor and capital:

$$\frac{K}{L} = \left(\frac{\pi_L}{\pi_K}\right)^{\sigma/(1-\sigma)}\left(\frac{E_L}{E_K}\right). \tag{5}$$

Now, if it is assumed that any changes in the *relative* efficiency level of labor which take place over the course of time do so at a constant geometric rate $(\lambda_L - \lambda_K)$ given by

$$\left(\frac{E_L}{E_K}\right)_t = \frac{E_L(0)}{E_K(0)}e^{(\lambda_L-\lambda_K)t}, \tag{6}$$

substitution of this condition into (5) leads to the following (natural) logarithmic relationship:

$$\ln\left(\frac{K}{L}\right) = \left(\frac{\sigma}{1-\sigma}\right)\ln\left(\frac{\pi_L}{\pi_K}\right) + (\lambda_L - \lambda_K)t + \ln\left(\frac{E_L(0)}{E_K(0)}\right). \tag{7}$$

Although the stipulation that biased factor-augmentation, either labor-

saving or capital-saving, can only proceed at a steady rate through time is admittedly quite restrictive, it does provide, in equation (7), the basis for a least-squares regression model that can be employed in estimating the elasticity of substitution without making the much stronger assumption of neutrality of technical change.[10] By the same token, this model permits estimation of the magnitude of the (exponential) bias in efficiency growth.

The implication of using equation (7) to estimate the elasticity of substitution (σ) under an a priori specification of Hicks-neutral technical change becomes fully apparent when one returns for a moment to consider equation (4a). From the latter it is immediately seen that, *ceteris paribus*, assuming Hicks-neutrality when $[(\dot{E}_L/E_L)-(\dot{E}_K/E_K)]$ is positive must lead to an overestimate of the elasticity parameter, whereas, when $[(\dot{E}_L/E_K)-(\dot{E}_K/E_K)]$ is actually negative, the estimate of σ will be biased downward. In a recent article Kendrick and Sato [19, pp. 980–81] present an estimate of the elasticity of substitution obtained as "the difference between the growth rate of capital and labor inputs, divided by the difference between the growth rates of the real prices of labor and capital"—a procedure which, as (4a) makes evident, rests upon the assumption of Hicks-neutrality.[11] The Kendrick-Sato estimate is $\sigma=0.58$ for the U.S. Private Domestic Economy, 1919–60; if, as is the case, technical change during that period has been labor-saving rather than neutral (i.e., if $(\lambda_L-\lambda_K)>0$, and $\sigma<1$), it is only to be expected that an estimation procedure allowing for nonneutrality in technical change will result in a smaller value being obtained for the elasticity of substitution.

It should be clear that from a theoretical viewpoint selection of the conventional capital-labor ratio as the dependent variable, in equation (7), for purposes of regression analysis is an arbitrary choice. On practical grounds, however, there is something to be said in its favor: the nature of the available data makes it quite likely that the ratio of real capital stock estimates to the estimates of man-hours employed will contain substantial year-to-year errors of measurement, errors which it will

[10] Throughout this paper we omit the stochastic term in presenting regression models.

[11] Kendrick [18, pp. 120–21] discusses the possibilities of labor-saving technical change, but then proceeds to compute estimates of the arc-elasticity of substitution from the observed changes in the capital-labor ratio and an index of relative factor prices, without mentioning the tacit assumption of neutral technical change involved in this calculation. The necessity of the neutrality assumption for this method of estimation is made clear by its appearance at the beginning of the formal derivation supplied in Kendrick and Sato [19, Appendix A], but it is nowhere referred to in the text of the article. I. B. Kravis, in his 1959 article [21, pp. 940-41] which anticipated the current interest in measuring the elasticity of substitution, took the same approach, but presented his findings in a more cautious manner: "The doubling of the quantity ratio [K/L, in our notation] and the drastic decline of the price ratio [r/w, in our notation] imply an 'historical' elasticity of substitution of .64, but the mechanism underlying these changes is far from clear."

not be feasible to correct. Such being the case, econometric considerations indicate the desirability of having the errors of measurement occur in the dependent variable, and not among the explanatory variables of the regression.[12] Nevertheless, the macroeconomic interpretation offered for equation (4b) provides a forcible reminder that a single-equation estimation procedure such as that proposed here must go forward under the burden of a simultaneous-equations bias of unknown dimensions. At present we are not prepared to remove this encumbrance by undertaking the simultaneous estimation of a complete model.

II. *Parameter Estimates for the U.S. Private Domestic Economy, 1899–1960*

The basic regression model developed in the preceding section will, in this section of the paper, be elaborated and fitted to observations on the U.S. Private Domestic Economy for the period 1899–1960. A description of the sources of the data employed, together with an all-too-cursory set of comments on the problems they pose, is to be found in Appendix C. To put the matter here most concisely, we make use of statistics provided by J.W. Kendrick [18] for man-hours employed and the value of the net capital stock in 1929 prices as measures of L and K, respectively. As an appropriate measure of π_L we have, with all due trepidation, fixed upon the proportion of employee compensation in Gross (Private) Business Product.

However, in Part A of this section data for the Private Domestic Economy in the period 1899–1960 will first be used to fit a regression model which does not require information about the real capital stock, but which nonetheless yields estimates of the elasticity of substitution and the rate of technical change—the latter on the assumption that technical change is neutral. This preliminary empirical step has a double purpose; it provides a basis for comparing our subsequent statistical results with those obtainable for the same social accounting entity and time period by the application of a more commonly employed estimation method, and, secondly, it serves to bring into focus some serious difficulties encountered by this well-established approach to estimating the elasticity of substitution. Then, in Part B, a regression model derived from equation (7), permitting estimation of a constant rate of change in relative conventional input efficiency, is fitted to the data after the introduction of a number of modifications designed to cope with problems

[12] Cf. footnote 18 below. The problem of errors of measurement associated with the ratio K/L is all the more serious in light of the fact that the regression model ultimately to be fitted to the data includes a lagged value of K/L. (Cf. Section II, equation (II).) This is equivalent to introducing a first-difference between the successive K/L ratios as one of the variables of the equation, and, given the likelihood of even greater measurement errors in the first-differences than exist in the original K/L series, it is especially desirable to arrange the regression model so that the first-difference term would appear as the dependent variable.

posed by cyclical variations in the rate of utilization of the capital stock and lags in the response of the capital-labor ratio to alterations in relative factor prices. In the final section (Section III) of the paper, the parameter estimates derived in Part II.B are used first to investigate the behavior of relative factor efficiency levels over time, and then to compute rates of growth of labor- and capital-efficiency and the contributions made by labor-augmenting and capital-augmenting improvements to the long-run rate of growth of total conventional factor productivity.

A. *Estimating the Elasticity of Substitution Without Capital Stock Data*

Although the preceding discussion has considered means of estimating the elasticity of substitution which involved relationships between the conventional capital-labor ratio and relative factor prices (or shares), an alternative approach to estimating parameters of the CES function is available.[18] Dividing both sides of equation (2) by w, and rearranging terms, we have

$$\pi_L = w^{1-\sigma} E_L^{\sigma-1}. \tag{8}$$

If it is then specified that the efficiency of conventional labor inputs, E_L, grows through time at the exponential rate λ_L, equation (8) leads immediately to a natural logarithmic expression from which the parameters σ and λ_L may be estimated without any information regarding the growth of conventional capital inputs:

$$\ln \pi_L = \ln E_L(0) + (1 - \sigma) \ln w + \lambda_L(\sigma - 1)t. \tag{9}$$

In their pathbreaking 1961 article ACMS [3, p. 244] essentially fitted equation (9) to data for the U.S. Private Nonfarm sector during the period 1909–49 and obtained $\sigma = 0.569$ as an estimate of the elasticity of substitution, and $\lambda_L = 0.0183$. It should be observed that although ACMS started by considering an aggregate production model in which technical change was Hicks-neutral, rather than the more general input-augmentation form here given by (1), their approach to the estimation of σ does not depend upon any assumption of Hicks-neutrality; the latter need only be invoked by ACMS *ex post* to justify interpreting the estimated rate of growth of labor efficiency as being identical to the rate of growth of capital efficiency, λ_K, and hence equal to the rate of neutral technical change, $\lambda = \lambda_L = \lambda_K$.

Fitting the same equation (9) by least-squares regression to the data

[18] Because this approach avoids the use of any information regarding the real capital stock, it is of considerable convenience in empirical work in areas where such data are scarce. Cf., P. A. David, "Economic History Through the Looking-Glass," a paper read at the Boston meeting of the Econometric Society, December 1963, abstracted in *Econometrica*, October 1964.

described in Appendix C for the Private Domestic Economy in the period 1899–1960,[14] yields the following result:

$$\text{(I)} \qquad \ln \pi_L = 16.1915 + 0.3815 \ln w - 0.0072t; \qquad R = .922$$
$$\qquad\qquad (9.54) \qquad (-6.54) \qquad d = 1.113.$$

The figures appearing in parentheses below the regression coefficients are the values of the t-statistics derived from tests of the null hypotheses that the respective coefficients are not significantly different from zero.[15] From (I) it is seen that the coefficient of the real wage variable is significantly different from zero at virtually any level of confidence one might care to name, implying that σ is less than unity and that the production function is therefore not of the Cobb-Douglas form. As an estimate of the elasticity of substitution we have $\hat{\sigma} = .619$. The coefficient of t is also highly significant, and taken in conjunction with that of $\ln w$ provides an estimate of $\hat{\lambda}_L = .0190$, or 1.90 per cent per annum.

These statistical results are in close agreement with those of ACMS [3], save for the interpretation placed upon the estimated rate of growth of labor efficiency. Although ACMS do not offer evidence in support of their reading of $\hat{\lambda}_L$ as the rate of neutral technical change, they do advance a test of neutrality [3, pp. 235–36, 245] which, in terms of the notation used in stating the production function (1), would amount to computing the values of E_L/E_K through time from the estimates of σ and λ_L and from a formula derived by writing (1) as:

$$\text{(10)} \qquad \left(\frac{E_L}{E_K}\right)^{\rho} = \left[E_L\left(\frac{K}{V}\right)\right]^{\rho} - \left(\frac{K}{L}\right)^{\rho},$$

where E_L is to be calculated from $E_L = (E_L(0))e^{(\lambda_L)t}$. The absence of a trend in the time path of E_L/E_K would speak in favor of the assumption of neutral technical change and the identification of λ_L with λ. However, if the estimate of the elasticity of substitution employed in making the computation is for any reason biased, the proposed test can be quite misleading. Should the estimate of σ be too large, its use in (10) biases the test against finding a significant upward trend in the relative efficiency of labor.[16] To put it simply, the degree of conviction with which this

[14] Average annual real wages, w, were derived for the purpose of fitting model (I) from the wage share π_L and real gross private domestic product per man-hour employed, since $w = \pi_L(V/L)$.

[15] The convention of reporting such t-statistics rather than the standard errors of the regression coefficients is adhered to throughout the presentation of our statistical findings; the letter d represents, as usual, the Durbin-Watson statistic, and R is the coefficient of multiple correlation adjusted for degrees of freedom.

[16] If σ is overstated by $\hat{\sigma}$, then $\hat{\rho}$ is biased downward, and the derived estimate $\hat{\lambda}_L$ will also be biased downward. The computed values of $\hat{E}_L$ will then fail to reflect the actual extent of the rise in E_L over time, and the time path of E_L/E_K calculated from (10) will be biased downward.

test of neutrality can be accepted hinges on the strength of one's trust in the estimated value for the elasticity parameter.

There are rather persuasive reasons for suspecting that the regression model provided by (9) leads to estimates of σ that are, in fact, too high.[17] Since it has been seen that this method of estimating the elasticity of substitution does not depend upon the assumption of neutral technical change, it should be clear that any such bias must arise from a quite different source. Specifically, we suggest that there is a problem of bias occasioned by the presence of errors of observation in the (explanatory) real wage variable appearing in (9). The adjustment of the capital-intensity of production in response to relative factor prices that underlies the regression model is not a short-run process; it may therefore be argued that the capital-labor ratios desired at various points in time are not significantly affected by transitory movements in real wage rates which are merely reflections of short-period variations in the level of aggregate demand. At any moment, then, the choice of technique is influenced not by the prevailing actual real wage which includes a transitory, cyclical component, but by what may be called the "structural" or "secular" real wage rate. The line of argument is now already so reminiscent of the statistical underpinnings of Friedman's [10] Permanent Income Hypothesis that it should not be necessary to elaborate it further in the text: the presence of a theoretically extraneous, transitory component in observed average annual real wage rates would produce a downward bias in the simple least-squares estimate of the coefficient of $\ln w$ in (9), which would in turn yield an upward biased estimate of σ.[18]

Essentially the same result may be seen immediately if equation (4) were to be used to compute $(\dot{E}_L/E_L)-(\dot{E}_K/E_K)=(\lambda_L-\lambda_K)$: given the rate of growth of K/L and of relative factor prices (or shares), employing too large an estimate of σ in equation (4) introduces a downward bias in the computed value of $(\lambda_L-\lambda_K)$.

[17] Cf. ACMS [3, p. 245] for discussion of the simultaneous-equations-bias problem raised by the time-series estimation of (9). There is no mention of other sources of bias or of the direction in which they might work.

[18] McKinnon [24, p. 514], has also pointed out that business-cycle phenomena may be a possible source of bias in time-series estimates of the elasticity of substitution, but his argument is rather different from that advanced here. Instead of (9) we start with the functional relationship

$$\text{(i)} \qquad \ln\left(\frac{L}{V}\right)^* = \ln E_L(0) - \sigma \ln w^* + (\sigma - 1)\lambda_L t,$$

where w^* is the structural real wage rate and $(V/L)^*$ is the productivity of labor as determined by E_L and $(K/L)^*$, the latter being determined—for a given state of technology—by $(w/r)^*$. Denote the actual average real wage by $w=w^*(1+\omega)$, where $w^*\omega$ is the transitory, cyclical component, and actual $(L/V)=(L/V)^*(1+\mu)$, where $(L/V)^*\mu$ is a tranistory component of actual labor requirements per unit of output arising from unplanned cyclical variations in the utilization of fixed capital. (On the short-run behavior of (L/V) in U. S. manufacturing, cf. Wilson and Eckstein [38].) Then, substituting for $(L/V)^*$ and w^* in (i), and adding $\ln w$ to both sides, we have:

As an alternative route to the conclusion that the least-squares estimate of σ from equation (9) is likely to be biased upward, it may be argued that there is a specification error in the model. Indeed, the presence of significant autocorrelation of the residuals in (I) does lend a measure of support to the notion that (9) contains a specification error of some sort.[19] It is therefore worth observing that in the short run π_L is influenced by the level of effective demand, tending to fall in booms, when the unemployment rate (U) is declining, and to rise in the downswing of the cycle. This occurs because labor productivity tends to rise more rapidly than real wages when unemployment is falling while the reverse happens at the stage of the cycle at which unemployment is beginning to rise. If $\ln \pi_L$ and $\ln U$ exhibit this positive relationship and $\ln w$ and $\ln U$ are negatively related, it can be shown through the application of Theil's [35, p. 43] formula for specification errors that the coefficient of $\ln w$ in (9) will be underestimated by simple least squares.[20]

In the light of such considerations one is inclined to question the validity of the test of neutrality suggested by ACMS [3] and to suspect that the method of estimating the elasticity of substitution and the rate of growth of labor efficiency employed in (I) results in an overestimate of the former and, consequently, in an understatement of the latter. Rather than explore further in this direction, we proceed to a more direct statistical test of the neutrality assumption which also attempts to provide a measure of the long-run elasticity of substitution free from the distorting influence of business-cycle phenomena.

B. *A Model Allowing Exponential Bias in Efficiency Growth*

The basic regression model given by (7), being derived from the production function (1), should be interpreted as indicating the relation-

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(ii) $$\ln \pi_L = \theta_0 + \theta_1 \ln w + \theta_2 t + \eta,$$

where $\theta_1 = (1-\sigma)$, and the stochastic term η is,

(iii) $$\eta = \ln(1+\mu) - \sigma \ln(1+\omega).$$

Even if the error terms $\ln(1+\mu)$ and $\ln(1+\omega)$ are assumed to be mutually and serially independent with constant variances, and also to be independent of w^* and $(L/V)^*$, the full assumptions justifying the application of simple least squares in estimating θ are not met; η is not independent of w, the explanatory variable in the regression model. It follows, then, that the simple least-squares estimate $\hat{\theta}$ will be biased and inconsistent, and that the direction of the bias is downward. (Cf., e.g., Johnston [17, pp. 149–50].) Since $\hat{\theta}$ is biased downward, $\hat{\sigma}$ will be too large.

[19] With $d = 1.113$, the hypothesis of no serial correlation must be rejected at the 1 per cent level. Cf. Theil [36].

[20] Writing a_1 for the regression coefficient of $\ln \pi_L$ on $\ln w$, a_2 for the regression coefficient of $\ln \pi_L$ on $\ln U$, and a_3 for the regression coefficient of $\ln U$ on $\ln w$, then

$$\mathcal{E}(a_1) = (1-\sigma) + a_2 a_3.$$

If $a_3 < 0$ and $a_2 > 0$, then $\mathcal{E}(a_1) = (1-\hat{\sigma}) < (1-\sigma)$.

ship between the *desired* capital-intensity of production and the *expected* or "normal" long-run level of relative factor shares, rather than between *actual* levels of capital-intensity ($k=K/L$) and relative factor shares ($\pi=\pi_L/\pi_K$) at every point in time.[21] Defining *desired* capital-intensity as k^* and the expected, long-run ratio of factor shares as π^*, (7) can be rewritten explicitly as:

$$\ln k_t^* = \frac{E_L(0)}{E_K(0)} + \left(\frac{\sigma}{1-\sigma}\right) \ln \pi_t^* + (\lambda_L - \lambda_K)t. \tag{7a}$$

It would then seem reasonable to assume that expectations regarding the "normal" level of relative shares are formed on the basis of the past history of their actual levels, and that recent history leaves a stronger imprint upon such expectations than do events farther removed in time. Following along these lines, π^* might be specified to be the exponentially weighted geometric average of all previous actual (π) values,

$$\pi_t^* = \prod_{j=0}^{\infty} \pi_{t-j}^{\phi(1-\phi)^j}, \quad \text{where} \quad 0 < \phi < 1, \tag{11}$$

or, taking logarithms,

$$\ln \pi_t^* = \sum_{j=0}^{\infty} \phi(1-\phi)^j \ln \pi_{t-j}, \tag{11a}$$

implying that the exponential weights increase geometrically as the actual (π) values considered approach the present. The distributed-lag specification given in (11a) is equivalent to

$$\ln \pi_t^* - \ln \pi_{t-1}^* = \phi(\ln \pi_t - \ln \pi_{t-1}^*), \tag{11b}$$

or, taking antilogarithms,

$$\frac{\pi_t^*}{\pi_{t-1}^*} = \left(\frac{\pi_t}{\pi_{t-1}^*}\right)^{\phi}. \tag{11c}$$

It is simply the ratio version of the familiar Nerlove [27] distributed-lag form, chosen here for its convenience in the estimation of regressions involving the logarithms of variables rather than the variables themselves.[22]

[21] The notion of producers forming expectations as to the behavior of relative factor shares is perhaps less intuitively appealing than the idea that they have expectations regarding "normal" or long-run factor prices. The two approaches may be logically equivalent, but it should be recognized that they may lead to different empirical results. While we have chosen here to avoid the complexities of introducing separate corrections for wage-rate and rental-rate expectations, this is clearly a matter calling for further investigation.

[22] For an extensive discussion of distributed-lag techniques, cf. Nerlove [27].

Although (7a) says that the desired capital-intensity is adjusted in response to the influence of π^*, it need not, indeed, it should not be assumed that such adjustments are completed so that the desired capital-intensity is always identical to the actual capital-intensity of production in each period of time. Since we are dealing with an adjustment process involving fixed capital of durability considerably greater than the length of annual periods of observation, and a capital stock comprised of some indivisible elements whose presence militates against complete, instantaneous alterations in factor proportions, the total adjustment of the actual capital-labor ratio to the desired level may be presumed to take place only with some lag. Once again employing the ratio form of the lag specification suggested by Nerlove, it may be postulated that:

$$\text{(12)} \qquad \frac{k_t}{k_{t-1}} = \left(\frac{k_t^*}{k_{t-1}}\right)^\gamma, \qquad 0 < \gamma < 1.$$

This expression says that changes in the ratio of actual capital-intensities in all pairs of consecutive periods are a constant fraction of the changes in the ratio of the capital-intensity currently desired to the actual capital-intensity of production in the previous period. The parameter γ may, therefore, be regarded as the elasticity of adjustment, indicating the fraction of the desired adjustment that is completed in the course of a single year.[23]

Let us consider the regression model that incorporates the foregoing corrections for the transitory component in the actual level of relative factor shares and for lags in the adjustment of the actual capital-intensity of production to the desired level. Taking (natural) logarithms of equation (12), we have

$$\text{(12a)} \qquad \ln k_t = \gamma \ln k_t^* + (1 - \gamma) \ln k_{t-1},$$

and, substituting from (7a) for $\ln k^*_t$:

$$\text{(13)} \quad \ln k_t = \gamma\alpha_0 + \left(\frac{\gamma\sigma}{1 - \sigma}\right) \ln \pi_t^* + \gamma(\lambda_L - \lambda_K)t + (1 - \gamma) \ln k_{t-1},$$

[23] Equation (12) is equivalent to the assumption that the actual values of k are exponentially weighted products of all previous desired values of k^*, with the weights rising geometrically as the values of k^* approach the present. Symbolically,

$$\ln k_t = \sum_{\tau=0}^{\infty} \gamma(1 - \gamma)^\tau \ln k_{t-\tau}^*, \qquad 0 < \gamma < 1.$$

This form is not superior on any a priori grounds to other conceivable specifications of the adjustment process. We will subsequently make use of a form which implies that past values of the capital stock in existence, rather than the capital stock in use, are considered in adjusting to the desired degree of capital-intensity.

where, for notational convenience, $\alpha_0 \equiv E_L(0)/E_K(0)$. From (13) we progress, via some intermediate steps, to the regression model:[24]

$$\ln k_t = v_0 + v_1 \ln \pi_t + v_2 t + v_3 \ln k_{t-1} + v_4 \ln k_{t-2}, \tag{15}$$

where,

$$v_0 = [\gamma(\lambda_L - \lambda_K)(1 - \phi) + \phi\gamma\alpha_0]$$

$$v_1 = \phi\gamma\left(\frac{\sigma}{1 - \sigma}\right)$$

$$v_2 = \phi\gamma(\lambda_L - \lambda_K)$$

$$v_3 = [(1 - \phi) + (1 - \gamma)]$$

$$v_4 = (1 - \phi)(\gamma - 1)$$

As it presently stands, the regression model given by (15) suffers from several deficiencies which militate against its immediate application to the available data. In the first place, inspection of the relationships among the coefficients ($v_0, \cdots, v_4$) reveals that while it is possible to estimate the sum and the product of the parameters ϕ and γ, and, hence, to estimate σ and $(\lambda_L - \lambda_K)$, one would still be unable to say which was ϕ and which was the elasticity of adjustment, γ. This in turn would prevent estimation of α_0. Secondly, as a rather more serious practical obstacle, the presence on the "explanatory" side of the equation of two previous values of the capital-intensity variable (k_{t-1} and k_{t-2})—which exhibits a strong upward trend—as well as the time variable, t, creates a virtually overwhelming multicollinearity problem, vitiating useful application of the regression model in this context.[25] Thirdly, the development of the model has glossed over a difficulty that almost always plagues empirical implementation of models that call for information

[24] Substitution in (13) from (11b) leads to:

$$\ln k_t = \gamma\alpha_0 + \left(\frac{\gamma\sigma}{1-\sigma}\right)\phi \ln \pi_t + \left(\frac{\gamma\sigma}{1-\sigma}\right)(1 - \phi) \ln \overset{*}{\pi}_{t-1} + \gamma(\lambda_L - \lambda_K)t + \ln k_{t-1}. \tag{14}$$

But from (13) we also have,

$$\ln \overset{*}{\pi}_{t-1} = \frac{(1-\sigma)}{\gamma\sigma} \ln k_{t-1} - \frac{(1-\sigma)(1-\gamma)}{\gamma\sigma} \ln k_{t-2} - \frac{(\lambda_L - \lambda_K)(1-\sigma)}{\sigma} t + \frac{(1-\sigma)[(\lambda_L - \lambda_K) - \alpha_0]}{\sigma},$$

which, upon substitution into (14), yields (15).

[25] The real villain of the piece is k_{t-2}, which entered as a consequence of the replacement of π in equation (7) by π^*, and the form of the distributed-lag specification given for π^*. To avoid the severe multicollinearity resulting from inclusion of k_{t-2} among the independent variables, it will be necessary to handle the correction for transitory movements in relative factor shares in a less straightforward manner.

about the input of conventional capital services: the existence of temporal variations in the proportion of the physical capital stock actually employed in production. Since we have already gone as far as to take a stock concept of capital as a proxy for the flow of capital services, thereby disregarding possible secular alterations in durability, the least that must be done to bring the data into line with the conceptual requirements of the production function is to introduce a means of adjusting the available statistics on the existing capital stock for short-run changes in the rate at which it is utilized.

It is possible to remove these deficiencies while retaining some form of correction for differences between actual and expected or "normal" relative factor shares, as well as an adjustment for lags in the response of the actual capital-labor ratio. But to do so, one must pursue a slightly different and somewhat cruder line of attack. We proceed by first reformulating the correction for the transitory component in the movements of observed relative factor shares. Next, a device to correct the data for variations in the rate of utilization of the existing, or nominal, capital stock will be explicitly introduced into the regression model. Finally, we shall modify the hypothesis about the way in which lagged adjustments are made in the degree of capital-intensity.

If, as has been previously argued, short-run fluctuations in relative (factor prices and hence in) factor shares are discounted as reflecting the transitory influence of business-cycle conditions, an alternate approach would try to take account of this discounting procedure in an explicit fashion, instead of doing so implicitly through the device of a distributed-lag specification for π^*. Since labor's share displays a tendency to move inversely to the rate of unemployment (U) over the course of the business cycle, it may be hypothesized that π_L^* is higher than actual π_L when the rate of unemployment is abnormally low, while it lies below actual π_L during abnormally high periods of unemployment. On this line of reasoning, it may be postulated that the ratio of "normal" to actual relative factor shares varies positively with the rate of employment, so that a 1 per cent change in the latter is always accompanied by a constant, but unspecified, per cent change (δ) of the former in the same direction. Symbolically, in place of equation (11), we assume that:

$$\text{(16)} \qquad \left(\frac{\pi_L}{\pi_K}\right)^* = \left(\frac{\pi_L}{\pi_K}\right)c_0(1-U)^\delta, \qquad c_0 > 0,\ \delta \geq 0,$$

or, taking (natural) logarithms,

$$\text{(16a)} \qquad \ln \pi^* = \ln \pi + \delta \ln (1-U) + \ln c_0.$$

Then, substituting (16a) in (7a), we obtain the revised basic equation for the desired capital-intensity of production:

$$(17)\quad \ln k_t^* = \left(\frac{\sigma}{1-\sigma}\right)\ln \pi_t + \left(\frac{\sigma\delta}{1-\sigma}\right)\ln(1-U)_t + (\lambda_L - \lambda_K)t + \alpha_0'' ,$$

where, for convenience,

$$\alpha_0' \equiv \left[\left(\frac{\sigma}{1-\sigma}\right)\ln c_0 + \alpha_0\right].$$

There is no prescribed way to correct statistics relating to the real capital stock in existence so that they will reflect the movements of that part of the stock which is actually being utilized. (Such a correction is necessary to transform the data on the observed conventional capital-labor ratio ($\hat{k}$) into a series describing the behavior of the actual degree of conventional capital-intensity (k), which is the variable that is called for by the model.) Yet it does seem sensible to relate variations in the utilization of capital to concurrent variations in the rate at which the labor force is utilized or, in other words, to the employment rate. Alternative specifications for this relationship have been advanced in the literature, but, supported by the results of some experimentation which is reported in Appendix A, we shall make use of the form

$$(18)\qquad k = \hat{k}d_0(1-U)^{\zeta}, \qquad \zeta > 0,$$

where d_0 is some positive constant, and U is the proportion of the labor force unemployed. Equation (18) says simply that percentage changes in the rate of utilization of the existing capital stock are a constant fraction or multiple of the concurrent percentage changes in the labor force employment rate. Instead of prespecifying the magnitude of this constant fraction or multiple (ζ), we shall leave it to be determined in the course of fitting the complete model.

Having distinguished the observed or nominal capital-labor ratio ($\hat{k}$) from the actual degree of conventional capital-intensity (k), it is now necessary to reconsider the process through which actual capital-intensity of production is brought into line with the desired degree of capital-intensity (k^*). The description of that process provided by equation (12) assumes that past values of the actual capital-intensity of production are compared with the level currently desired. However, it appears no less reasonable to suppose that, in adjusting the actual technique of production to the desired technique, the capital stock *in existence* in preceding periods is what is considered by decision-makers, rather than just that part of the stock which had actually been in use. Because of fixed costs of capital, firms may well tend to compensate for periods of unplanned underutilization of existing plant and equipment by moving, in the next period, to a level of actual capital-intensity somewhat higher than would have been selected in the absence of previous sub-

normal utilization. Keynes's [20, pp. 69–71] argument, that user cost on equipment tends to fall if capital is redundant and entrepreneurs anticipate that the redundancy will not be removed quickly in the future, would seem to support this line of reasoning. Abnormal underutilization, reflected in $(\hat{k} > k)$, would lower calculated costs of using equipment rather than leaving it idle, and thus tend to raise actual capital-intensity (k) relative to desired capital-intensity (k^*) in the following period.[26]

The foregoing considerations provide one type of justification for formally hypothesizing that the process of adjusting the actual capital-intensity of production is perhaps better described by

$$\text{(19)} \qquad \frac{k_t}{\hat{k}_{t-1}} = \left(\frac{k_t^*}{\hat{k}_{t-1}}\right)^{\gamma},$$

than by equation (12). There is another, and possibly more persuasive, reason for working with (19): retaining the form given in (12) would, in conjunction with (18), introduce both current and lagged values of the employment variable, $\ln(1-U)$, into the regression analysis. This would only lead one back into a serious multicollinearity problem. (Cf. Appendix A.)

Therefore, taking (natural) logarithms of (19), and substituting for k^* from (17), we arrive at the expression:

$$\text{(20)} \quad \ln k_t = \gamma\left(\frac{\sigma}{1-\sigma}\right)\ln \pi_t + \delta\left(\frac{\gamma\sigma}{1-\sigma}\right)\ln(1-U)_t + \gamma(\lambda_L - \lambda_K)t + (1-\gamma)\ln \hat{k}_{t-1} + \gamma\alpha_0'.$$

Substitution for k from (18) then leads to the revised regression model:

$$\text{(21)} \quad \ln \hat{k}_t = v_0' + v_1' \ln \pi_t + v_2' t + v_3' \ln \hat{k}_{t-1} + v_4' \ln(1-U)_t$$

$$v_0' = \left[\left(\frac{\gamma\sigma}{1-\sigma}\right)\ln c_0 + \gamma\left(\frac{E_L(0)}{E_K(0)}\right) + \ln d_0\right]$$

$$v_1' = \gamma\sigma/(1-\sigma)$$

$$v_2' = \gamma(\lambda_L - \lambda_K)$$

$$v_3' = (1-\gamma)$$

$$v_4' = \left[\left(\frac{\gamma\sigma\delta}{1-\sigma}\right) - \zeta\right]$$

[26] One implication of such a situation is that actual rates of utilization of capital would not tend to fall as sharply as rates of employment during cyclical contractions; in terms of (18), the elasticity parameter ζ ought to be found to be less than unity. Cf. below, for evidence pointing in this direction and for a discussion of the difference between the concept of capital underutilization adopted here and the more commonly held notion of "excess capacity."

From the viewpoint of the desirability of providing estimates of *all* the parameters of the model, equation (21) is unfortunately no more satisfactory than (15), inasmuch as separate estimates for only three of the eight parameters can be obtained directly from the coefficients fitted for (21) by simple least-squares regression; values for $E_L(0)/E_K(0)$, c_0, d_0, δ and ζ, unfortunately cannot be shaken out of the vector of estimates $(v_0', \cdots, v_4')$. However, the parameters σ, $(\lambda_L - \lambda_K)$, γ, which can be estimated, are those of major interest here. It is also possible to go a bit further and compute a lower-bound estimate of ζ, the elasticity of the utilized capital stock with respect to the labor force employment rate.[27]

The result of fitting equation (21) to observations on $(\hat{K}/L)$, $(\pi_L/(1-\pi_L))$, and $(1-U)$ for the U.S. Private Domestic Economy during the period 1899–1960,[28] by simple least squares is shown by

$$\text{(II)}\qquad \ln\left(\frac{\hat{K}}{L}\right)_t = -2.1670 + \underset{(1.91)}{0.1285}\ln\left(\frac{\pi_L}{1-\pi_L}\right)_t + \underset{(2.13)}{0.0020t} + \underset{(8.09)}{0.7225}\ln\left(\frac{\hat{K}}{L}\right)_{t-1} - \underset{(-2.53)}{0.3153}\ln(1-U)_t,$$

$$R = .980, \qquad d = 1.252.$$

Making use of relationships (21a)–(21d), in footnote 27, the estimated parameters implied by (II) are:

$$\hat{\gamma} = 0.2775$$
$$(\lambda_L \hat{-} \lambda_K) = 0.0072$$
$$\hat{\sigma} = 0.3165$$
$$\hat{\zeta} \geq 0.3153$$

[27] These estimates are obtained from (21) as:

$$\text{(21a)}\qquad \hat{\gamma} = (1 - \hat{v}_3'),$$

$$\text{(21b)}\qquad (\lambda_L \hat{-} \lambda_K) = \frac{\hat{v}_2'}{(1-\hat{v}_3')},$$

$$\text{(21c)}\qquad \hat{\sigma} = \left[\frac{\hat{v}_1'}{(1-\hat{v}_3')}\right] \Big/ \left[1 + \frac{\hat{v}_1'}{(1-\hat{v}_3')}\right] = \frac{\hat{v}_1'}{1 + \hat{v}_1' + \hat{v}_3'},$$

and, since

$$\text{(21d)}\qquad \zeta = v_1'\delta - v_4',$$

and $\delta > 0$, if it is to have the meaning assigned to it in (16), we have the lower-bound estimate $\hat{\zeta} \geq -\hat{v}_4'$, if $\hat{v}_1' \geq 0$.

[28] Cf. Appendix C for source of data on the rate of unemployment in the civilian labor force, used in fitting (II).

From the t-values for the H_o: $v_i'=0$ (one-tail) significance tests shown below the coefficients ($\hat{v}_1', \cdots, \hat{v}_4'$), it may be observed that, since there are 56 degrees of freedom, the null hypotheses can be rejected with 95 per cent confidence in each of the four tests.

As far as the elasticity of adjustment estimate ($\hat{\gamma}$) is concerned, this amounts to the finding that the hypothesis of complete adjustment ($\gamma=1$) can be rejected. Indeed, we note from $\hat{\gamma}$ that in the course of a single year a 10 per cent change in the ratio between current desired capital-intensity and the previous existing capital-labor ratio leads to but a 2.8 per cent alteration in the ratio of the current actual capital-intensity to the previous existing capital-labor ratio.[29]

Given the considerable lag in the adjustment of the capital-labor ratio, the estimated long-run elasticity of substitution $\hat{\sigma}=.316$ is arrestingly small. Yet this should occasion little surprise:[30] it was anticipated that if the coefficient $(\widehat{\lambda_L-\lambda_K})$ was found to be significantly above zero, the estimate obtained for the elasticity of substitution would lie below the values that have been derived on the assumption of neutral technical change by Kendrick and Sato [19], and Kravis [21], i.e., $\hat{\sigma}=.58$ and $\hat{\sigma}=.64$, respectively. It may also be observed that the substitution parameter estimated from regression (II) is roughly half as large as the estimate provided by regression (I) in Part II.A, and is only a bit more than half that obtained with the same regression model by ACMS [3]. We are inclined to regard the latter comparisons as empirical support for the earlier argument that the usefulness of the regression procedure indicated by equation (9) is impaired by a mis-specification of the wage-rate variable—equivalent to the presence of errors of observation in that variable as employed in (I)—which produces upward-biased estimates of σ. The short-run elasticity estimate ($\hat{\sigma}_S=\hat{\sigma}\hat{\gamma}=0.088$) provided by regression (II) is truly minute, but it is nonetheless significantly greater than zero—as the statistical significance of $\hat{v}_1'$ implies.

Even as a lower bound, the estimate that appears above for ζ is also strikingly small; it suggests that a 1 per cent fall (or rise) in the employment rate is accompanied by only a 0.3 per cent drop (or rise) in the rate of utilization of the existing stock of capital. Moreover, if we were to inquire how far above zero the estimated value of δ would have to lie before it could be said that percentage changes in the rate of capital-utilization were equal to those in the employment rate—an assumption

[29] Although $\hat{\gamma}$ is low, it is significantly greater than zero; the t-statistic for $H_0: \gamma=0$ is $t=3.09$. Cf. Appendix B on possible downward bias in $\hat{\gamma}$.

[30] This estimate does not seem out of line with the results of several recent studies: Brown and De Cani [5] [6], Lucas [23], McKinnon [24]. However, the statistical procedures used in the foregoing studies may very well have led to more severe downward biases in the estimates of σ—for reasons cited in Appendix B—than the present study encountered.

sometimes made in aggregate production function analysis—the relationship given in (21d) implies that if $\zeta = 1$, δ is at the implausibly high level $\delta = 5.33$. For $\delta = 2.0$, which might be appropriate to the experience of the mid-1950's but seems to be still rather high for the 1899–1960 period as a whole, the corresponding estimate $\zeta = .5718$ remains well below unity.[31]

On first consideration, these findings might appear to fly in the face of the common observation that variations in the rate of employment in the U.S. economy are accompanied by *more than proportional* changes in measured rates of "capacity-utilization." However, the concept underlying reported rates of capacity-utilization is rather different from the concept of the rate of capital-utilization adopted here. The most familiar indices of capacity-utilization (and of excess capacity) are measures of the relationship between actual *output* and full-capacity *output*, the latter being defined either as maximum output obtainable under normal work scheduling or as that level of output observed during periods of peak production.[32] By contrast, full-capital-utilization, as it is defined here, refers to that flow of capital services yielded by the capital stocks of firms operating at optimum points on their cost curves; it is that flow which, with product and factor prices remaining unchanged, would occasion neither net investment nor disinvestment. Since our concept

[31] The statements regarding the plausibility of implied estimates of the parameter δ are based on the following considerations. From (21d) and (II) we have the relation: $\zeta = .1285\delta + 0.3153$. In other words, for $\zeta = 1$, a 1 per cent drop in the employment rate would mean that the ratio of the "normal," or long-run, to the actual relative shares would fall by more than 5 per cent. If (in the very short run) π^* were taken as constant, this means that the actual relative share of labor (π) would rise by 5 per cent, roughly, when the employment rate dropped by 1 per cent. The latter hardly appears to be reasonable in light of the short-period movements of the available data on factor shares and the employment rate. From the data described in Appendix C one can compute the following estimates of δ over three-year intervals of rising unemployment, on the strong assumption that π^* is constant within each of those short intervals:

Interval of Rising Unemployment Rate	Arc-Estimates of $\delta = -\frac{(1-U)}{\pi}\left(\frac{\Delta\pi}{\Delta(1-U)}\right)$
1919–22	1.2
1929–32	0.5
1955–58	1.9

The sample is obviously restricted, but from the above estimates and the general behavior of the data, it does seem difficult not to conclude that $\zeta = 1$ implies an estimate of δ ($= 5.33$) that is clearly too high.

[32] Cf. Phillips [28]. The technique of selecting peak production years as full-capacity-utilization reference points and assuming, in constructing measures of capacity-utilization for purposes of business-cycle analysis, that the relationship between the capital stock and output changes only very slowly, if at all, seems quite inadmissible in the context of the present study. Emphasis here is placed upon discovering the breadth of factor-substitution possibilities and the long-run rates of factor-augmentation.

of the rate of capital-utilization does not involve comparisons of actual and full-capacity *output*, there is no reason to suppose that the estimate of ζ should coincide with inferences based upon the observed relationship between fluctuations in the usual capacity-utilization measures and those in the rate of employment.[33]

The implication of the fractional upper- and lower-bound estimates of ζ, rough as they are, is that, over the course of the business cycle, firms collectively act to reduce the impact of demand fluctuations upon the rate of utilization of the existing capital stock, thus shifting the burden of adjustments to deficiencies in effective demand onto the labor force. This jibes with the inference one draws from Keynes's [20] discussion of the behavior of user cost over the cycle, and might therefore be interpreted as indirect support for specifying (19) in a manner in which the existence of idle capacity in the preceding period is allowed to raise the ratio of current actual capital-intensity to current desired capital-intensity. It should, of course, be recognized that the production model with which we are working strictly does not admit the possibility of departures from the full-capital-utilization optimum; idle capital, whose marginal productivity is zero, simply doesn't fit into the present theoretical framework. This may result in some downward bias in the estimate of ζ when the model is fitted to data generated by a world where capital sometimes stands idle.

Perhaps the most intriguing result provided by (II), and certainly the most novel finding, is the estimate obtained for $(\lambda_L - \lambda_K)$, the rate of bias in the growth of conventional input efficiencies. Discussion of this parameter estimate has, like all good things, been saved 'til last. The fact that θ_i' is positive and significantly greater than zero leads immediately to the conclusion that over the period 1899–1960 technical change has not been neutral, but has instead increased conventional labor-input efficiency more rapidly than the efficiency of conventional capital inputs. To restate the point in Hicks's [15] terminology (noting that $0 < \hat{\sigma} < 1$): *technical progress in the Private Domestic Sector of the U.S. economy has been labor-saving during the present century.* As for the mag-

[33] The short-run relation between changes in the measures of capacity-utilization and the employment rate could reflect variations in output per man employed with constant capital service inputs, due to changes in employment and the number of hours worked per man employed, assuming the possibility of varying factor proportions on old equipment. (Cf. Wilson and Eckstein [38].)

It may be noted that the small values implied for ζ *do not* result from the fact that the employment rate is defined in terms of persons employed, while the measure of (constant efficiency) labor input used in (II) is defined in terms of man-hours employed. Since the variance of an employment rate $(1-U')$ defined as the proportion of potential full-employment man-hours actually worked would be greater than that of the employment rate as it is conventionally defined, the regression coefficient of $\ln(\hat{K}/L)$ on $\ln(1-U')$ would be *smaller*, neglecting sign, than that obtained in (II); the implied estimates of ζ would, consequently, lie even further below unity than those we have found.

nitude of the differentially faster rate of labor-augmentation, the estimated rate is 0.72 per cent per annum; over the course of the six decades since 1900, the efficiency of labor has increased by roughly 54 per cent more than the efficiency of capital. These findings are sufficiently striking in their import to warrant their further consideration in Section III.

But, before examining the question of the apparent nonneutrality of efficiency growth more closely, it is necessary to enter an explicit caveat regarding the parameter estimates just discussed. The estimates offered here are, at best, tentative, for the estimation procedure has several potentially serious sources of bias. As the existence of a simultaneous-equations bias of unknown dimension has already been pointed out, it is not necessary to dwell longer on that unpleasant vision; two other econometric horrors must be confronted—lagged values of the dependent variables and serial correlation of the disturbance terms. A glance at (II) will suffice to confirm that both are indeed present, although the serial correlation of disturbances is not terribly severe ($d=1.252$) and is slightly less pronounced than in regression (I).

The conflicting effects of these sources of bias upon the parameter estimates receive some further consideration in Appendix B. While one will conclude from that discussion that nothing very concrete can be asserted about the existence, direction, and magnitude of over-all biases in the estimates derived from regression (II), it is found that the inclusion of the lagged dependent variable among the explanatory variables in (II) does not *in itself* result in the estimate of $(\lambda_L-\lambda_K)$ which supports the inference that technical change has been labor-saving.

III. *Time Paths of Labor and Capital Efficiency*

Despite the fact that technical change may be characterized by a fairly steady, persistent labor-saving bias, at least over the very long run, it is rather farfetched to imagine that either the magnitude or the direction of that bias will necessarily be the same in all short intervals of time or that such departures from the long-term trend as do occur will be of a random sort. Quite the contrary, if one thinks of technical change in terms of the introduction, extension, and diffusion of discrete innovations in organization and production techniques, and quite possibly also in the content of labor force training, one is dealing with a set of processes that are played out over time rather than being instantaneously completed. It might therefore be expected that the absorption of one major innovation (or a limited number) may well dominate the technological scene during an extended period of time; and that while this absorption is going on, the impact of such dominant innovations will impress itself upon the direction and rate of change of relative factor efficiency in the economy as a whole. With this as the paradigm of tech-

nical progress, departures from the long-run trend in relative factor efficiency (E_L/E_K) would be anomalous if they turned out to be serially independent rather than autocorrelated.[34]

It is therefore of some interest to inquire what the data and the parameter estimates obtained in (II) imply about the time path of E_L/E_K in the Private Domestic Sector of the U.S. economy during the present century. Answering this question by calculating the time profile of E_L/E_K proves to be a comparatively easy matter. From (5), (6), (18) and (19) we have—noting that $\rho = [(1-\sigma)/\sigma]$—the following expression:

$$(22) \qquad \hat{k}_t d_0 (1 - U)_t^{\zeta} = \left(\pi_t c_0 (1 - U)_t^{\zeta}\right)^{\gamma/\rho} \hat{k}_{t-1}^{(1-\gamma)} \left(\frac{E_L(0)}{E_K(0)} e^{(\lambda_L - \lambda_K)t}\right)^{\gamma},$$

which may be rewritten as,

$$(23) \qquad \left(\frac{E_L}{E_K}\right)_t = [\hat{k}_t^{1/\gamma} (\hat{k}_{t-1})^{(1-\gamma)/\gamma} \pi_t^{1/\rho} (1 - U)_t^{(\delta/\rho - \zeta/\gamma)}] c_0^{1/\rho} d_0^{1/\gamma}.$$

Now, making use of the relationships between the regression coefficients in (II) and the structural parameters of the model, E_L/E_K can be computed *in index form* from:

$$(24) \qquad \left(\frac{\widetilde{E_L}}{E_K}\right)_t = \frac{(E_L/E_K)_t}{(E_L/E_K)_0} = \left[\left(\frac{\hat{K}}{L}\right)_t \left(\frac{K}{L}\right)_{t-1}^{v_3'} \pi_t^{v_1'} (1 - U)_t^{v_4'}\right]^{1/(1-v_3')}.$$

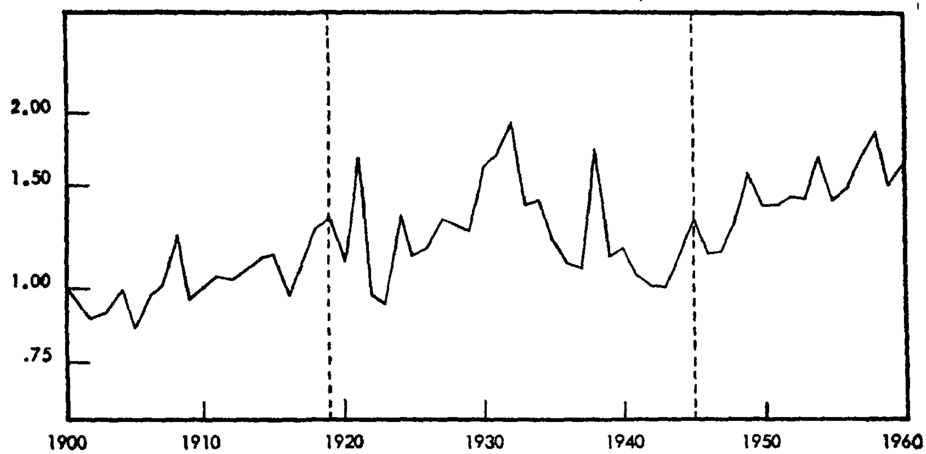
Applying the estimates $\hat{v}_1'$, $\hat{v}_3'$, and $\hat{v}_4'$ from (II) and the corresponding time-series observations, equation (24) generates the time path of $\widetilde{E_L/E_K}$ plotted on semilogarithmic scales in Chart 1.

Quite apart from the sawtooth short-period movements of the index that appear in the Chart, as a consequence of having forced the production function to account for the data exactly rather than stochastically, the time path of $\widetilde{E_L/E_K}$ reveals the deficiencies of the assumption of a constant rate of bias in the direction of technical change.[35] Indeed,

[34] These observations have some implications for the econometric difficulties encountered in estimating the model considered here, particularly the problem of autocorrelation in the disturbance terms. Although artificial statistical contrivance may be employed in an effort to render residuals serially independent, it does seem sensible to view the element of serial correlation that persists in the residuals of regression (II) as the consequence of the incomplete or incorrect specification of the regression model. (Cf. [12, pp. 71–72].) Thus regarded, the problem of serial correlation in the disturbances of (II) may be ultimately intractable, for one of the most likely sources of mis-specification that comes to mind is the assumption of a constant geometric rate of bias in the growth of factor efficiencies, and upon that assumption the entire approach pursued here must rely.

[35] It should be clear that the computed index of E_L/E_K simply represents the estimated trend values (given by $e^{.0072t}$) plus the residuals of regression (II). The pronounced short-run

CHART 1. INDEX OF RELATIVE LABOR EFFICIENCY (E_L/E_K)
1900=1.00



visual inspection of Chart 1 suggests that in the United States the six decades since 1899 might be thought of as encompassing three major periods, each characterized by a rather different experience with regard to the relative growth of labor and capital efficiency:

(a) 1900–1918, in which labor-saving technical changes took place more rapidly than the long-term trend rate of bias (0.72 per cent per annum);

(b) 1919–45—a longer interval over whose entire course no significant labor- or capital-saving bias emerged, despite marked internal changes of some duration in the direction of movements in $\widetilde{E_L/E_K}$;

(c) 1946–60, the postwar period, during which the rise in relative labor efficiency was resumed at a rate even faster than that experienced prior to 1919.

Any serious effort to account for the appearance of such alterations in the drift of "technical change" or to relate them to other economic events distinguishing the three periods clearly lies beyond the scope of the present study. It may, nonetheless, be observed that the decline in the level of $\widetilde{E_L/E_K}$ during the latter part of the second of these periods

movements of the residuals around the trend line probably result in part from errors of measurement in the capital-labor ratios, i.e., in the dependent variable of the regression. So long as we retain the basic assumptions that the elasticity of substitution is constant, that there are constant returns to scale, and that markets are free from monopoly elements, the more sustained departures of E_L/E_K from its trend line in Chart 1 are attributable to the unsteady character of technical progress. Were one prone to discard the foregoing basic assumptions of the analysis, other possible explanations would spring to mind: changes in the elasticity of substitution, changes in the degree of monopoly in product and factor markets, etc.

(i.e., during 1933–43) may well be a reflection of a sustained rise in the rate of utilization of the existing stock of capital, brought about initially by the slow recovery from the depression of the 1930's and then extended by the pressure upon available capacity during World War II. The reversal of this movement and the extremely rapid rise of $\widetilde{E_L/E_K}$ in Chart 1 during the late 1940's would, on this line of explanation, reflect the return to lower normal rates of capital-utilization following the war. This is, however, no more than speculation intended to provoke further inquiry.

Although a direct estimate of the (constant) long-run disparity between the rate of growth of labor efficiency and the rate of growth of capital efficiency has been secured from regression (II)—as well as an alternative (larger) estimate of $(\lambda_L \widehat{-} \lambda_K)$ from regression (III) in Appendix B—nothing has been said about the magnitudes of the actual rates of labor- and capital-augmentation over the period 1899–1960. Yet, answers to the question of just how rapidly labor efficiency and capital efficiency have grown, and an assessment of the importance of the contributions made to the rate of growth of conventional total factor productivity, lie within reach.

Going back to equation (1a), and noting that α and β may be replaced by the shares of labor and capital in output, respectively, we have the relationship

$$(25) \qquad \pi_L \frac{\dot{E}_L}{E_L} + (1 - \pi_L) \frac{\dot{E}_K}{E_K} = \left(\frac{\dot{V}}{V} - \frac{\dot{L}}{L}\right) - (1 - \pi_L)\left(\frac{\dot{K}}{K} - \frac{\dot{L}}{L}\right)$$

which, recalling the definition of $\lambda \equiv \dot{E}/E \equiv \alpha\lambda_L + \beta\lambda_K$, is equivalent to

$$(25a) \qquad \lambda = \left(\frac{\dot{V}}{V} - \frac{\dot{L}}{L}\right) - (1 - \pi_L)\left(\frac{\dot{K}}{K} - \frac{\dot{L}}{L}\right).$$

In addition, from the definition of λ, it is known that

$$(25b) \qquad \lambda_K = \lambda - \pi_L(\lambda_L - \lambda_K).$$

More sophisticated alternatives are available, but it is sufficient for the present purpose to proceed by estimating the long-term rate of total conventional input efficiency growth (λ) from the estimated trend rates of growth of V, K, and L in the Private Domestic Economy during the period 1899–1960, using the arithmetic average of labor's share in that interval as the weight for the $[(\dot{K}/K) - (\dot{L}/L)]$ term in (25a).[36] The estimate previously obtained for $(\lambda_L \widehat{-} \lambda_K)$ can then be used with $\hat{\lambda}$ and $\bar{\pi}_L$

[36] It is not necessary to correct the existing capital stock ($\hat{K}$) data for variations in the rate of utilization, since the rate of exponential growth of $\hat{K}$ is obtained by regressing observations of the natural logarithm of $\hat{K}$ on time for the entire long period 1899–1960. See Appendix C for regression equations used to obtain estimates of $(\dot{V}/V)$, $(\dot{K}/K)$, and $(\dot{L}/L)$.

TABLE 1—ESTIMATES OF AVERAGE ANNUAL COMPOUND RATES OF GROWTH OF FACTOR EFFICIENCIES, U.S. PRIVATE DOMESTIC ECONOMY, 1899–1960
(in percentages)

Trend Estimates of:		(Average) Level of:	Estimate from (25a) of:	Implied Estimates from (25b) of:		Relative "Contribution" of λ_L:
$\frac{\dot{V}}{V} - \frac{\dot{L}}{L}$	$\frac{\dot{K}}{K} - \frac{\dot{L}}{L}$	$\bar{\pi}_L$	$\hat{\lambda}$	$\hat{\lambda}_L$	$\hat{\lambda}_K$	$\bar{\pi}_L(\hat{\lambda}_L/\hat{\lambda})100$
A. Regression Model II Estimate $(\lambda_L \widehat{-} \lambda_K) = 0.0072$						
2.38	1.01	.476[a]	1.85	2.23	1.51	57.4
2.38	1.01	.751[b]	2.13	2.30	1.58	81.0
B. Regression Model III Estimate $(\lambda_L \widehat{-} \lambda_K) = 0.0086$						
2.38	1.01	.476[a]	1.85	2.30	1.44	59.1
2.38	1.01	.751[b]	2.13	2.34	1.48	82.5

[a] Employee compensation in Gross (Private) Business Product, Appendix B.
[b] Labor's share in National Income, average for 1899, 1919, 1929, 1937, 1948, and 1957, from Kendrick [18, Table 28, p. 179].

to calculate $\hat{\lambda}_K$ from (25b), and, therefore, to compute $\hat{\lambda}_L$. The results of the computations just described are presented in Table 1.

Since the average level of labor's share is quite sensitive to variations in the definition of payments for labor services and the inclusiveness of the aggregate output measure, these computations are affected by the particular concept of labor's share that is employed.[37] Panel A of Table 1 therefore presents two sets of factor-efficiency growth rate estimates; these have been computed using the estimate $(\lambda_L \widehat{-} \lambda_K) = 0.0072$ from regression model (II) and alternative averages for labor's share over the 1899–1960 period. The first line shows the results obtained with the share concept adopted in the present study—essentially, employee compensation exclusive of entrepreneurial income as a proportion of gross private business product. The second line shows the same calculation with the much higher average value of labor's share in *national income*, defined to include an estimate of the wage component of entrepreneurial incomes; this definition and measure of labor's share are adopted by Kendrick [18, Tables 25, 28, pp. 112, 179]. It should be observed that the estimates of $\hat{\lambda}_L$ and $\hat{\lambda}_K$ are really quite

[37] Cf. Kravis [21] and Grant [11] for a recent survey of alternative measures of labor's share and a discussion of the problems of consistent measurement. Cf. also notes in Appendix C. We are concerned here only with the mean level of π_L, implicitly basing this concern on the observation that differences in the definition of π_L produce greater alterations in the average levels of the shares than in their long-run growth rates. Marked differences between trends in alternative factor share measures would lead to regression estimates of $(\lambda_L - \lambda_K)$ other than those found here.

insensitive to the very marked difference between the average values used for $\bar{\pi}_L$, although, as would be expected, the estimate of λ the rate of growth of total (conventional) input productivity is more profoundly affected.

Alternative estimates, showing the effects of the same variation in the magnitude of the average share going to labor, are also given in Panel B of Table 1, where the calculation is repeated using the higher estimate of $(\lambda_L - \lambda_K)$ obtained with regression model (III). Comparison of $\hat{\lambda}_L$ and $\hat{\lambda}_K$ values in Panel B with those in Panel A permits an assessment of their rather low degree of sensitivity to possible bias in the estimate of $(\widehat{\lambda_L - \lambda_K})$ accepted from regression (II), upon which the computations in Panel A are based.

The rates of growth in Table 1 speak fairly well for themselves. Focusing on the first line in Panel A, it is found that the efficiency of labor has grown at an annual rate of approximately 2.2 per cent per year, and the efficiency of capital has increased at an annual rate of roughly 1.5 per cent.[38] The estimated percentage rate of growth of (weighted) total factor efficiency is 1.85 per cent per annum; it is strikingly close to the 1.83 per cent per annum estimate provided for the total productivity growth rate during 1909–49 by ACMS [3] and agrees fairly well with the findings of other comparable studies.[39] There is a touch of irony here: since it has been found that technical change *is not neutral*, the 1.83 per cent estimate presented by ACMS is, strictly speaking, not an estimate of λ, but of λ_L, and regarded as such, it appears to fall rather to the low side of the truth. (Cf., above, Section II.A.) Further, when one uses a figure for $\bar{\pi}_L$ that corresponds to the more usual labor-share concept adopted by the other aggregate productivity studies cited, i.e., $\bar{\pi}_L = .75$, the second line of Table 1 shows that the resulting estimate for λ would be 2.1 per cent per annum—a good bit higher than the general run of long-run total productivity growth rates found in those studies!

The last column of Table 1 offers the results of a computation designed to provide an answer to the question of the importance of the contribution made to the rate of total factor efficiency growth (λ) by the annual rate of increase in the efficiency of labor. It is evident that the particular way one defines labor's share can make a considerable difference here. With $\bar{\pi}_L = .476$ it appears that something less than 60 per cent of the rate of growth of factor productivity is accounted for by labor-augmenting technical changes, whereas with $\bar{\pi}_L = .751$ it would seem that

[38] The direct estimate furnished for λ_L by regression model (I) is, by contrast, 1.9 per cent per annum. It will be recalled that we argued in Section II.A that the latter estimate was likely to be subject to a downward bias.

[39] Kendrick [18, Table 25, p. 113] gives total factor productivity growth rates for the Private Domestic Economy which average out to 1.77 per cent per annum for the period 1899–1957. Cf. also, Abramovitz [1, p. 11] and Solow [33, p. 316].

labor-augmentation accounts for roughly 80 per cent of the annual total productivity growth rate. Although it can be said with reasonable assurance that more than half of the "residual," or economically unexplained, component of the output growth rate of the Private Domestic Economy has come in the form of labor-augmenting improvements, the spread between 57 per cent and 81 per cent (compare Panel A) seems too wide to be very useful in guiding research into the sources of aggregate productivity growth. In the latter connection it would certainly be better to rely on the estimates of λ_L and λ_K instead; to work towards identification of the secular developments which have been responsible for something like a 2.2 or 2.3 per cent annual rate of increase in the efficiency of conventionally measured labor inputs on the one hand, and a 1.5 or 1.6 per cent annual rate of growth of the efficiency of conventionally measured capital inputs on the other hand.

Of course, if one were tempted by the thought that the search for the sources of productivity growth could be called off completely, simply by defining the inputs in the aggregate production function in terms of efficiency units and thereby doing away with the very notion of total productivity change, it could be said that "capital inputs"—$(E_K K)$ in the production function (1)—have been growing at the rate of approximately 3.23 per cent per annum, while "labor inputs" $(E_L L)$ have grown at 2.98 per cent per annum. Since the annual rate of growth of real output in the Private Domestic Economy has been about 3.04 per cent during the present century, one is left with the following very simple characterization of the long-run pattern of growth: there has been a tendency for output per unit of "labor input" to rise slightly in consequence of the increasing "capital"-intensity of the aggregate production process, while, on the other side of the coin, the increasing relative abundance of "capital" and the rather restricted scope for input substitution have led to a moderate rise in the share of real output received by labor.

Whatever the virtues of simplification, this vignette unfortunately brings us no closer to fully understanding the mechanism underlying the growth of effective labor inputs and the rising relative abundance of effective capital inputs in the United States; but, as was announced at the outset, such questions are quite distinct from those we have attempted to answer here and we are content for the moment to leave them to others.

Appendix A: Correcting the Nominal Capital Stock for Variations in the Rate of Utilization

Capital stock data can be corrected for changes in the rate of utilization prior to its use in the regression analysis by assuming that the rate of utilization is equal to the rate of employment. This procedure was adopted by

Solow [31]. When this device is used, in place of (II) we obtain:

$$\text{(IIA)}\quad \ln\left(\frac{\hat{K}(1-U)}{L}\right)_t = 4.0256 + \underset{(2.31)}{0.1886} \ln\left(\frac{\pi_L}{\pi_K}\right)_t + \underset{(5.49)}{0.0051t} + \underset{(4.94)}{0.3484} \ln\left(\frac{\hat{K}}{L}\right)_{t-1}, \quad R = .963,\ d = .376.$$

The implied parameter estimates, compared to those derived from (II), show a higher elasticity of adjustment and, therefore, a lower elasticity of substitution $\hat{\sigma}=0.22$. The estimated bias toward labor-saving in technical change is somewhat more pronounced: $(\lambda_L \widehat{-} \lambda_K)=0.0078$, compared to 0.0072 found with (II). These estimates are not in conflict with the general conclusions of the text based on (II), but the proportion of the total variance explained by (IIA) is not as large as that explained in (II) and, more important, despite the use of a lagged dependent-variable serial correlation of the disturbance terms in (IIA) is very pronounced—indicating some serious specification error in the model.

It is sometimes argued (cf., e.g., [34], [4]) that the correction for underutilization of capital should be made in a more flexible manner than that used in (IIA), on the grounds that when the rate of unemployment is already low, further reductions in U will have a smaller influence on the rate of utilization of capital. On this argument the effective or utilized capital stock (K) could be related to the nominal stock as: $K=\hat{K}e^{-(nU+nU^2)}$. Substitution of this expression in place of the specification given by equation (18) leads to the fitted regression equation:

$$\text{(IIB)}\quad \ln\left(\frac{\hat{K}}{L}\right)_t = -\,3.6223 + \underset{(1.90)}{0.1294} \ln\left(\frac{\pi_L}{\pi_K}\right)_t + \underset{(2.11)}{0.0020t} + \underset{(7.99)}{0.7216} \ln\left(\frac{\hat{K}}{L}\right)_{t-1} + \underset{(0.07)}{0.0027U_t} + \underset{(0.25)}{0.0004U_t^2}; \quad R = .980,\quad d = 1.255.$$

While there is virtually no difference between the estimates of σ and $(\lambda_L-\lambda_K)$ obtained with (IIB) and those secured with (II), as a consequence of the greater degree of multicollinearity among the explanatory variables in (IIB), the standard errors of the regression coefficients are greater, and the t-statistics shown in parentheses below the coefficients are in every case smaller than those given by (II). Moreover, neither the regression coefficient of U nor that of U^2 in (IIB) is significantly different from zero. Thus, compared to (II), (IIB) fails to offer an improvement in the reliability of the results.

It was argued in the text (Section II.B) that application of the specification of the lagged adjustment process given by (12) to the capital-labor ratio adjusted for underutilization in the preceding period, as well as in the current period, creates some statistical problems which make the adjustment specification given by (19) an attractive alternative. For the satisfaction of the curious, we present the outcome of taking the route suggested by (12). Using the form for the underutilization correction given by (18) in conjunction with (12) leads to the addition of a term $[\hat{v}_5' \ln(1-U)_{t-1}]$ to the RHS of (21). Fitting this amended model yields, in place of (II):

$$\text{(IIC)} \quad \ln\left(\frac{\hat{K}}{L}\right)_t = -1.9624 - \underset{(-1.08)}{0.0379} \ln\left(\frac{\pi_L}{\pi_K}\right)_t + \underset{(1.57)}{0.0007t}$$

$$+ \underset{(20.47)}{0.9741} \ln\left(\frac{\hat{K}}{L}\right)_{t-1} - \underset{(-13.11)}{1.1349}[\ln(1-U)_t]$$

$$+ \underset{(13.37)}{1.2569} \ln(1-U)_{t-1}; \qquad R = .995, \quad d = 2.321.$$

As noted in the text, multicollinearity is quite serious in this case. In addition, the coefficients of the first two independent variables turn out not to be significantly different from zero, and the estimate of the elasticity of adjustment (γ) from $(1-\hat{v}_3')=.0259$ is implausibly low.

Appendix B: Parameter Estimation Biases

Hurwicz [16] has shown that for small sample sizes the classical least-squares estimate of the regression coefficient of a lagged dependent variable, such as $\hat{v}_3'$ in (II), will be subject to a downward bias. Although the 61 observations used in fitting the model do not constitute a "large sample" as these things go, a sample of this size does tend to lessen the problem of extreme bias of this sort.[40] Yet this affords but cold comfort: Griliches [12] shows that if there is serial correlation in the disturbances, least-squares estimates of the coefficient of a lagged endogenous variable ($\hat{v}_3'$ again) will be biased even in the case of large samples. In contrast to the case considered by Hurwicz, the bias in $\hat{v}_3'$ will be upwards when the disturbances are autocorrelated. What must be hoped for, then, is that the opposing small sample and autocorrelation biases, whose magnitudes we do not know, tend to cancel each other. If, however, the net result is that an upward bias persists in $\hat{v}_3'$, the estimate $\hat{\gamma}$ will be biased downward; σ will,

[40] Cf. McKinnon [24]. This consideration constitutes an argument against following the lead provided by Brown and De Cani [5] [6], who subdivided a roughly equivalent number of time series observations into groups corresponding to comparatively short 'epochs' and then ran separate regressions for these groups to allow for secular variations in the elasticity of substitution. Despite the fact that the above authors work with a distributed-lag specification to obtain estimates of the long-run elasticity of substitution, they fail to note the effect that breaking their observations into shorter series has in this connection.

therefore, have been overestimated and $(\lambda_L \hat{-} \lambda_K)$ will also be biased upward. As far as the strength of the inferences drawn from (II) regarding the nonneutrality of technical change is concerned, it would clearly be comforting to believe that if the biases arising from the Hurwicz and Griliches cases failed to cancel out, the effect of downward bias due to small sample size dominated. The estimate of γ would then be upward biased and $(\lambda_L \hat{-} \lambda_K) = 0.0072$, would lie below the true parameter value.

Although nothing definite can be said about the actual size of the biases that may exist in the estimates derived from (II), it is possible to provide some indication that the inclusion of the lagged dependent variable ($\ln \hat{k}_{t-1}$) in (II) does not *in itself* produce an estimate of $(\lambda_L - \lambda_K)$ which is biased upward, i.e., one which favors the conclusion that technical change is labor-saving. If it is assumed that the elasticity of adjustment defined in (19), i.e., γ, is unity, the worrisome lagged dependent variable drops out of regression model (21). Fitting the resulting lagless version of (21) with the same data as was employed in (II) yields the following regression equation:

$$\text{(III)} \quad \ln\left(\frac{\hat{K}}{L}\right)_t = -10.5476 + \underset{(2.05)}{0.1942} \ln\left(\frac{\pi_L}{1-\pi_L}\right)_t + \underset{(12.01)}{0.0086t}$$

$$\underset{(-9.37)}{-1.0882} \ln(1-U)_t; \qquad R = .958, \quad d = 0.194.$$

Denoting the vector of regression coefficients, in order of their appearance in (III) by $(v_0'', \cdots, v_3'')$, the estimates of the relevant production-function parameters are $(\lambda_L \hat{-} \lambda_K) = v_2'' = .0086$, and $\hat{\sigma} = v_1''/(1+v_1'') = .1626$—the latter being interpreted as the elasticity of substitution under the assumption of complete adjustment within a single year. In the absence of simultaneous-equations bias,[41] the estimate $v_2'' = .0086$ should be unbiased. We note, therefore, that it is *larger* than the corresponding estimate $(\lambda_L \hat{-} \lambda_K) = .0072$, obtained with the distributed-lag model (II).

As a final comment in this cautionary vein, it should be remarked that the presence of some serial correlation in the disturbances of (II) makes it likely that application of the usual least-squares formula in computing the sampling variances of the regression coefficients leads to underestimates of the standard errors, jeopardizing the strict validity of the usual t-tests. (Cf., e.g., Johnston [17, pp. 179 ff].) Yet, in contrast with the very high degree of autocorrelation of disturbances in (III), where $d = .194$ and the t-statistics given in parentheses below the regression coefficients appear spuriously large, this problem is much less serious in the case of (II). The latter constitutes a clear advantage of the inclusion of the lagged dependent variable in (II), but it is an advantage gained, as has been seen, at the cost of possible biases in the parameter estimates.

[41] Lucas [23], by application of two-stage least squares to the estimation of the elasticity of substitution, has found the simultaneous-equations bias not to be very serious, but his results relate to the case of a single industry.

Appendix C: Sources of the Data

1. ***$\hat{K}$: Unweighted Real Capital Input in the U.S. Private Domestic Economy (PDE), in 1929 Dollar Millions.***

a. Data for 1899–1953 from Kendrick [18, Table A-XXII: Supplement, pp. 336–37 and Table A-XV, p. 321]. Extensions for 1954–60 based on revisions and further data kindly supplied by J. W. Kendrick and Maude Pech. The deflation of gross capital formation data, from which these real capital stock figures are derived [18, p. 35] is intended to provide a base-period resource cost measure of capital in which increases in productive efficiency of comparable items of the stock are not reflected unless more resources are used in their production. However, in cumulating real capital formation figures into stock estimates, Kendrick has adopted a real stock concept *net of depreciation allowances* (rather than gross of depreciation and net of replacements) as a "better measure of a basic capacity to contribute to production." (*Ibid.*) Since the latter allows for obsolescence in the measure of capital, it conflicts with the stated intent of the deflation operation and creates an ambiguity as to the precise meaning of the figures.

b. Exponential trend rate of growth in $\hat{K}$, estimated from:

$$\ln \tilde{K}_t = 3.9807 + \underset{(31.3)}{0.0167t}; \qquad R = .971$$

2. *L: Unweighted Input of Labor in the U.S. PDE, in Millions of Man-hours Employed.*

a. Data for 1899–1953 from Kendrick [18, Table A-XXII, pp. 33–34 and Table A-XII, p. 315]. Extensions for 1954–60 based on same source as those made for capital.

b. Exponential trend rate of growth in L, estimated from:

$$\ln L_t = 4.2936 + \underset{(10.9)}{0.0066t}; \qquad R = .816$$

3. *V: Real Gross Private Domestic Product, in 1929 Dollar Millions.*

a. Data for 1899–1953 from Kendrick [18, Table A-XXII, pp. 333–34 and Table A-III, Col. (7), p. 299]. Extensions for 1954–60 based on same source as those for capital.

b. Exponential trend rate of growth in V estimated from:

$$\ln V_t = 3.6174 + \underset{(38.2)}{0.0304t}; \qquad R = .980$$

4. *Labor's Share: Employee Compensation as a Proportion of U.S. Gross Private Business Product.*

a. Data for 1929–60 from U.S. Department of Commerce, *U.S. Income and Output* Supplement to the *Survey of Current Business*, (Washington, D.C.: U.S. GPO, 1958, Table I-12, p. 134). Payments to labor include compensation of employees in corporate business (line 14), sole proprietorships

and partnerships (line 16), other private business (line 26). Wage payments to employees in general government, in government business, in households and institutions are omitted and no allowance is included for wage payments in proprietors' incomes. Data for 1899–1928 from extrapolation of later series on the share of employee compensation in Gross Private Domestic Business Product computed from Grant [11, Table 2, Col. (1) and Table 3, p. 279] for 1899–1929. The two series are virtually identical in the overlap year, 1929.

b. Apart from the exclusion of wage payments to workers in the government sector, the major difference between the numerator of the labor share measure used here and the more conventional measures lies in the exclusion of any imputation of entrepreneurial income to labor. As Lebergott [22, pp. 190–219] points out in a critical survey of the controversy over the stability of labor's share, virtually any attempt to split up entrepreneurial income between labor service and capital service payments will necessarily be arbitrary and, for the early years of the twentieth century, will rest on extremely treacherous data. Lebergott suggests that studies of factor substitution be limited, therefore, to those sectors of the economy in which entrepreneurial income is insignificant. Acceptable as this recommendation is, continuous time series for aggregate inputs of labor and capital in the private nonentrepreneurial sector of the economy are not yet readily available for analysis of the kind pursued here.

As for the denominator in the share measure, Grant [11] has advanced persuasive reasons in support of his contention that use of the Commerce national-income concept poses problems of inconsistencies among the components of the denominator which distort the picture of the movements in income distribution over time. He also points out that distortions have been introduced by the arbitrary reconciliation of national income estimates for the pre-1929 period with the Commerce concept. In preparing estimates of Gross Private Domestic Business Product as an alternative and preferable denominator, Grant takes Kendrick's [18] annual figures for GNP on a Commerce basis as a starting point and proceeds by deducting "irrelevant" nonbusiness items.

5. *U: Proportion of the U.S. Civilian Labor Force Unemployed.*
Data for 1899–1960 from Lebergott [22, Tables A-3 and A-15].

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CONSUMER'S SURPLUS AND THE COMPENSATION PRINCIPLE

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Rarely in the formulation of economic theory has so much thought been devoted to refinements of minor importance as in the development of the concept of consumer's surplus. I venture to add yet another article in the hope that it might enable us to reconcile Marshallian simplicity with the purist's insistence on precision, and at the same time to supplement the compensation principle with a workable guide to policy in cases where compensation will not be paid.

The new interpretation of consumer's surplus, which I have called consumer's gain, is developed in Section I. Section II clarifies and extends some recent developments in the compensation principle. Section III concludes with a criterion for policy.

I. *Consumer's Surplus and Consumer's Gain*

The very term "consumer's surplus" is now somewhat ambiguous as we already have four precise, yet different, measurements of related concepts, two of them stemming from attempts to measure precisely what A. Marshall defined as consumer's surplus and two of them from attempts to measure the concept which J. R. Hicks at first thought Marshall was trying to measure [3, p. 96 n. 2]. These four different interpretations have been rigorously analyzed by Hicks [1] [3].

J. Dupuit [4, p. 39] originated the concept, and unwittingly the confusion, when he stated, *"l'économie politique doit prendre pour mesure de l'utilité d'un objet le sacrifice maximum que chaque consommateur serait disposé à faire pour se le procurer,"* and measured "consumer's surplus" as the area under the demand curve and above the price line. Marshall's contribution, besides popularizing the idea, was to point out that the area in question represented the definition only if the marginal utility of money is constant. Marshall was content to let the matter rest there as he rightly regarded the degree of inaccuracy involved when the marginal utility of money is not constant to be normally of negligible importance [10, pp. 692-93]. Hicks pointed out that, when we drop the assumption of a constant marginal utility of money, "what ceases to hold is the equivalence between the consumer's surplus and the area of the Marshallian triangle; a correction has to

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be introduced to overcome the discrepancy" [2, p. 109]. But the discrepancy was not between the *consumer's gain* (as I shall call the basic concept) and the triangle, but between Marshall's definition and the triangle. One could either correct the triangle or the definition. Hicks chose to correct the triangle to fit the definition, and also changed the definition, but not in such a way that it would fit the triangle. I aim to show that, by changing the definition and retaining the triangle, we not only retain geometric simplicity but also have a completely accurate interpretation of the consumer's gain.

A. *Hicks's Measures*

Hicks's four methods of measuring the consumer's gain or loss from a fall or rise in the price of one commodity are all in terms of amounts of money which, if taken from or given to the consumer, would have the effect of offsetting or substituting for the gain or loss. The problem, however, is that the amount of money which would offset a gain or loss is not the same thing as the gain or loss itself. The differences between the four measures result from the different circumstances under which the money adjustment is made. In ascending order of size for the case of a price fall, they are [3, p. 99]:

The compensating surplus, or the amount of money which the consumer would have to lose, after committing himself to the purchase of that amount which he would choose to purchase after the price fall if no adjustment to his income were made, in order to leave him just as well off as he was before the price fall.

The compensating variation, or the amount of money which the consumer would have to lose, before committing himself to the amount of his purchases after the price fall, in order to leave him just as well off as before the price fall.

The equivalent variation, or the amount of money which the consumer would have to gain in the absence of the price fall, before committing himself to the amount of his purchases, in order to make him as well off as he would be with the price fall and no adjustment in income.

The equivalent surplus, or the amount of money which the consumer would have to gain in the absence of the price fall, after committing himself to the purchase of that amount which he would choose to purchase in the absence of either price fall or money adjustment, in order to leave him just as well off as he would be with no money adjustment after the price fall.

Each of these concepts has its significance, and is accurately measured by Hicks's methods, where we want to measure how much would be necessary to compensate a person for the loss arising from a price

increase or restriction in the quantity of his purchases, or how much a person would pay for the privilege of a price reduction or increased quantity. This is the basis of the compensation principle. But when compensation will not actually take place, these measurements are only close approximations to the gains or losses which will continue to accrue to consumers.

B. *The Variations and the Gain*

To show the difference between the size of the consumer's gain and the loss of income which would just offset that gain I shall consider

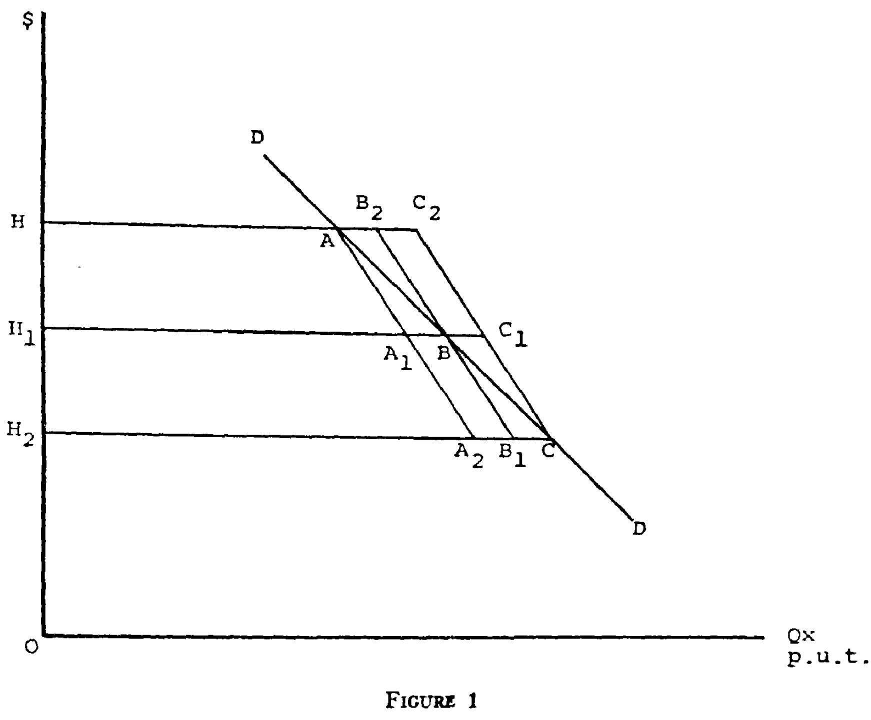


FIGURE 1

first the case of the compensating variation for a price fall. On Figure 1 we begin at price OH at which the consumer purchases HA, A being a point on the uncompensated demand curve DD. When price falls to OH_1, the compensating variation is measured by HAA_1H_1, being the area bounded by the compensated demand curve AA_1A_2 [1] [3, Ch. 8]. If the price now falls to OH_2, the size of the compensating variation for this second price fall depends upon whether the compensating variation for the first price fall was actually taken from the consumer or not. If the first compensating variation was deducted, the consumer would be at A_1, a position indifferent to the initial position A. If price

then falls from OH_1 to OH_2, the compensating variation for this price fall is $H_1A_1A_2H_2$, giving a total compensating variation for the fall in price from OH to OH_2 of HAA_2H_2. However, if the compensating variation for the price fall from OH to OH_1 is not deducted, the consumer will follow the uncompensated demand curve rather than the compensated demand curve and purchase H_1B at price OH_1. If the price now falls from OH_1 to OH_2, the compensating variation of the second price fall would be measured by the area under the compensated demand curve through B, i.e., $H_1BB_1H_2$. Thus we see that while the area HAA_2H_2 accurately measures the compensating variation of the fall in price from OH to OH_2 *if the variation is actually deducted,* it underestimates the extent of the consumer's gain if the variation is not deducted. By working through a price between OH and OH_1 it can similarly be shown that the compensating variation HAA_1H_1 underestimates the consumer's gain resulting from the fall in price from OH to OH_1. If we regard the price fall as composed of a series of many small price falls, we find the consumer's gain to be measured by the area under the uncompensated demand curve DD. It is not really surprising that if we assume the compensating variation to be applied continuously so as to offset consumer's gain, we should measure it by the area under the compensated demand curve, whereas if we assume that no compensating variation is actually made, the consumer's gain is measured by the area under the uncompensated demand curve; but it is interesting to note that we have returned to the simple Marshallian measure.

The attempt to equate consumer's gain with the adjustment in income which would be necessary to offset it, while originating in a desire to allow for the changing marginal utility of money, actually involves a discrepancy because of a failure to allow fully for the changing marginal value of money. The change in the marginal value of money to the consumer when the price of one commodity which he buys falls is the result of two opposing forces. On the one hand the price fall increases his real income, and this results in a fall in the marginal utility of the goods which money will buy. On the other hand the price fall raises the purchasing power of money. The measurement of the compensating variation assumes that the variation applies at the same time as the change in price, so that the consumer is never any better or worse off. But if the compensating variation is not deducted, its value to the consumer rises by reason of the increase in the purchasing power of money which accompanies the price fall. Thus if in Figure 1 we regard all curves as linear and consider the simple two-stage price fall, the difference between the size of the compensating variation of the second price fall if the first is deducted, and its size

if the first is not deducted, is the area $A_1BB_1A_2$, which is simply the cost difference on the amount A_1B, or the additional saving on those units which would be purchased only if the compensating variation of the first price fall from OH to OH_1 is not deducted.

The consumer's gain which results from a price fall is due to the change in the purchasing power of his income, and he has a further gain from the change in the purchasing power of the compensating variation if this is not deducted. The gain to the consumer if he keeps the gain is therefore greater than the amount which would have to be taken from him to offset it. The difference between them is the triangle ACA_2. This will normally be an insignificant amount in practice, but since Hicks has taken the pains to deduct it from the Marshallian triangle to arrive at the compensating variation, we are surely justified in adding it to the compensating variation to arrive at the consumer's gain.

In just the same way that the compensating variation for a price fall is less than the consumer's gain, so the equivalent variation is more than the consumer's gain. Hicks has shown [3, p. 81] that the equivalent variation for a price fall is the same as the compensating variation for a price rise, and it is both simpler and more meaningful to show that the compensating variation for a price rise overestimates the consumer's loss than that the equivalent variation for a price fall overestimates the gain. If price rises from OH_2 to OH_1, the compensating variation is $H_1C_1CH_2$. If this is paid to the consumer and price then rises from OH_1 to OH, the compensating variation for the second price rise is $HC_2C_1H_1$, making a total compensating variation for the rise in price from OH_2 to OH of HC_2CH_2. However, if the first compensating variation is not paid, the consumer moves to B at price OH_1, and the compensating variation for the price rise from OH_1 to OH is only HB_2BH_1. Thus just as the consumer's gain from a price fall, if the variation is not deducted, exceeds the compensating variation because the variation itself increases in purchasing power as the price falls, so the compensating variation for a price rise, if it is added to the consumer's income, exceeds the consumer's loss, because he then has to be compensated for the loss in purchasing power of the variation itself. While the compensating variation for a price rise exceeds that for a price fall, the consumer's loss from a price rise equals his gain from a price fall if neither is compensated for, the gain or loss being measured simply by the area under the uncompensated demand curve.

We are now in a position to inquire into the relationships among the compensating variation, the equivalent variation, and the consumer's gain in the case of a price fall. To simplify this we can adopt Hicks's assumption that the two compensated demand curves AA_2 and

C_2C are linear and parallel and that the uncompensated demand curve is linear, i.e., that the figure AC_2CA_2 is a parallelogram bisected by the uncompensated demand curve DD. The consumer's gain is now exactly halfway between the compensating variation and the equivalent variation. A and A_2 both lie on indifference level 1, and C_2 and C on indifference level 2. The area of the parallelogram, which is the difference between the compensating and equivalent variations, is the cost difference resulting from the price change on that additional quantity of the commodity, AC_2 or A_2C, which is purchased at the higher indifference level but not at the lower indifference level. The equivalent variation includes this amount, whereas the compensating variation does not. Thus, although the slopes of the compensated demand curves now allow for the substitution effect, we still have to face the choice between the inner and outer cost differences. When measuring the consumer's gain in the absence of income variations, we cannot include the whole of the cost difference on the amount AC_2, since he was not willing to purchase this amount before the price fall. On the last unit at C he gains no surplus since he is only just prepared to buy this unit at the new price and indifference level. To include the whole of the cost difference on all the units between A and C_2 (or A_2 and C), as is done by the equivalent variation, clearly overestimates the consumer's gain. But to exclude the cost difference on these units altogether, as is done by the compensating variation, errs on the side of underestimating consumer's gain, since in the case of the first unit at A he gains effectively the whole of the cost difference. The consumer's gain must include the sum of the cost differences on each unit of the quantity AC_2 between the highest price at which he is just persuaded to acquire it and the price which he actually pays after the price fall, since he gains only by cost reductions below the maximum price he would pay. This of course is the amount measured by the area under the uncompensated demand curve.

Hicks has shown that the change in quantity between A and A_2 or C_2 and C is the substitution effect, and the change between A and C_2 or A_2 and C the income effect. To measure consumer's surplus for a price fall by the equivalent variation assumes that the income effect precedes the substitution effect, while to measure it by the compensating variation assumes that the substitution effect precedes the income effect. I have discussed elsewhere [16] the problem associated with assuming these effects to be consecutive when in fact they are necessarily simultaneous. In this case of course it is perfectly justifiable to regard them as consecutive when measuring the equivalent and compensating variations since these are defined as changes in income which would precede (or act instead of) and follow the substitution

effect caused by the price change. But if we are trying to measure the consumer's gain in the absence of income variations, we cannot consider the income and substitution effects to be consecutive in either order; they operate simultaneously as the price falls. The only way to measure the effect of their simultaneous operation is to follow them together over the range of the price change, which is exactly what the uncompensated demand curve does.

C. *The Changing Marginal Utility of Money*

The problems associated with expressing a change in utility in money terms arise where the utility of money is not constant. Marshall avoided the difficulty by assuming the *MUm* to be constant. Hicks attempted to overcome the problem where *MUm* is not constant by measuring the equivalent and compensating variations in terms of the value of money before and after the price change. Use of the Marshallian triangle when the *MUm* is not constant involves measurement in money, the marginal utility of which changes in the course of the measurement.[1] While there are pitfalls in using units of measurement, money, which do not have a constant relationship to the thing being measured, utility, there must also be objections to using any money of constant utility to measure changes in a case where the utility of money is not in fact constant. That Hicks's measurements are valid for his definitions is attributable not to the money used in his measurements having constant utility, but to the fact that the changes in the marginal utility of money in the course of the measurement are the same as those that would arise in the postulated changes in income being measured. That Hicks's measurements are not in money of constant marginal utility can be shown by the following case.

Assume that the consumer's income elasticity of demand for a good, x, is positive, that the consumer's price elasticity of demand for x is unitary, and that the utilities derived from the consumption of x and other goods are mutually independent. If the price of x falls, the same amount of money will be spent on x, and the quantities of other goods purchased will not change. Since other prices do not change, the marginal utility of the last unit of money spent on each of the other goods does not change. The marginal utility of money must be the same for all goods purchased, both before and after the price change, and the marginal utility of money to the consumer has remained constant, even though the income elasticity of demand for x is positive. The necessary condition for use of the Marshallian triangle is satisfied, but the income effect of a compensating or equivalent variation would not be

[1] I am indebted to J. R. Hicks for focusing my attention on this point.

zero, and the Hicksian measurements would not coincide with the Marshallian, even though one of these accurately measures what Marshall defined as consumer's surplus. The Marshallian measure is in terms of money of constant marginal utility, its significance being that if the consumer's surplus is $1, then the consumer has gained an increment in utility equal to 100 times the utility derived from the last cent of his income either before or after the price change. But this increment in utility is more than he would get from the addition of $1 to his income in the absence of the price fall, since, with a positive income elasticity of demand, the marginal utility of the last cent of the dollar would be less than that of the first cent. The equivalent variation takes account of this and would be more than one dollar. Similarly the compensating variation would be less than one dollar. Neither of these variations is in terms of money of constant marginal utility, yet both are meaningful measurements of the extent to which the consumer is better off as a result of the price fall.

In the above case the increment in utility can be expressed either in terms of money of constant marginal utility or in money of nonconstant marginal utility; both are valid and significant, though their meanings are different. In a case where the price elasticity of demand is not unity, Hicks's measurements remain equally valid, since they are not dependent on money having constant marginal utility. The expressions in terms of money of constant marginal utility are still possible, though there are now two of these based respectively on the marginal utility of money before and after the price change, and neither can be measured by the Marshallian triangle. The triangle still measures the concept of consumer's gain developed above, but is no longer in terms of money of constant marginal utility. Neither the Hicksian measurements nor the Marshallian triangle are in money of constant marginal utility, but the reason for the variaions in *MUm* differ.

The marginal utility of money when all other prices are constant can be regarded as a function of either money income and *Px* or real income and *Px,* since real income is itself a function of money income and *Px.* Hicks's compensating variation can be thought of in either of two ways. If price falls in a series of small stages and the variation in money income is applied at each stage (the compensating variation being the sum of these), then real income remains constant, but money income and *Px* vary, and so does the marginal utility of money. If the compensating variation is measured and applied after the complete price fall, then *Px* is constant throughout the variation, but money income and real income vary, and so does the marginal utility of money. The consumer's gain measured by the Marshallian triangle is the sum of the series of compensating variations applicable to a series of small

price falls, on the assumption that the variations are not applied. Money income is constant, but real income and Px vary. Thus in each case one of the three variables is held constant while the other two vary, and the marginal utility of money varies through the course of the measurement. Hicks's equivalent variation differs from the compensating variation because both real income and Px change as a result of the price fall, and the measurement can be based on either value.

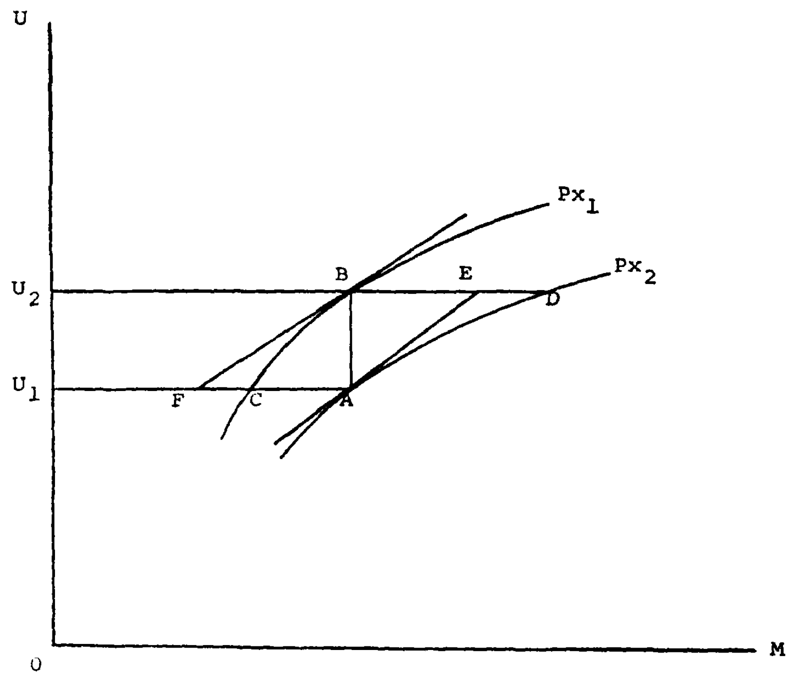


FIGURE 2

Since money income does not in fact change, there is only one value for the consumer's gain.

D. *Illustration of the Measurements*

The meanings of the various measurements are illustrated by Figures 2 and 3. Each curve in Figure 2 shows total utility as a function of money income for a particular price of x. Px_1 is lower than Px_2, and prices of other goods are assumed constant. When price falls from Px_2 to Px_1, the consumer moves from A to B and rises from utility level U_1 to U_2. The problem is to convert this change in utility level to money terms when the consumer's money income does not change. The same data are rearranged in Figure 3, where each curve represents the combinations of money income and Px which yield the consumer the same

utility. Again the price fall from Px_2 to Px_1 moves the consumer from A to B and from U_1 to U_2.

The measurements in terms of money of constant marginal utility can easily be read from Figure 2. The *MUm* before the price change is the slope of the curve Px_2 at A. Converting the entire utility increment AB to money at this *MUm* gives the measurement BE. If we base our measurement on the *MUm* after the price change, we must use the slope of curve Px_1 at B which yields the measurement AF.

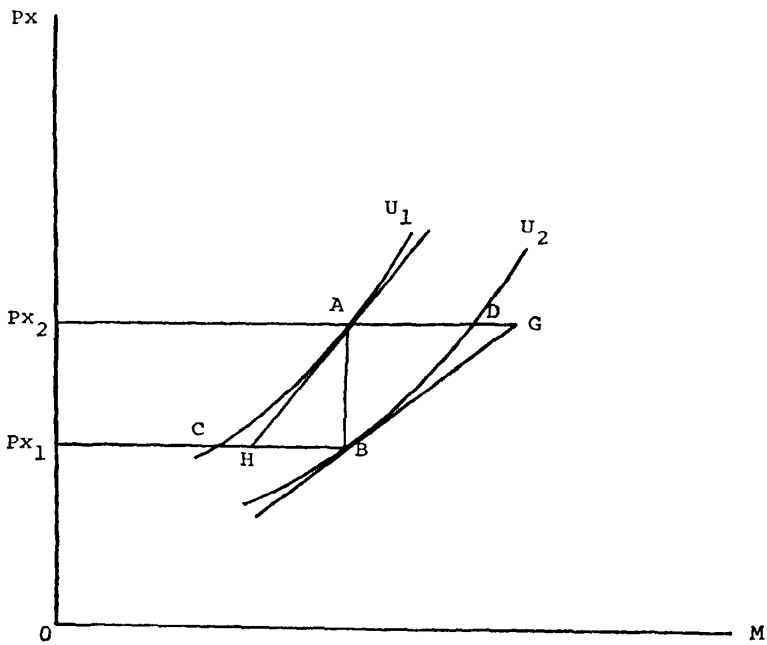


FIGURE 3

Both of these are in terms of money of constant marginal utility, but neither is of much practical significance. The analogous procedure on Figure 3 would be to convert the price change directly to money by holding constant the marginal rate of substitution between money and *Px*. This marginal rate of substitution is given by the slope of curve U_1 at A before the price fall, and the slope of curve U_2 at B after the price fall. The results are BH and AG, respectively, and are again without practical significance.

Hicks's techniques of measuring the compensating and equivalent variations, and the measurement of the consumer's gain, can be shown on either figure. On Figure 2 we start at the initial position A and hold *Px* constant at Px_2. The increment in utility to U_2 now necessitates a

movement along the curve Px_2 to D, which could be achieved by an addition to money income of BD. This is Hicks's equivalent variation and can be thought of either as a direct movement from B to D holding utility constant at U_2 or as the sum of a series of small utility increments caused by a series of small price changes each converted to money at the appropriate slope of the curve Px_2 as we move from A to D. The compensating variation is similarly measured by holding Px constant at Px_1 and moving from B to C along curve Px_1, converting each small utility increment to money at the appropriate slope of Px_1 and summing the series. The result is AC, which could alternatively be thought of as a direct movement from A to C by holding utility constant at U_1 while price falls from Px_2 to Px_1. By following AD or BC, the equivalent and compensating variations sum series of converted utility increments along paths which the consumer does not in fact follow. The consumer's gain sums the series of small utility increments, each converted to money at the slope of the appropriate curve between Px_2 and Px_1 where it cuts AB. In following AB rather than AD or BC, the consumer's gain follows the path which the consumer does in fact follow. The result cannot be read directly on the money axis, since there is no change in money income, but it can be read easily as the area of the Marshallian triangle under the demand curve. In a case where the slopes of Px_2 at A, Px_1 at B, and all intermediate curves along AB are equal (i.e., where the MUm is constant over the price change), then AF would equal BE, which would equal the consumer's gain, and there would be no problem about use of the Marshallian technique; but the equivalent variation BD would be greater than BE, and the compensating variation AC would be smaller than AF.

The same results can be achieved by use of Figure 3, without the necessity for a cardinal utility axis. Here the series of price changes are converted to money terms directly at the marginal rates of substitution of money for Px, which are shown as the slopes of the indifference curves. If we follow AC, holding utility constant at U_1, the result is BC, which is the compensating variation. If we follow BD, the result is AD, which is the equivalent variation. And if we follow AB, the path which the consumer does in fact follow, the result is the consumer's gain.

The above techniques enable us to define the precise meaning of the Marshallian triangle, and also point out the relative advantages of the different measurements. While the compensating and equivalent variations are easily read from the above figures and are expressed as variations in income which are readily understandable, they involve measurements over paths which the consumer does not in fact follow. The

:onsumer's gain is measured over the path which the consumer does in 'act follow, and is the only measurement which can be taken directly 'rom the demand curve, but the result has no familiar meaning. Since ve are trying to express in money terms a change which by definition loes not involve a change in money income, it is not really surprising hat the result is not familiar.

E. *The Welfare Significance of Consumers' Surplus*

The concept of consumer's surplus, by itself, is of little value in wel-are theory, since any policy change affects large numbers of individ-ıals. Aggregation of changes in consumer's surplus involves interper-onal comparisons, while the welfare significance of any net change nvolves a value judgment. The original version of the compensation ırinciple, now generally referred to as the Kaldor-Hicks criterion, vhen applied to measurements of Hicks's compensating variations, .nder the assumption that full compensation will in fact be made, is ıniquely free from either interpersonal comparison or value judgment. 'he compensating variation measures not changes in satisfaction, but mounts of money necessary to offset those changes. Under the as-umption that all changes will be offset, the only question is whether he amount of money collected from gainers will be enough to compen-ate losers; all comparisons are in hard, homogeneous dollars. It is nly when the possibility, or desirability, of compensation is ques-ioned that problems arise. Then some people might be worse off, and : becomes necessary to compare changes in satisfaction. At this stage oth the Scitovsky criterion and a value judgment become relevant, ut their development has obscured the fact that the Hicksian mea-ırements do not measure changes in satisfaction, they measure hanges in money income necessary to offset those changes. When the ain or loss will continue to accrue to the gainer or loser, its expression ı money terms is more appropriately made by the Marshallian mea-ıre.

Use of the Marshallian triangle to measure consumer's gain or loss oes not overcome problems of aggregation and comparison, however. dollar's worth of satisfaction to the gainer is not the same amount satisfaction in any objective sense as a dollar's worth of satisfaction a loser. The important comparison, however, is not between nounts of private satisfaction, but between the social valuations of ıose amounts. A rich honest man might get less satisfaction from his st few dollars than a poor dishonest man, but society might still gard the distribution as optimal, for value judgments involve the eth-s of deserts. A thief might derive great private satisfaction from his -gotten gains, while the social valuation of this satisfaction is nega-ve. I shall use the term "utility" with reference to private satisfac-

tions and "welfare" with reference to the value placed on these satisfactions by society. The distinction is important because attention has been focused in the literature on interpersonal comparisons of private satisfactions, while the real issue involves value judgments about the relative values of those satisfactions to society, for it is the economic welfare of society which welfare economics attempts to maximize. The value judgments on distribution introduced by Little, and applied below to a criterion based on the measurement of consumer's gain, should be interpreted in that sense. To say that redistribution is good means that society attaches greater value to a dollar's worth of utility gained by a gainer than to a dollar's worth lost to a loser. To say that redistribution is bad means the converse, while to say that society is indifferent about the redistribution in question means that it attaches equal importance to the amount of utility gained by a gainer and measured by the gain criterion as one dollar, and to that possibly different amount lost by a loser but also measured by our criterion as one dollar. In the case of indifference, straight aggregation of gains and losses is valid for our purposes. If redistribution is good, straight aggregation underestimates gains relative to losses, while if it is bad, straight aggregation overestimates gains relative to losses. Thus if measurements of gains and losses by our criterion are aggregated and show a net gain in utility, and the redistribution involved is considered good, there is a gain of welfare greater than that indicated by the gain in utility, and the policy change would be a good thing. Conversely, if the criterion shows a net loss of utility and redistribution is bad, the loss of utility underestimates the loss of welfare, and the policy would be a bad thing. Only in cases where there is a net gain in utility but redistribution is bad, or a net loss but redistribution is good, is the inference in doubt. Then it would be necessary to quantify the redistribution by a judgment as to the relative weights to be attached to gains and losses of utility in converting them to gains or losses in welfare, so that the direction of the change in welfare could be determined. Assessing such weights would be difficult, but they would be called for only in some cases, and their very necessity would itself indicate the desirability of a change in distribution policy.

With these limitations of the aggregation process in mind, and with the above interpretation attaching to the value judgment which identifies the direction of any error involved in aggregation, we are now ready to examine the compensation principle.

II. *The Compensation Principle*

We need not concern ourselves with the early development of the compensation principle for this has been admirably summarized by I. M. D. Little [7]. The *Critique* provides an excellent springboard for

our purposes because in a few pages it achieved three things: a summary of the various criteria previously developed by Hicks, N. Kaldor, and T. Scitovsky; the introduction of an explicit value judgment on the desirability of income redistribution; and the clear separation of the assumptions that compensation either is or is not possible. Little's treatment of the criteria has proved to be somewhat controversial, however, and before basing my case for the consumer's gain upon it, I must first correct what appears to me to be an inconsistency in Little's analysis, and secondly extend that analysis to cases of weak ordering.

A. *Criticisms of Little's Analysis*

There is widespread, though not universal,[2] agreement that if everyone is made better off by a policy it is a good thing and if it results in all persons being worse off it is a bad thing. The problem arises where a policy makes some people better off and some worse off. To say that distribution would be improved as a result of a policy change which moved us from Q_1 to Q_2 (Figure 4) means, according to Little, "that a point on the utility possibility curve of Q_1, which is either south-west or north-east of Q_2, is better than Q_1" [7, p. 103] [8, p. 229 n. 2].

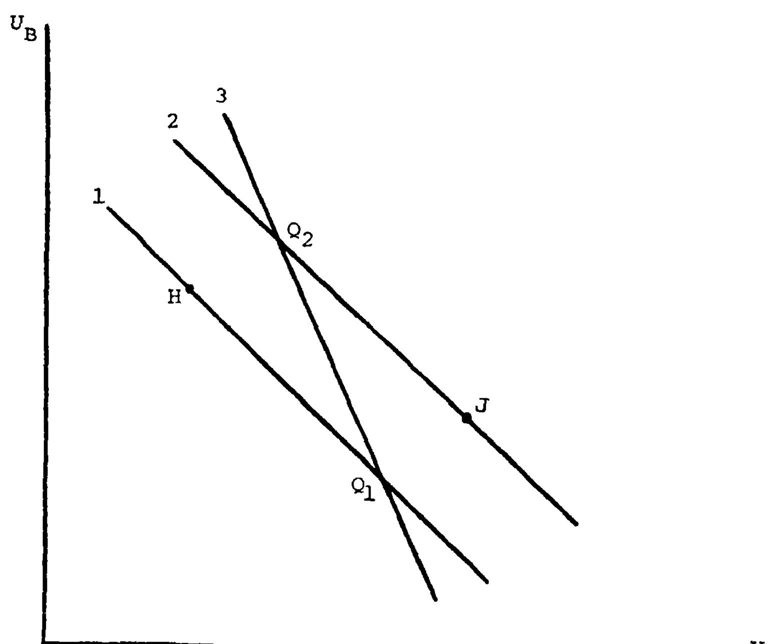


FIGURE 4a

[2] Mishan [13, p. 242] would add to the statement "if everyone is made better off" the proviso "without making the distribution of welfare less equal."

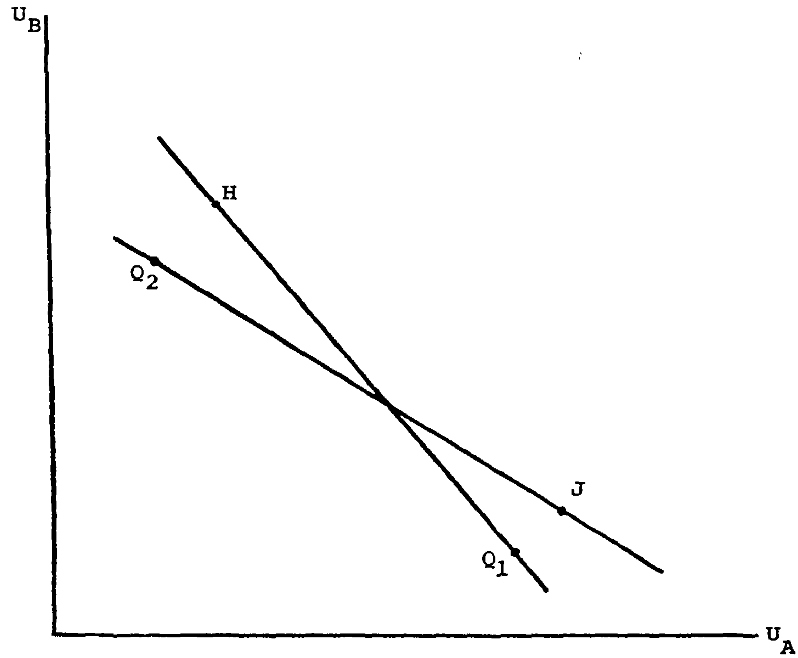


FIGURE 4b

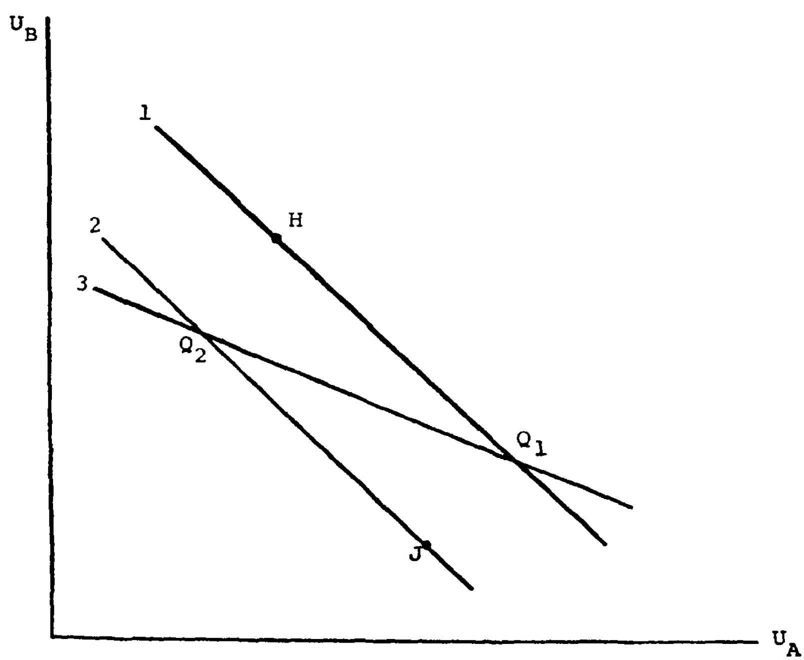


FIGURE 4c

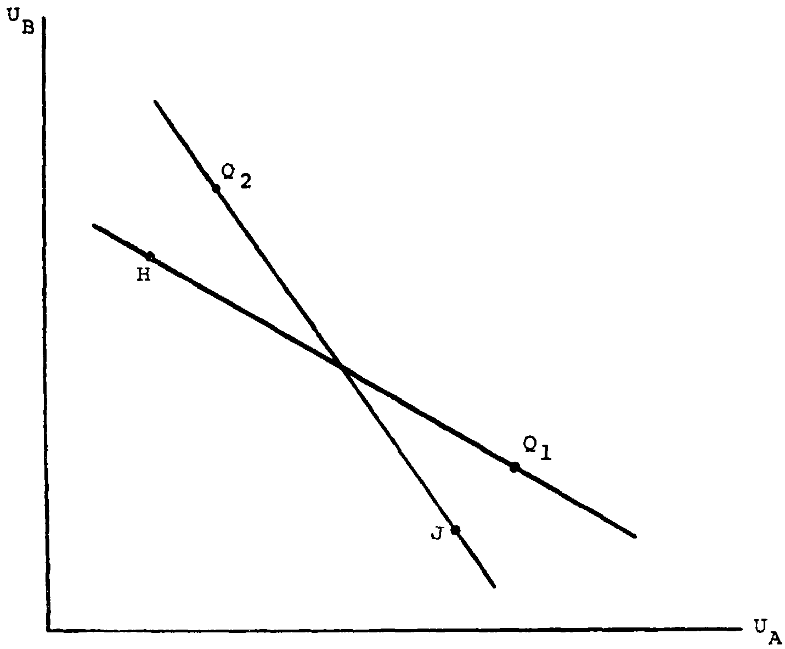


FIGURE 4d

It could alternatively mean that a point on the utility possibility curve of Q_2, which is either southwest or northeast of Q_1, is worse than Q_2. Little maintains that no conflict could arise between these meanings and says: "For consistency, it is required that if H is better than Q_1, then J is worse than Q_2" [7, p. 102]. I shall dispute this requirement below, but first I shall show that, if Little assumes this, his conclusions do not follow.

Figure 4 is adapted from Little's Figure VII [7, p. 104]. Curves 1 and 2 of Figure 4a depict Little's case 2 [7, p. 105] in which a policy change moves us from Q_1 to Q_2. The Kaldor-Hicks and Scitovsky criteria are both satisfied, but redistribution is bad. Under the assumption that compensation is impossible, Little concluded that the criteria give no guidance on whether the policy change is good, i.e., we have no basis on which to order Q_1 and Q_2. However, if we imagine an alternative policy which would yield utility possibility curve 3, passing through Q_1 and Q_2, this case is determinate. Such a policy is not necessarily impossible for it is the case depicted in 4b, and it does not matter whether such a policy is possible in fact or not; we are using it only as a hypothetical link in our chain of reasoning. Now if Little is correct when he maintains that if Q_1 is preferred to H, then J is preferred to Q_2, then it must also be true that if the policy yielding curve

3 were implemented, distribution Q_1 would be preferred to Q_2, for Q_1 is the point J on curve 3. Positions Q_1 and Q_2 are points, and there is no welfare difference between Q_2 on curve 2 and Q_2 on curve 3, nor between Q_1 on curve 1 and Q_1 on curve 3. If Q_1 is preferred to Q_2 for the curve 3, then the same ordering will be true for these points however they are achieved. That the policy giving rise to curve 3 is hypothetical is unimportant, for the available policies can yield either point Q_1 or Q_2. Thus, although Little's criteria apparently result in indeterminacy, his consistency requirement on distributional preferences enables us to say that the policy change should not be adopted. We can similarly show that Little's case 6 [7, p. 105] is determinate by reference to Figure 4c. In this case neither the Kaldor-Hicks nor the Scitovsky criterion is satisfied, but the redistribution is assumed to be good, i.e., H is preferred to Q_1, Q_2 is preferred to J, and therefore, by Little's consistency requirement, Q_2 (on curve 3) is preferred to Q_1. Accordingly the policy should be implemented to yield Q_2 on curve 2.

If we substitute these solutions for the question marks in Little's Table II [7, p. 105] we find that under assumption A (no redistribution possible) the value judgment on redistribution alone is enough to determine whether a policy change is good or bad, and the Kaldor-Hicks and Scitovsky criteria are irrelevant. To say that a policy should be adopted if it results in a favorable redistribution of utilities, no matter what it does to the levels of utility, is not wrong for it follows logically from Little's premises, but it is unacceptable.

B. *The Recent Controversy*

Considerable debate has been aroused by Little's judgment on distribution, and it is necessary to consider this at some length, partly because of the unsatisfactory state in which the matter currently rests, and partly because of its importance for my conclusions. The controversy can be grouped around three items of dispute: (1) whether one can meaningfully say anything about the distribution of utilities without simultaneously considering the levels of utility; (2) whether it can be consistent to prefer a redistribution in one direction under one set of circumstances, and in the opposite direction in a different set of circumstances; (3) whether one's preferences on distribution need be strongly ordered, or whether weak ordering is enough. Since my conclusions on these issues do not appear to conform entirely with those of any of the disputants to date, they must be examined in turn.

1. There appear at present to be three sides on this question, and I fear that I might be adding a fourth. The first group appears to maintain that one can express a preference about distribution which will hold regardless of the levels of income involved. Little must be re-

garded as belonging to this group in view of his statement that "it is required that if H is better than Q_1, then J is worse than Q_2" [7, p. 102], which he applies to cases where Q_2 and J represent unequivocally higher, and therefore different, levels of utility than H and Q_1. E. J. Mishan clearly belongs to this group and is dogmatic where others appear unprejudiced in maintaining that not only must a judgment on distribution be made, but that it should be a preference for equality. He accepts[8] C. Kennedy's criterion (iii) that "there is a social improvement if the distribution of welfare is better and everyone is not made worse off" [6, p. 339] and would even sanction a movement which reduced the wealth of the rich without increasing the wealth of the poor. That this judgment on distribution is unaffected by the levels of utility involved is shown by his statement, "I would substitute *more equal* distribution for better distribution" [14, p. 349].

The second side is J. E. Meade's, who says, "In crude and inaccurate language we cannot separate the consideration of the 'distribution' and the 'size' of real income, but must weight the two together in a single one-part criterion" [11, p. 127]. This would appear to be an extreme view that not only can one not talk about distribution without considering size, but one must treat both aspects of the problem in one criterion. I suspect, however, that Meade might regard the word *must* as one of the inaccuracies of this statement, in which case I would agree with him.

It is difficult to classify Kennedy's [5] [6, p. 338] views on this issue, and I therefore regard him as constituting a third side. Suffice it to say that he regards the simultaneous consideration of distribution and size as possible, simpler, and adequate, and therefore prefers it to an attempt to separate them.

My own view is that one can usefully distinguish two criteria, but the nature of the judgment on distribution might well be affected by the levels of utility involved both before and after redistribution, and one cannot therefore consider redistribution in isolation from the question exactly what redistribution of exactly what. To say that the directional preference on distribution might depend on circumstances raises the possibility of preferring redistribution in different directions in different circumstances, which brings us to item 2.

2. Little's consistency requirement has already been cited. Meade objected to this in his review [11, p. 127] and said, "Whether or not there is any sense in the unqualified statement that in general one prefers distributions of the kind Q_2 to those of the kind Q_1 is not clear to the present reviewer; but it is clear to him that one might well

[8] Mishan corrects "everyone is not worse off" to "not everyone is worse off" [14, p. 348]. It appears from the context that Kennedy would accept this.

say that one prefers H' to Q_1 (if a lump sum transfer from A to B would make B much better off but A only a little worse off) while one prefers Q_1 to H (if the same lump sum transfer would make B only slightly better off at the cost of making A much worse off)." D. H. Robertson [15, p. 228] accused Meade of being "in a very queer frame of mind" to say this "from a pure distributional point of view," which is surprising since Meade had maintained in the same paragraph that it is impossible to take a purely distributional point of view. It is unfortunate that Meade expressed his crucial point in what he later admitted to be a careless way [12, p. 232], for his restatement of it has not had the impact which it deserves. He says [12, p. 232], "There is nothing to prevent one simultaneously *both* preferring the point J to the point Q_2 . . . *and also* preferring the point H . . . to the point Q_1." Little agreed [8, p. 230] that Meade's earlier statement was "both true and irrelevant," but Meade's second statement showed that while it was irrelevant *under* Little's definition, it was highly pertinent as a criticism *of* that definition. Although Little made a rejoinder [9, pp. 233-34] to Meade's reply, he did not answer this criticism. Mishan accepted Meade's statement as correct [14, p. 347 n. 1] but felt he had adequately allowed for it in qualifying his criteria to exclude the possibly intransitive consequences. It is unfortunate in view of the apparently universal agreement with Meade that no one has treated Meade's point as a way of extending Little's analysis. Instead it has been regarded as an awkward case calling for qualification to the criteria. Such an extension is made in Table 1 below, but, since this includes weak ordering, I shall first discuss the dispute over strong and weak ordering of distributional preferences.

3. A preference for strong ordering is very common, but this is usually due not to any theoretical objection to weak ordering, but to the belief that strong ordering is simpler and less subject to intransitive consequences. The great advantage of weak ordering is that it is comprehensive and includes cases of indifference between two distributions. In many cases where policy changes are considered, governments are aware of the fact that there will be redistributional effects, but regard these as neither good nor bad, per se. Since this attitude is quite common, a complete theory should allow for it.

C. *Little's Matrix Revised*

The matrix in Table 1 adopts Little's technique of permutating the possible criteria and uses Little's diagrams [7, pp. 104-5], but the assumptions are different. It allows for the fact that the direction of the distributional preference might depend upon the levels of utility, and on the relative extents of the gains and losses involved; that pref-

TABLE 1

Case No.:	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
Criteria:																		
Kaldor-Hicks criterion satisfied?	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Scitovsky criterion satisfied?	Y	Y	Y	Y	Y	Y	Y	Y	Y	N	N	N	N	N	N	N	N	N
Q_1: *H*	<	>	<	>	=	=	=	>	<	<	>	<	>	=	=	=	>	<
Q_2: *J*	>	<	<	>	=	>	<	=	=	>	<	<	>	=	>	<	=	=
Inference: should the change be made?																		
A. Redistribution impossible	Y	?	Y	Y	Y	Y	Y	Y	Y	Y	N	?	*	*	*	N	*	Y
B. Redistribution possible	Y	Y	Y	Y	Y	Y	Y	Y	Y	N	Y	?	*	*	*	Y	*	N

erences for redistribution in opposite directions might be made in different circumstances; and that positions can be weak-ordered. The choice of these assumptions rests on a principle which I regard as fundamental to any welfare theory, but which rarely receives explicit mention.

It is now generally recognized that it is impossible to reach any conclusions in welfare economics without involving value judgments, but that it is not the economist's job to make value judgments for society. Some defeatists have argued that since any recommendation will involve a value judgment, one might just as well make the complete decision as one value judgment and there is no point in developing any theoretical criteria. Others share my belief that criteria can offer useful guidance to the government making value judgments, but the criteria cannot circumvent the need for value judgments. But to develop any criteria it is necessary to constrain the value judgments which society can make. This is an unwelcome task for the economist for to constrain a value judgment is to make a value judgment. Thus, while it is necessary to constrain society's value judgments, the aim of the economist should be to minimize those constraints. The failing of most writ-

Case No.:	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36
Criteria:																		
Kaldor-Hicks criterion satisfied?	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N	N
Scitovsky citerion satisfied?	N	N	N	N	N	N	N	N	N	Y	Y	Y	Y	Y	Y	Y	Y	Y
$Q_1: H$	>	<	<	>	=	=	=	>	<	>	<	<	>	=	=	=	>	<
$Q_2: J$	<	>	<	>	=	>	<	=	=	<	>	<	>	=	>	<	=	=
Inference: Should the change be made?																		
A. Redistribution impossible	N	?	N	N	N	N	N	N	N	N	Y	*	?	*	Y	*	N	*
B. Redistribution possible	N	N	N	N	N	N	N	N	N	N	Y	*	?	*	Y	*	N	*

ers on welfare theory is that they do not make explicit the constraints which they impose. My constraints are as follows, and I believe they will meet widespread acceptance.

1. The nature of any value judgment shall not change in the course of the analysis.

2. Value judgments shall be consistent in this respect: if a movement from Q_1 to H involves making A worse off and B better off, and either Q_1 is indifferent to H, or H is preferred to Q_1; then a movement from J to Q_2, which involves making A worse off by a lesser amount and B better off by a greater amount, shall be regarded as a good thing. This constraint is not intended to imply cardinal measurement of utility and is therefore relevant only to Figures 4b and 4d. In Figure 4d, for example, Q_2 is northeast of H, and Q_1 is northeast of J. The loss to A in moving from J to Q_2 is therefore less than his loss in moving from Q_1 to H even on ordinal axes, for Q_2 and J both lie within H and Q_1 on the U_A axis. Similarly the gain to B in moving from J to Q_2 is greater than his gain in moving from Q_1 to H. If, then, H is preferred to or indifferent to Q_1, Q_2 must be preferred to J. This con-

straint is all that is needed to avoid intransitivity with weak ordering.[4]

3. Society shall prefer a position in which all persons are better off, provided that the distribution of utility is not made worse.[5]

These three constraints are all necessary, unless other constraints which involve these and more are made, but they are sufficient for our criteria to be rigorous and transitive. The complexity of much of the debate on this topic has arisen from an attempt to build the constraints into the criteria instead of expressing them separately. The matrix of Table 1 appears to me to be the clearest way of laying out the possibilities. With fewer constraints the possible value judgments become more numerous, but not unmanageable. Separate judgments are needed between the distributions at Q_1 and H, and between those at Q_2 and J; and each of these can be a preference either way or indifference. Thirty-six cases are now possible, where Little had only eight; but eight of the 36 are intransitive and are ruled out by the consistency constraint (2) above. These cases are indicated by an asterisk.

The cases are laid out in Table 1; the symbols $>$, $<$, or $=$ in row Q_1: H meaning Q_1 preferred to H, H preferred to Q_1, and Q_1 and H indifferent, respectively. Cases 1-9 are illustrated by Figure 4a, cases 10-18 by 4b, cases 19-27 by 4c, and 28-36 by 4d. Cases 1, 2, 10, 11, 19, 20, 28, and 29 are the same as Little's cases 1 to 8, respectively [7, p. 105], but cases 2 and 20 are indeterminate under assumption A (no redistribution possible) because Little's consistency requirement by use of which they were shown to be determinate above has been relaxed.

III. *Consumer's Gain as a Welfare Criterion*

With the aid of this matrix of cases, it is now possible to establish the contention made in Section I of this article, namely that the consumer's gain (or loss) as measured by the Marshallian technique is more appropriate than Hicks's compensating and equivalent variations, in cases where compensation will not in fact be paid. Henceforth I shall therefore be concerned entirely with Little's assumption A (no redistribution possible), for I do not question the validity of the Hicksian measurements under the assumption that compensation actually will be paid.

[4] I must disagree with Kennedy when he said, "Given strong distributional ordering, still necessary to remove the possibility of contradiction . . ." [6, p. 339].

[5] This is Kennedy's value judgement (i″) [6, p. 339]. It is an improvement on the Paretian criterion in being a lesser constraint on society's value judgments. To make this lesser criterion sufficient, however, it is necessary to define H as being not merely southwest or northeast of Q_2, but within those segments of the southwest and northeast quadrants in which distribution is considered to be no worse than at Q_2. J is similarly defined with respect to Q_1.

The superiority of the Marshallian over the Hicksian measuremen rests on three grounds: it is simpler, in many cases both would giv the same result, and in those cases where the two measurements woul give different results the Marshallian will give the correct answe: That the Marshallian measurement of the area under the deman curve is simpler to apply is obvious, for it is easier to estimate a de mand curve than either a compensated demand curve or indifferenc curves; and in actual cases where many persons are involved, the ma ket demand curve is more readily estimated than the correspondin Hicksian concepts. That the two methods will often give the same re sults relies on the assumption that the differences between the compe sating variation, equivalent variation, and consumer's gain will ofte be small, and accordingly rarely a deciding factor. That in cases (conflict the consumer's gain gives the correct answer is more difficu to prove, and is best established by consideration of such a case, a ca which I shall call the *nibble paradox* for reasons which will become a parent below.

A. *The Nibble Paradox*

This case is most easily shown by use of the Kaldor-Hicks and Sc tovsky criteria, overlooking the value judgment on distribution, and easily depicted by the compensated and uncompensated demand curv used in the first part of this article. It will then be shown that the sar paradox can arise even with weak-ordered distributional preference but is overcome by use of the consumer's gain.

The policy under consideration would have the effect of increasi the price of X, and reducing the price of Y. X is consumed by par A, who consumes no Y; and Y is consumed by B, who consumes no . The policy can be undertaken in two stages, stage one moving price each case from P_1 to P_2; and stage two changing prices further fro P_2 to P_3. Alternatively, the complete move from P_1 to P_3 could undertaken in one stage. Figure 5a depicts good X (party A) a Figure 5b good Y (party B). Both diagrams are of the same type Figure 1, but for convenience the letters refer to areas.

Stage 1 (P_1 to P_2)

(1) Assume $g > b + d + 2f$

$\therefore$ Kaldor-Hicks criterion satisfied

$b + d < g + 2j$ follows from (1)

$\therefore$ Scitovsky criterion satisfied

$g + j > b + d + f$ follows (1)

$\therefore$ Gain criterion satisfied

Recommendation: adopt stage one (no compensation pos ble).

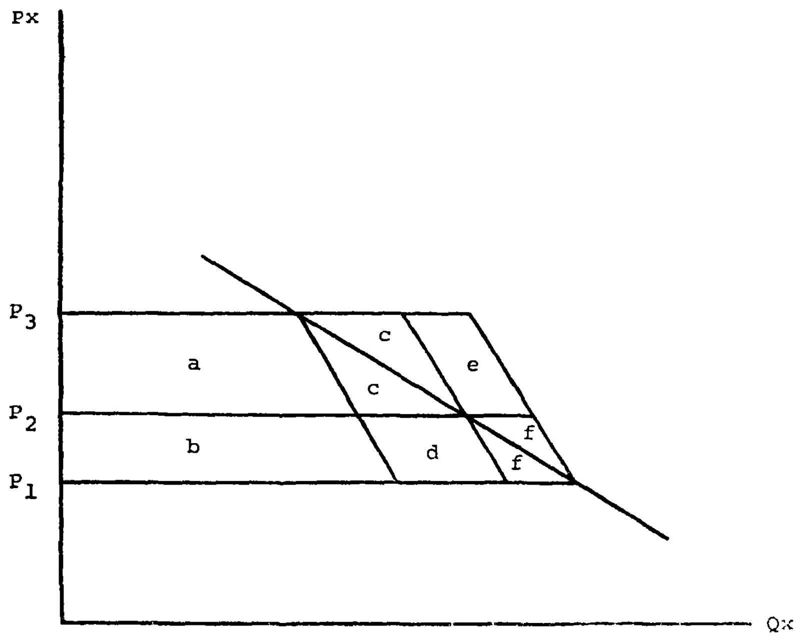


FIGURE 5a

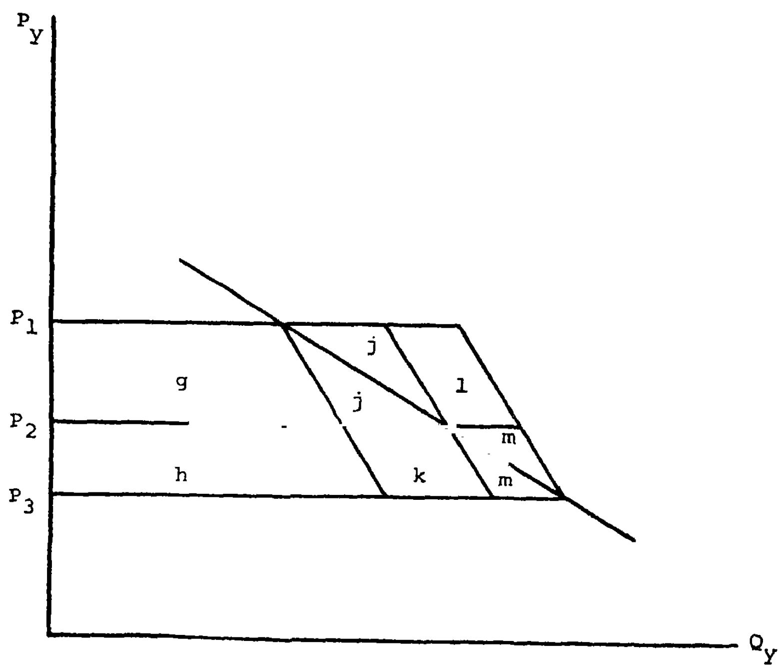


FIGURE 5b

Stage 2 (P_2 to P_3)

(2) Assume $h + k > a + 2c$
$\therefore$ Kaldor-Hicks criterion satisfied
$a < h + k + 2m$ follows from (2)
$\therefore$ Scitovsky criterion satisfied
$h + k + m > a + c$ follows from (2)
$\therefore$ Gain criterion satisfied
Recommendation: adopt stage two.

Complete move (P_1 to P_3)

(3) Assume $g + h < a + b + 2c + d + e + 2f$
$\therefore$ Kaldor-Hicks criterion not satisfied
$a + b < g + h + 2j + k + l + 2m$ follows from (1) and (2)
$\therefore$ Scitovsky criterion satisfied
$g + h + j + k + m > a + b + c + d + f$ follows from (1) and (2)
$\therefore$ Gain criterion satisfied
Recommendation by Kaldor-Hicks-Scitovsky criteria: doubtful whether to adopt policy or not.
Recommendation by gain criterion: adopt complete move.

The criteria are all agreed that stages one and two should be adopted, and the gain criterion is consistent in maintaining that the complete move should be effected. However the Kaldor-Hicks-Scitovsky criteria show that while it is questionable whether the policy should be adopted, it would be a good thing to adopt both parts of the policy. This is the same as saying that while we do not know whether to eat the cherry, the cherry is worth nibbling at until it is all gone.

The same paradox can arise when value judgments on distribution are included in the criteria, as is illustrated by the utility possibility curves depicted in Figure 6, where Q_1 represents the initial position, Q_2 the position after the adoption of stage one and Q_3 the position after the adoption of stage two.

The value judgment on distribution might be such that the movement from Q_1 to Q_2 is case 8 in Table 1 and should be adopted, movement from Q_2 to Q_3 is case 6 and should be adopted, while the movement from Q_1 to Q_3 is case 31, and adoption is questionable. Again we have the nibble paradox.

The above case does not by itself prove the universal superiority of the gain criterion, but the argument can be generalized for normal situations as follows.[6] The compensating variation for a price fall (the

[6] In the case of inferior goods a similar argument is possible. Consideration of the movement in consecutive small stages will again pull the compensated curves towards the

extent to which the gainer is better off) will be less than the consumer's gain, while the compensating variation for a price rise (the extent to which the loser is worse off) will be greater than the consumer's loss. The compensation principle based on the compensating variation will therefore be the more difficult to satisfy, and any time it is satisfied the gain criterion will necessarily be satisfied. The only case where conflict could arise is where the gain criterion is satisfied but the Kaldor-Hicks cirterion is not. But if the move is considered in small

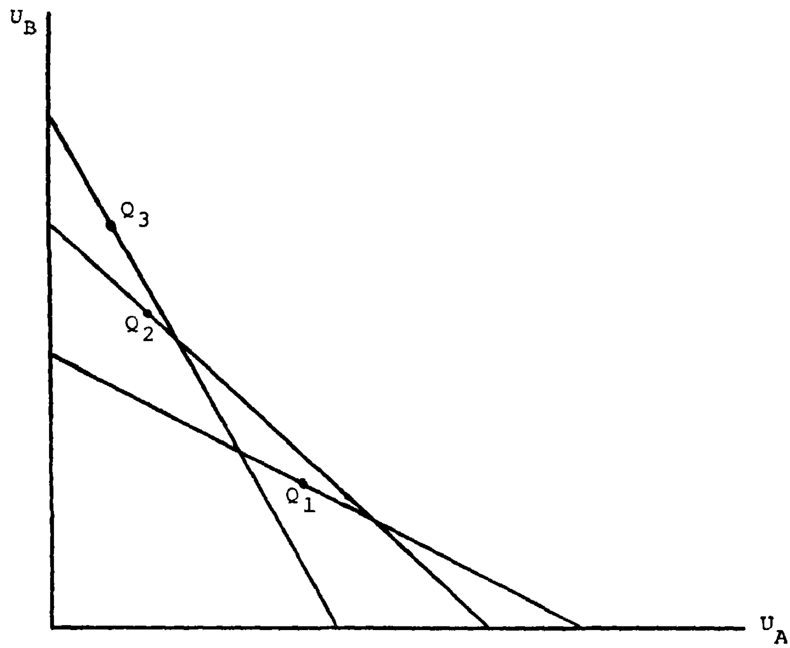


FIGURE 6

enough stages, the compensating and equivalent variations will equal the consumer's gain or loss for each stage. If the gain exceeds the loss for each stage, the Kaldor-Hicks criterion will therefore advocate the policy in stages in any case where the gain criterion advocates it in one move. No lapse of time is involved between the stages, and it must therefore follow logically that if all parts of a policy are worth adopting, the entire policy is worth adopting. That the policy in question might in fact be indivisible is thus of no consequence. If the policy is divisible it might, of course, be worth adopting only some stages. Since it is always easier to get an answer in one simple move than in many

uncompensated, so that for each small stage the compensating and equivalent variations will approach the limit of the consumer's gain or loss. It is therefore necessary that whatever the Hicksian measurements advocate in stages, the gain criterion will advocate in one move.

complicated moves, the gain criterion is therefore shown to be superior.

B. *The Welfare Criteria*

Substituting the Marshallian for the Hicksian measurement of consumer's surplus does not merely simplify measurement, it considerably simplifies the criteria shown in Table I for cases where no compensation is possible. The distinction between the Kaldor-Hicks and the Scitovsky criteria hinges on the difference between the compensating and equivalent variations, but the essence of consumer's gain is that it replaces both of these. If we use the gain criterion, no conflict can arise between the Kaldor-Hicks and Scitovsky criteria, for our one criterion replaces both. If the gain criterion is satisfied, cases 1 to 9 of Table I apply, and if the criterion is not satisfied, cases 19 to 27 apply. Cases 10 to 18 and 28 to 36 are no longer applicable.

Examination of Table 1 shows that where the gain criterion is satisfied the policy should be adopted in all cases except case 2 where the redistribution involved is considered unequivocally bad. Similarly, in all cases where the criterion is not satisfied, the policy should not be adopted, except in case 20 where the redistribution is considered good. We can therefore stipulate the welfare criteria applicable to policy changes where compensation would not be paid in a very simple form.

If the sum of consumers' gains, measured by the changes in areas under the relevant market demand curves, exceeds the sum of consumers' losses similarly measured, the policy should be adopted unless the redistribution involved is held to be sufficiently unfavorable to nullify this conclusion.

If the sum of consumers' losses exceeds the sum of the gains, the policy should not be adopted unless the redistribution involved would be sufficiently favorable to outweigh the gain criterion.

If the redistribution criterion does nullify the decision indicated by the gain criterion, consideration should be given to a change in distribution policy which would make any remaining redistribution acceptable.

IV. *Conclusions*

The Marshallian concept and measurement of consumer's surplus suffered from two limitations. The rigid conversion of utility to money required that the marginal utility of money be constant, and the aggregation of consumers' surplus required that society attach the same importance to a dollar's worth of satisfaction no matter who received it. Hicks succeeded in circumventing both problems. The compensating variation does not measure utility changes in money, it measures money-income changes in money, the change in income measured being

the amount necessary to offset a postulated change in utility. Since the measurements are in hard dollars, it is possible to compare objectively the amounts which could be recouped from gainers with the amounts which would be necessary to compensate losers for the effects of a policy change. *If full compensation were made, the Hicks criterion alone would suffice to establish objectively that the position achieved by policy* change and compensation would be better than the original position. But this criterion alone says nothing about a choice among redistribution without the policy change, policy change without compensation, and policy change with compensation. The Scitovsky criterion pinpoints one problem encountered in applying the compensation principle without compensation, while Little has shown that the desirability of redistribution or compensation is essentially a value judgment. Even with weak judgments permitted and without Little's consistency constraint, however, one fundamental weakness of the compensation principle remains. If full compensation is not to be paid, gains and losses in utility must be compared. The compensating variation does not make this possible; it avoids the problems by assuming that compensation will be paid. Since this assumption is basic to the measurement which underlies the compensation principle, that principle is not appropriate as a criterion in cases where compensation is impossible. The criterion is then unnecessarily complicated, though sufficient it is not necessary, and the dual criteria can lead to the indeterminacy of the nibble paradox.

Re-examination of the Marshallian triangle shows that it does yield an accurate measurement of consumer's gains and losses from price changes, even when the marginal utility of money is not constant. Any net gain or loss resulting from aggregation, however, is an accurate measurement of the gain or loss of welfare only if society is indifferent to the redistribution involved. If the redistribution is considered good in itself, aggregation underestimates a net gain or overestimates a net loss. If redistribution is considered bad, aggregation overestimates a net gain or underestimates a net loss. Policy decisions can therefore be based on the gain criterion in cases where the effects of the policy change would manifest themselves in price changes and where compensation is not practicable. Only where the criterion shows a net gain, but redistribution is considered bad, or where it shows a net loss, but redistribution is considered good, does the criterion fail to give a solution. Even in those cases it is useful, for it focuses the attention of the policy-maker on the importance of considering a change in distribution policy.

Where redistribution or compensation are practicable, the Kaldor-Hicks-Scitovsky-Little criteria, as expanded in Section II of this article, are appropriate.

In many cases calling for policy decisions where the effects do no lend themselves to the measurement of consumers' gain because the are not manifested in changes in market situations subject to measure ment by areas under demand curves, the gain criterion is not usable but in these cases the compensation principle, although theoreticall valid, also faces severe difficulty as the Hicksian techniques of measur ing compensating and equivalent variations are also unusable. In prac tice there are many cases of policy issues where the gain criterion, to gether perhaps with the analogous concept of producers' surplus, i appropriate; and in these cases at least we can extract from welfar economics criteria which are capable of practical application.[7] I would appear that consumer's surplus is, after all, considerably mor than "a totally useless theoretical toy" [7, p. 180].

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[7] For an attempt to make this criterion suitable for practical application, see [17].

REGULATION AND CONSPIRACY

By FRED M. WESTFIELD*

The spectacular conspiracies among manufacturers of electrical equipment have received wide publicity. Few economists, indeed few readers of newspapers, can have failed to become acquainted at least with the broad outlines of the schemes used by the manufacturers to enforce higher prices on the capital goods sold by them to electric utilities. Somewhat less widely publicized have been the subsequent financial settlements reached by some of the manufacturers with TVA, with the federal government, and with other public as well as private power companies. Some students who are familiar with the details of the conspiracies believe that the settlements being negotiated are, on the whole, extremely favorable to the conspirators. And it has been suggested that the private regulated electric power companies are sympathetic to the plight of the equipment suppliers and are enforcing their legal claims reluctantly. On the other hand, a failure on their part to press the claims might subject them to lawsuits from stockholder-owners and to reprisals from regulatory agencies.

It is the purpose of this article to shed light on these matters by scrutinizing the buyer-seller relationship in the market for capital goods supplied to a regulated industry. We demonstrate (i) that it can be in the interest of the regulated private power generating company to pay a higher rather than a lower price for the plant and equipment it purchases, and (ii) that in other instances, though not benefited, the regulated utility is not at all harmed by the inflated prices that result from conspiracies.

Such propositions may be self-evident to the emotional public-power-antiprivate-power advocate. They may have appeal to the apologist for the electrical industry who wishes to attribute the very rapid technological advances in power generation and transmission technology to the research and development financed by equipment producers out of the profits of the conspiracies. And they are obvious where the utility has an ownership interest or the equivalent in its suppliers. The argument of this paper, however, is direct and postulates no extra-market profit-sharing arrangements. It is that profits of the regulated,

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profit-maximizing public utility need not decrease when the price of capital goods is raised by the seller.

Common sense as well as conventional marginal analysis tell us that a firm which is free to maximize profit and constrained only by its technology and the market it faces must experience a decline in profit if the price paid for any input is increased. For the regulated public utility, however, prices of inputs often help to determine how much profit the firm is permitted to earn. Regulation of electric utilities by State or local regulatory commissions usually involves some limitation on the rate of return on value of assets. Details vary widely. At one extreme may be token regulation that exercises no effective restriction at all on the behavior of the public utility. Allowable rates of return, however computed, are in excess of the rate of return associated with unregulated maximizing behavior. Although here too, given the allowable percentage rate, higher prices for capital goods would in effect be a license to higher earnings for each level of physical gross investment, this easing of the restriction would clearly be of no consequence. As for the unregulated monopoly firm, higher prices of inputs must reduce profit. On the other hand, in circumstances where the regulatory climate is less favorable and the restriction on the rate of return does take a bite, the increase in cost associated with the higher capital-goods prices may at least in part be counterbalanced by what amounts to a relaxation of the regulatory commission's restriction on profit, as the value of the capital base rises with capital-goods prices. If, in sharp contrast to token regulation, the utility is forced by the regulatory commission to operate on an inelastic portion of the demand curve for electricity, then the higher prices will surely allow the utility to more than compensate for higher costs by additional revenues, whether the commission's rate of return is based on capital valued at original cost or at replacement cost. Although as we will demonstrate, restrictions supplementing the ceiling on rate of return are generally necessary if the commission is to induce the utility to operate on an inelastic portion of the demand curve, inelastic demand is not a necessary condition for a full offset of the cost increase. With demand elastic, whether or not profits will respond favorably to a price increase of capital goods will depend on the production function, on precisely how higher priced capital goods are permitted to influence the value of the capital base, as well as on the demand function for output.

Our task, then, is to demonstrate that there exist opportunities for conspiracy that are profitable to both buyer and seller and to indicate the circumstances that lead to this perversity which can make the privately owned utility regulated by a commission a passive, if not active, co-conspirator with its suppliers of plant, machinery, and equipment.

I. *The Model*

In order to develop the argument, a simple statical model of the regulated monopoly firm will be constructed.

First, it is supposed that there is a production function for the single output, electricity, X, produced with two inputs: capital goods, i.e., machinery, equipment, etc., K, and variable inputs, i.e., labor, fuel, etc., V. This relationship summarizes the physical input-output possibilities of the production process and is written as the smooth function

$$X = f(K, V). \tag{1}$$

The usual properties of smoothly diminishing marginal rates of substitution and diminishing marginal physical productivities may be assumed.

Second, it is assumed that the market opportunities of the firm are summarized by a total-revenue function

$$R = R(X), \tag{2}$$

which has the conventional inverted U-shape, with marginal revenue ($R' = dR/dX$) and average revenue (R/X) decreasing functions of output. It is not essential to suppose that all output is sold at a single price. There may be price discrimination or peak-load pricing, for example. Average revenue may be thought of as an average of the various prices that are altered in a specified way to generate annual sales, X, and the total annual revenue, R, corresponding to these sales.

On the input side it is postulated that the firm is able to purchase as much or as little as it chooses of the variable inputs, V, and the capital inputs, K, at prices W_V and W_K per unit, respectively. There is, in short, neither monopsony power nor quantity discount. Input prices are competitive or fixed by sellers.

Next, we assume that the rate of interest, r, is used by the firm to discount future earnings. This interest rate is constant through time and independent of the quantity of capital purchased and of all other variables subject to the control of the firm.

It is also convenient for the presentation of the basic argument to rule out dynamic influences. The demand, i.e., total revenue function, each year remains the same. The capital stock does not depreciate—once purchased it lasts indefinitely. Such other dynamic effects as technological change of the production process or in the quality of inputs, as well as problems stemming from uncertainty, are neglected.

While these assumptions drastically oversimplify all aspects of mar keting, production, and investment in the electric power generating in dustry, it will become apparent that a more complex model woul serve mainly to obscure rather than invalidate the argument.

In the absence of regulatory restrictions, other than possibly thos concerning relative prices among various types of service that are buil into the demand and total revenue functions, the firm can be expecte to maximize the present value of the future net income stream

$$Z = \sum_{0}^{\infty} [R(X) - W_V V](1 + r)^{-t} - W_K K, \tag{3}$$

or what amounts to the same thing under our assumptions, to maxi mize annual profit

$$\pi = rZ = R[f(K, V)] - W_V V - rW_K K. \tag{4}$$

The solution of this standard maximization problem, if both inputs ar used, leads to the familiar equilibrium conditions

$$\partial R/\partial V \equiv R'f_V = W_V, \tag{5a}$$

and

$$\partial R/\partial K \equiv R'f_K = rW_K. \tag{5b}$$

Variable inputs, V, must be purchased so that the increment to tot revenue from an additional unit ($R'f_V$) is just equal to the cost of suc an additional unit (W_V). And the amount of capital goods, K, to b used in equilibrium must be such that the increment to total revenu for the year from an additional unit ($R'f_K$) is precisely equal to th cost of using such a unit—the interest on the market price (rW_K We designate the equilibrium quantities of inputs obtained by sol ing the two equations as K^0 and V^0, and the corresponding equilibriur output and annual profit as X^0 and π^0, respectively.

Regulation of public utilities takes many forms, and here no effort i made to catalog them. Electric, gas, telephone, and other public utili ties have often imposed on them some limitation on the "rate of retur on capital," which may not be exceeded during a year's operatio Such a "fair-rate-of-return" is usually calculated as a ratio of tot revenue less current expenses (variously defined) to a valuation of th capital stock (variously determined). It is natural within the model o

the firm here considered to suppose that the public utility commission places a ceiling, say, γ, on the ratio of total revenue less variable cost to total capital stock valued at some price λ. In terms of our notation, it is required of the utility that the inequality

$$\frac{R[f(K, V)] - W_V V}{\lambda K} \leq \gamma \tag{6}$$

be satisfied by the equilibrium position. The restriction could realistically take on somewhat different forms; but here is not the place to study the details of differences in results arising from variations in the numerator and denominator of the quotient on which the ceiling is placed. Indeed, without loss of generality, we make until further notice the additional simplification that $\lambda = W_K$.

Some consequences of such a ceiling on rate of return have been recently investigated by Averch and Johnson [1] and Wellisz [7].[1] We shall now develop the model of such a regulated firm somewhat further, avoiding the formalism of nonlinear programming of [1] and the linear specializations of [7].

Three separate situations must be distinguished. First, the "fair-rate-of-return" allowed by the regulatory commission may be in excess of the rate of return that unhampered profit maximization, analyzed above, in fact yields. If we label this desired rate of return of the unregulated utility, ρ^0, one has

$$\rho^0 \equiv \frac{R[f(K^0, V^0)] - W_V V^0}{W_K K^0} < \gamma$$

for the equilibrium constellation. This is token regulation. The firm does not want to earn such a high rate γ; and earning it would result in less total profit. The restriction is like a Korean War ceiling price: above market levels.

The second possibility is the opposite extreme in which the allowable "fair-rate-of-return" is below the market rate of interest, i.e., where $\gamma < r$. In such a case the profit-maximizing public utility will wish to own no capital stock at all and presumably go out of business because the restriction (6) obviously implies $\pi \leq (\gamma - r)W_K K$. And for $\pi \geq 0$ this can be satisfied only for $K = 0$. No output, no firm in

[1] Formally the problem of the effects of a price ceiling for a monopolist, first fully treated in [4, Ch. 13], and also in [2], and that of a rate-of-return ceiling, have much in common with each other.

long-run equilibrium! This possibility, like the first, is clearly not of much interest for our purposes.

The third possibility is the interesting case. If the "fair-rate-of-return" is set somewhere between the desired rate of return ρ^0 associated with unfettered profit-maximization and the market rate of interest r, the firm must modify its input-output-price configuration associated with the standard monopoly equilibrium to make the largest possible profit and yet satisfy the limitation on rate of return. Adding the restriction given by (6) to the maximization problem yields the solution that the marginal condition (5a) must continue to be satisfied. The condition (5b), however, is replaced by the equation

$$\rho^* \equiv \frac{R[f(K^*, V^*)] - W_V V^*}{W_K K^*} = \gamma, \qquad \rho^0 \geq \gamma \geq r. \tag{7a}$$

This means that, as in the absence of rate-of-return regulation, vari able inputs must be purchased up to the point where their margina revenue product is just equal to their unit cost. But marginal revenu product of capital must be lowered through acquisitions of capita goods—even though the additional cost of capital is greater than addi tions to revenue. In fact, the acquisitions must be used to lower tota profit to the point where the actual rate of return, ρ^*, is just equal t the maximum allowable "fair rate" γ. Obviously it does not pay to g still further with capital purchases, pushing the actual rate, ρ^*, belov the "fair rate" γ, and satisfying the constraint (6) as *in*equality.

One might wonder why it is that capital outlays alone are to b bought beyond the point of marginal profitability. Why are not th variable inputs also increased beyond the margin so as to bring tota profit in line with the amount of profit permitted by the regulator commission? The answer is that every additional capital-dollar spent while reducing profit, also increases the total amount of profit tha may be earned with a given "fair rate" γ, because the capital purchase increase the base on which the rate is computed. Additional purchase of variable inputs would reduce profit without, at the same time, in creasing the total amount of permitted profit.

The two equations (5a) and (7a) can be solved for the new equi librium quantities of inputs K^*, V^* which determine, via the produc tion function, X^*, and via the total revenue function, total profit π^* Because of the "stability conditions" and the smooth conventiona curvatures of the functions it follows that K^* is greater than K^0,[2] and

[2] This consequence of rate-of-return ceilings is emphasized in [1] and [7].

of course, π^* is less than π^0. Complete characterization of the responses of equilibrium quantities V, X and of the product price requires fuller specifications of the demand and production functions. If marginal products and marginal revenue are nonnegative and inputs V and K are substitutes in production (in the sense that, whatever their prices, a reduction in the price of one would, in the absence of a regulatory constraint, induce a decrease in the equilibrium quantity purchased of the other), then $V^* < V^0$; if they are complements, $V^* > V^0$.[3] Similarly, only if K and X are "regressive"[4] (so that reductions in the price of capital for an unconstrained firm bring reductions in equilibrium output) would $X^* < X^0$. Since such regression can be shown to require that capital be an "inferior" factor of production, it is surely uncharacteristic of electric power production technology. Thus, under these conditions, equilibrium output will surely increase and its price fall as K rises.[5]

Some of the principal features of the analysis are conveniently summarized in Figure 1. The curve labeled T_K is the largest amount of Total Net Revenue Product of Capital,

$$T_K = \max_V \{R[f(K, V)] - W_V V\} = T(K),$$

that can be earned with various amounts of capital, K.[6] The shape of this function embodies the properties of both the production function and the total revenue function. Behind the scenes, for each value of K, the quantity of the variable input, V, is being adjusted optimally

[3] These results are obtained, on the one hand, by differentiating the system of equations (5a) and (5b) totally with respect to rW_K or W_V and observing that $dV/d(rW_K) = dK/dW_V = hR_{VK}$, where h is a negative number because of the stability condition and $R_{VK} = \partial(R'f_K)/\partial V = \partial(R'f_V)/\partial K$. On the other hand, by differentiating the system of equations (5a) and (7a) totally with respect to γ, one obtains $dV/d\gamma = gR_{VK}$, where g is a negative number because of the stability conditions and because the regulatory constraint is binding. By assuming that dV/dW_K has a sign independent of W_K and W_V, we specify that R_{VK} has the same sign for all V and K; and our assertion that the sign of $dV/d\gamma$ is the same as the sign of $dV/d(rW_K) = dK/dW_V$ is proved.

[4] Cf. [3, pp. 93ff.].

[5] Using the results of footnote 3, and the notation suggested there, one obtains, on substitution from the total differentiation of the set (5a) and (5b), $dX/d(rW_K) = h(f_V R_{KV} - f_K R_{KK})$ and from the set (5a) and (7a), $dX/d\gamma = g(f_V R_{KV} - f_K R_{KK})$. For positive marginal revenue and marginal physical products $dX/d(rW_K) > 0$ implies "regression," and thus our statement that a reduction in γ increases equilibrium output and lowers price, except in the case of regression, is proved. Further details on short-run economics of electrical utilities can be found in [8].

[6] The designation of T_K as Total Net Revenue Product is suggested by Joan Robinson's terminology in [4]. The maximum of $R - W_V V$ with respect to V, for K as parameter, requires that its partial derivative with respect to V, $R'f_V - W_V$ vanish. But this is condition(5a) which, solved for V as function of K and substituted for V in $R - W_V V$, yields $T(K)$.

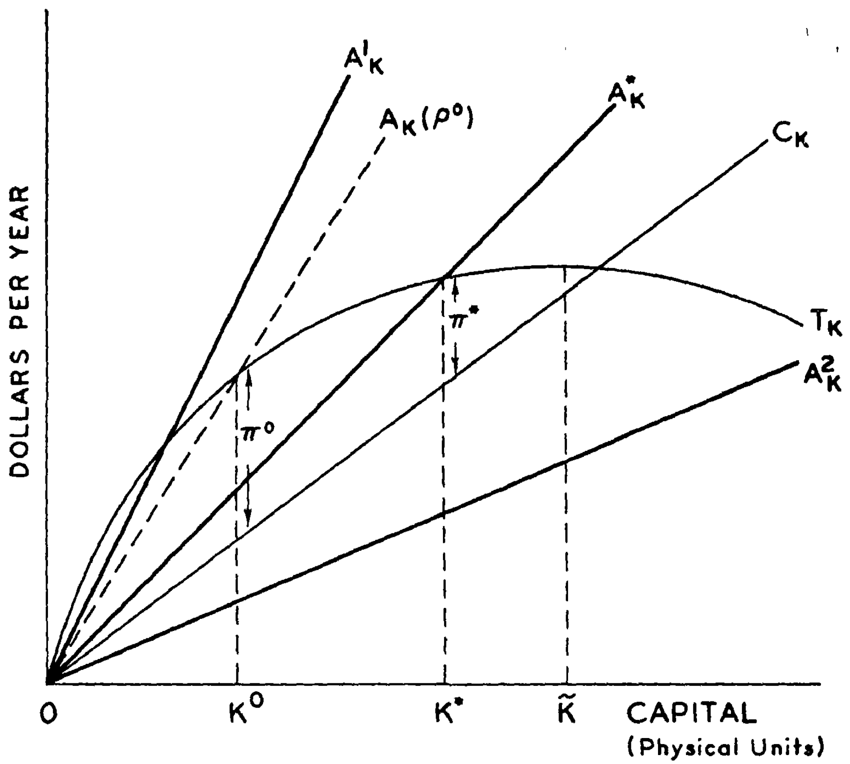


FIGURE 1. INITIAL EQUILIBRIUM

At K^* the average rate of return on capital, ρ^0, is proportional to the slope of tl dashed ray. Maximum profit is established at K^0, when $\gamma > \rho^0$, where marginal rate return on capital equals the interest rate; at a point like K^*, when $r < \gamma < \rho^0$, whe average rate of return on capital equals the allowed rate; and at $K = 0$, when $\gamma <$ where the firm leaves the industry.

so as to satisfy (5a).[7] The curve can be thought of as an envelope (a family of net revenue product curves that might be drawn with as parameter. The ray from the origin labeled C_K is the Total Capit: Cost function $C_K = rW_KK$ showing the cost per annum of using var ous amounts of capital. Maximum total profit π, associated with eac possible level of capital stock K, is given by the vertical distance b tween T_K and C_K. This maximum reaches its maximorum at the lev of capital input K^0, where the two curves have the same slope. It is simple matter to show that the slope of the T_K-curve is $R'f_K$[8] and (

[7] Thus for each quantity of fixed plant K, the "short-run" marginal cost of outp W_V/f_V, considered as function of X by substituting for V from the production functi (1), is equated to marginal revenue R' for maximum "short-run" profit.

[8] $dT(K)/dK = R'[f_K + f_V dV/dK] - W_V dV/dK = R'f_K$.

the C_K-ray, rW_K. Hence at K^0 the diagram depicts the conditions of equilibrium in the absence of the regulatory ceiling—(5a) is satisfied behind the scenes and (5b) on the diagram itself.

The limitation on the rate of return imposed by the regulatory commission is depicted by the requirement that for a given valuation of capital goods, λ, and a specified "fair rate" γ, T_K must lie below the ray depicting the Maximum Allowable Total Net Revenue Product of capital, A_K for short. This ray is given by $A_K = \gamma\lambda K = \gamma W_K K$ as a function of K. That the requirement $T_K \leq A_K$ amounts to the same thing as the rate of return restriction (6) is immediately evident if we multiply both sides of that condition by $\lambda K = W_K K$ and observe that the left-hand side maximized with respect to V is T_K and the right, A_K. Three A_K-rays are drawn corresponding to the three possibilities we have outlined. If the allowable rate of return is as high as γ^1, higher than the desired rate of return ρ^0 associated with unregulated profit maximization, the relevant ceiling is given by the ray labeled $A_K{}^1$. Clearly, the maximum profit position—the largest distance between the T_K- and C_K-curves that does not push T_K higher than the $A_K{}^1$-ceiling—continues to be the solution associated with K^0. If the allowable rate is as low as γ^2, lower than the prevailing rate of interest r, the relevant ceiling is given by the ray labeled $A_K{}^2$. The ceiling does not allow the firm to cover total cost. Thus the best profit position occurs at $K = 0$. If the allowable rate of return is γ^*, $r \leq \gamma^* \leq \rho^0$, as for example the ray labeled $A_K{}^*$, then the maximum profit position subject to the ceiling is given by the intersection of $A_K{}^*$ with T_K—the condition (7a). For different values of γ^* in this region the ray $A_K{}^*$ is pivoted about the origin; the solution moves along the T_K-curve, and evidently K^* must rise and π^* fall for reductions in γ^*.

II. *Profits and Prices of Capital Goods*

What are the conditions that allow the profit-maximizing electric utility, subject to restraint on its rate of return, to realize an increase, or at least not a decrease, in total profit when confronted by an increase in the price of assets purchased. The answer to this question also tells under what circumstances such a firm should be enthusiastic or at least apathetic about a price-rigging conspiracy among suppliers of capital goods.

The analysis of the equilibrium position of the regulated firm developed so far provides the groundwork for the answer. We shall use the technique of comparative statics. First, we suppose that the initial equilibrium specified above is disturbed by, say, a once-and-for-all in-

crease in demand for electricity[9] which, with input prices unchanged, leads to higher equilibrium output, increased derived demands for capital and variable inputs, and larger maximum profit. Then we proceed to examine how this new equilibrium position would change if, instead of constant prices, higher prices for the new investment goods demanded would prevail—how the new profit position would be affected by an equilibrium adjustment to such higher prices. We shall not discuss further the cases, uninteresting for the purposes at hand, of nonviable ($\gamma < r$) and nonrestrictive ($\gamma > \rho^0$) rate of return ceilings.[10]

In terms of our concepts, an increase in demand for electricity leads to an upward shift in the T_K-function. If additional capital in excess of K^*, call it I, can be purchased at a price W_I equal to the (average) price, W_K, paid for the existing capital stock K^*, then the capital cost curve, C_K, is unchanged—it continues to be the same ray o Figure 1. Similarly, if the new capital is valued for rate-making purposes the same way as the old, then the Allowable Net Revenue Product curve, A_K, also continues to be the same ray A_K^* of Figure 1. The equilibrium conditions determined by equations (5a) and (7a) for the new T_K-function yield the new desired capital stock $\bar{K}$, with investment $\bar{I} = \bar{K} - K^*$. In Figure 2 this new equilibrium is indicated by the intersection of the new T_K-function with the old A_K-ray extended through the initial equilibrium point a^*. The new higher maximum profit that can be earned is given by the distance $\overline{ca}$.

So much for the effects of the increase in the demand schedule fo electricity, with capital goods price unchanged. Now to the effects of higher capital goods price, given the shift in the demand schedule.

Original vs. Reproduction Cost Valuation

A higher price paid for new plant, equipment, and machinery affects both costs and, because the fair-rate-of-return is computed a percentage of asset value, the allowable net revenue product associated with any physical amount of capital. The C_K- and A_K-function of Figure 2 both shift up and hence the equilibrium associated wit the new demand situation is not represented by the point $\bar{a}$ but b the intersection of a new higher A_K-function with the new T_K-functior

[9] The disturbance could just as well be a fall in variable input prices, a tax cut, technological change, etc.

[10] *Cf.* Stigler and Friedland [5]. "[Their] study was undertaken primarily to investigat the feasiblity of measuring the effects of regulation. . . ." But they conclude that the failure "to find any significant effects of the regulation of electrical utilities" is an indication of lack of effect of regulation rather than an indication of a defect in their measurement techniques. It is doubtful whether an expert-observer of the regulatory proce could be found who believes nonrestrictive regulation to be the typical case.

The function representing capital costs, instead of continuing in a straight line through the point c^*, associated with the initial equilibrium capital stock (K^*), is now kinked at this point and continues in a straight line with steeper slope to reflect the higher incremental cost W_I of new capital goods purchases. Symbolically, $C_K' = rW_KK^* + rW_II$.

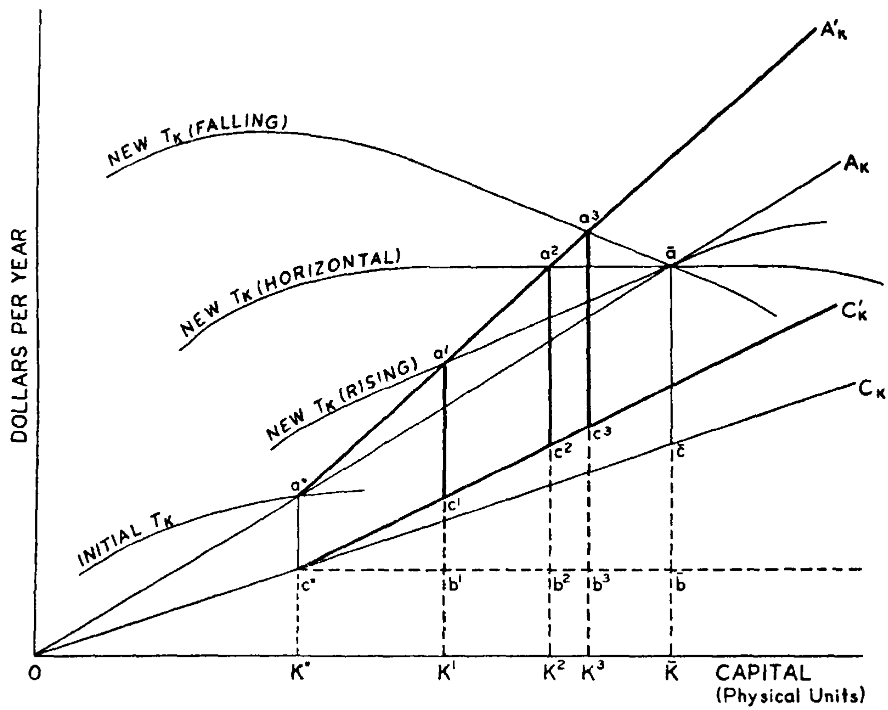


FIGURE 2. ORIGINAL COST VALUATION

Shifts in demand are shown as upward shifts in T_K, increases in prices of new capital goods by kinks in C_K and A_K. It is necessary that $c^2a^2 = \bar{c}\bar{a}$ because $K^*a^* : K^*c^* = K^2a^2 : K^2c^2 = \bar{K}\bar{a} : \bar{K}\bar{c}$, by construction, and because $K^2a^2 = \bar{K}\bar{a}$, by hypothesis of a horizontal T_K-function. Similarly, $c^3a^3 > \bar{c}\bar{a}$ and $c^1a^1 > \bar{c}\bar{a}$. The distances bc, being rentals on new capital goods, are proportional to receipts by the manufacturers from their sale.

The effect of the price increase on the allowable net revenue product ray is slightly more complex. It depends on how the regulatory commission allows new higher priced capital goods to influence the rate base.

At one end of the regulatory spectrum, the regulatory authority may insist that the value of the capital base for regulatory purposes be increased by the original cost of the new capital goods and by no

more. In this case the original A_K-ray will, like the C_K-ray, also exhibit a simple kink at the initial equilibrium. And the new allowable net revenue product is $A_K' = \gamma\lambda K^* + \gamma W_I I$, where the valuation factor $\lambda = W_K$ for the initial capital stock remains unaffected by an increase in the market price for new capital goods. The steeper slope to the right of a^* shows that new capital purchased can sustain increments to allowable net revenue proportional to the higher price, i.e., value, of the additional new units of capital.

Thus both costs and allowable net revenue for any level of new investment increase with increases in prices of capital goods; but allowable net revenue increases by a greater amount for each level of real investment I because the functions are tilted upward in proportion to the initial slopes—the difference between slopes of the new segments of A_K' and C_K' in Figure 2 is $(\gamma - r)W_I$, positive as condition for viable regulation.

Whether in a regime of original-cost valuation the increase in allowable profit can be exploited to yield higher equilibrium profit depends entirely on the shape of the T_K-function in the neighborhood of equilibrium. Three cases illustrated, in Figure 2, must be distinguished. First, if the equilibrium intersection of $T_K = A_K'$ lies on a rising portion of the T_K-curve, the new equilibrium-level capital stock due to a higher price W_I is K^1 rather than $\bar{K}$. Total annual profits fall from $\overline{ca}$ to c^1a^1. It is seen to be a mathematical necessity that the latter profit figure is smaller. In this case increases in allowable profits made possible by a conspiracy cannot be captured by the utility. But there is no presumption, as we shall see, that the conjunction of demand for electricity, technology, variable input prices, interest rates, and regulatory policy leads to an equilibrium falling on the rising portion of a T_K curve. A second possibility is that equilibrium occurs on a horizontal portion of the curve. Here the utility can fully offset the increase in costs. Equilibrium capital stock falls from $\bar{K}$ to K^2 as a result of the increased price, but annual profits are unchanged. The distances $\overline{ca}$ and c^2a^2 are of necessity equal. Thus we have the situation where the profit-maximizing utility can be expected to be indifferent to a conspiracy forcing up the price of capital goods. The third possibility is that equilibrium occurs on a falling portion of the T_K-curve. As the equilibrium capital stock is forced down from $\bar{K}$ to K^3 in response to higher cost, equilibrium profit increases! The distance c^3a^3 is necessarily greater than $\overline{ca}$. Here the utility will surely welcome the price increase.

Before turning to an examination and evaluation of the economic

circumstances that generate horizontal and falling T_K-functions and hence perverse profit response under original cost valuation, let us examine how equilibrium profits would respond to the increase in the price of capital goods in regulatory jurisdictions using elements of replacement costs in determining the value of the capital base. The market price of capital goods is, in short, allowed to influence the value of capital goods already owned.[11]

The valuation coefficient λ applied to the initial capital stock, so far set equal to the constant W_K, now must be considered a function of W_I, say $\lambda(WI)$. In the extreme case, the antithesis to original-cost valuation, $\lambda = WI$ and the value assigned to existing assets reflects dollar-for-dollar all changes in current prices of capital goods. More generally, $0 \leq d\lambda/dW_I \leq 1$, which says that the value placed on existing capital stock by a regulatory commission increases with the market price paid for capital additions by amounts equal to or less than the increase in price. For the pure case of original-cost valuation considered in Figure 2, $d\lambda/dW_I = 0$; for the pure case of replacement cost $d\lambda/dW_I = 1$. Probably the typical real-world case of valuation, whether called original cost or replacement cost, lies somewhere between these polar cases.

Escalation in the value assigned to the initial capital stock broadens the potentiality for rising profits from rising capital-goods prices. This is at once evidence from Figure 3 which differs from Figure 2 in that the A_K-function, here labeled A_K'', besides a kink, has also been shifted upward for the interval to the left of K^* to reflect the impact of the change in W_I on λ. The slope of A_K'' to the right of K^* is, for a given price W_I, the same as in Figure 2; marginal allowable net revenue product for new investment is not altered by the change in valuation schemes, but average allowable net revenue product is everywhere higher for the case of Figure 3. The illustration depicts $d\lambda/dW_I < 1$. For the extreme case of $d\lambda/dW = 1$, which the interested reader may wish to superimpose on Figure 3, the kink in A_K'' disappears entirely and the slope to the right of K^* applies everywhere—average allowable revenue product is by this valuation scheme equated to the marginal allowable revenue product. The C_K-function is, of course, unchanged from Figure 2: though higher market prices increase the "value" of existing assets, they cannot affect the amount paid for their use.

Evidently now even an upward-sloping T_K-function may, and a

[11] According to [6, p. 310], as of early 1961, about one-third of the State regulatory commissions, explicitly "make use of valuation factors such as reproduction cost, 'current replacement,' etc., other than original or investment cost, in order to obtain a 'fair value' rate base."

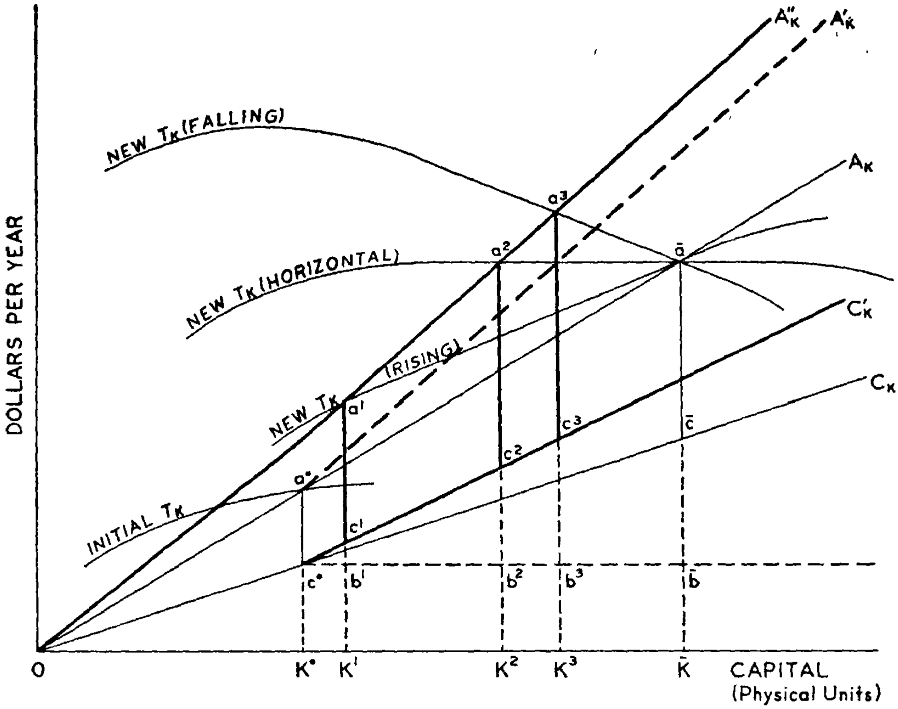


FIGURE 3. REPLACEMENT COST VALUATION

The increase in price of new capital goods brings in addition to a kink at K^*, an upward shift for A_K. A'_K from Figure 2 is shown as a dashed line for comparison. Note that $c^*a^* > c^2a^2 > \bar{c}\bar{a}$. Although $c^1a^1 < \bar{c}\bar{a}$, it is clear that for a somewhat lower positive slope of the new rising T_K-function the inequality is reversed.

horizontal T_K-function must, generate increasing equilibrium profits. Of course, the downward-sloping T_K-function continues to generate the perverse profit response also. Indeed, examination of Figure 3 reveals that the increased price of capital goods raises equilibrium profits of the utility by a greater amount (a) the lower is the slope of the T_K-function for a given value of $d\lambda/dW_I$, and (b) the higher is $d\lambda/dW_I$ for a given slope of T_K. Equilibrium profits, however, decline when $d\lambda/dW_I$ is not large enough to outweigh the positive slope of T_K.

All of our conclusions regarding the relationship between price increases of capital goods and equilibrium profits shown geometrically in Figures 2 and 3 may also be deduced analytically. By differentiating the profit function

$$\pi = R - W_V V - rW_K K^* - rW_I I \tag{4}$$

with respect to W_I, remembering that the equilibrium conditions (7a) now modified to

$$T_K = \gamma[\lambda(W_I)K^* + W_I I], \tag{7b}$$

and also (5a) must continue to be satisfied, one obtains the general relationship

$$d\pi/dW_I = \frac{R'f_K - rW_I}{R'f_K - \gamma W_I}\gamma(K^* d\lambda/dW_I + I) - rI. \tag{8}$$

This shows how annual equilibrium profits respond to the increase in the price of capital goods. Since $R'f_K - rW_I < 0$ because regulation has teeth and $\gamma > r$ because it is viable, the first term on the right is positive. Whether the price increase causes increase, no change, or decrease in equilibrium profit depends on whether the positive last term on the right, rI, reflecting the rate of increase in cost from the price increase, is less, equal to, or greater than the positive first term, reflecting the rate of increase in revenue.

In addition to the influence of the slope of the T_K-function ($R'f_K$) and the rate of escalation in the valuation factor ($d\lambda/dW_I$) on the profit response of the utility, we can see from the geometry or from expression (8) that at least for nondownward-sloping T_K-functions the positive profit response from a price increase is greater, the lower is the firm's growth rate I/K^*. Indeed a utility conceivably might not even have to be in the market for new capital goods at all to benefit from price conspiracies. If a regulatory commission allows upward revisions in λ simply on the basis of a higher price level for new electrical equipment and machinery, then the utility that has not even experienced a change in demand for its output may still be able to saddle its customers with price increases. On the principle that inflation has raised the cost of replacing existing capital, allowable revenue product will be raised (A_K being rotated about the origin); and higher profits are realized whatever the slope of T_K, as long as the regulatory restriction on rate of return has teeth.

Horizontal and Falling T_K-Functions

Under a regime of original-cost valuation the water shed between normal and abnormal profit responses was seen to be zero slope of the T_K-function. To complete the argument that even under such valuation rules profits need not decrease if the price of capital goods is increased, we must examine the considerations, so far hidden in the background, that determine the shape of T_K-functions. What are the circumstances that lead to a horizontal or falling T_K-function?

In principle two types of situations produce a peak in total net revenue product T_K: physical saturation or market saturation. Marginal revenue product of capital ($dT_K/dK = R'f_K$) is zero either if mar-

ginal physical product of capital (f_K) or marginal revenue of output (R') is zero. However, fundamental technological considerations appear to rule out the case of market saturation unless a regulatory commission imposes additional rules that supplement a rate-of-return ceiling. The firm will dodge market saturation so that physical saturation will occur first. To verify this, consider what must be happening behind the scenes as K is increased in moving along a T_K-curve. The condition (5a) must continue to be satisfied as output, price, and variable inputs are adjusted to maximize net revenue for each value of K. In terms of the conventional diagram showing equilibrium price and output for a firm by intersections of marginal cost and marginal revenue curves, this condition can be interpreted as requiring that for any quantity K of capital stock, output must be adjusted so that the "short-run" marginal cost curve of output (V varying as K is held constant) intersect the marginal revenue curve.[12] As K rises, this short-run marginal cost curve will shift downward, and the intersections trace out increasing equilibrium output of electricity and reductions in its price.[13] But regardless of how large the capital stock, it seems safe to assert that technology of electricity production requires some positive quantities of variable input having finite marginal product. Short-run marginal costs can thus not be zero, regardless of how large the capital stock. Therefore, marginal revenue in equilibrium will not reach zero as the capital stock is increased, and market saturation does not cause the T_K-curve to reach a peak. Indeed, an important corollary to this theorem is that a regulatory commission, though able to encourage expansion of the physical plant, can never with rate of return regulation *alone* succeed in forcing a utility to price and produce on an inelastic portion of the demand curve—an important argument against a regulatory scheme that has as an aim the simulation of competitive markets.

Physical saturation, on the other hand, may occur as K is increased in movements along the T_K-curve. For a well-behaved demand curve, we have just argued that there is an upper bound to equilibrium output, call it $\hat{X}$. And we asserted as technological fact that there is an absolute minimum amount of variable input, say $\hat{V}$, required for the production of this amount of electricity. If there exists a *finite* quantity of capital stock K which in cooperation with $\hat{V}$ is capable of yielding $\hat{X}$, then the T_K-function first reaches its peak value at the smallest

[12] Cf. footnote 7.

[13] However, if the input K is "regressive" this short-run marginal cost curve would have to shift upward: equilibrium output would decrease and its price increase with increases in K.

amount of capital, say $\tilde{K}$, which with $\tilde{V}$ just barely yields $\tilde{X}$. For this amount of capital, $\tilde{K}$, the total net revenue product is

$$\tilde{T}_K = \tilde{R} - W_V \tilde{V},$$

where $\tilde{R} = R(\tilde{X}) = R[f(\tilde{K}, \tilde{V})]$. At this configuration, it is readily seen, increments to the capital stock cannot induce further downward shifts of the short-run marginal cost curve, and the marginal product of capital is zero.

How will the T_K-function behave if the amount of capital acquired by the firm must exceed $\tilde{K}$? For industrial processes like electricity generation it may be technologically feasible to apply redundant capital judiciously in a Knightian Region III without suffering a decline in physical output or requiring additions to variable input. The firm's short-run marginal cost curve is then not forced by the additional capital to shift up from its low point along the marginal revenue curve. The result is a horizontal continuation of the T_K-function for $K > \tilde{K}$. Technological conditions, however, may not be this favorable. The capital stock in excess of $\tilde{K}$ may contaminate the production process so that even to maintain the output $\tilde{X}$ more variable input $V > \tilde{V}$ is required. Like redundant rain in an agricultural productivity experiment, redudant capital may cause physical output to decline in the Knightian Region III. In this case we obtain a falling T_K-curve for $K > \tilde{K}$.

A negative slope for the T_K-curve may also result, even when the technological constraints do not require it, from supplementary regulatory constraints. A strict regulatory commission, in addition to limiting the utility's rate of return, may insist that capital acquisitions be "used and useful."[14] The commission may require a price reduction to force the utility to usefully employ excess capital $K - \tilde{K}$, or it may insist merely that such capital be utilized, giving the utility, as it were, the option of lowering price or of wasting output by absorbing it inefficiently in internal operations.[15] In either of these cases the T_K-curve will turn down at $\tilde{K}$ even though technological considerations alone would allow it to be horizontal for additional capital; and the opportunity to pay higher prices for capital goods becomes a mechanism by which the utility escapes such a forceful regulatory grip and earns higher profit.

Profits of Capital-Goods Producers

It has been demonstrated that under a number of plausible circumstances higher price tags on capital goods leave the buyer either with

[14] For a valuable discussion of the concept "used and useful" in regulatory practice, cf. [6, pp. 363ff.].

[15] A *de facto* license to waste output completely rather than sell it at some price would be utilized by the utility only as an alternative to selling on an inelastic demand curve.

undiminished or even with higher profits. We must now briefly investigate the other side of the market. Are the circumstances that generate perverse profit responses for buyers perhaps precisely those that insure that a price-rigging conspiracy on the part of sellers is unprofitable? The answer to this question is an emphatic no.

In all the situations explored we have observed that increased capital-goods prices lower the quantity of derived demand for investment. Under pure original-cost valuation, however, when the equilibrium position occurs on declining or horizontal T_K-functions, conditions certain to produce the profit anomaly, the derived demand for capital goods is not even elastic. Total revenues of the seller do not decline when price is increased (they increase or stay constant); therefore his profits will surely rise. So at the same time that a buyer's profit position is unaffected or improved, the seller's profits rise.

The relationships between price of capital goods and total revenues of sellers can be easily deduced with the help of Figures 2 and 3. Total revenue of sellers, $W_I I$, is proportional to total annual capital cost of new investment $rW_I I = C_K - rW_K K^*$, the "rental" on new plant and equipment. Since in Figure 2 the distances labeled bc represent these costs, and b^3c^3 is necessarily greater than $\overline{bc}$, which in turn is necessarily equal to b^2c^2, the conclusion of the previous paragraph follows.

Under replacement-cost valuation, as Figure 3 demonstrates, derived demand for investment may be elastic even with equilibrium falling on a declining portion of the T_K-function. Whether a price increase profitable for the buyer is also profitable for the seller will, therefore, depend on the latter's costs. If the saving in cost from reduced sales is greater than the loss in revenue, the price increase pays. Thus certainly also in this regulatory situation both seller and buyer can profit from price conspiracies, though it is also possible that at some point an additional price increase may be profitable for the buyer yet unprofitable for the seller.

III. *Conclusions*

It has been the purpose of this paper to demonstrate rigorously that even under the simplest assumptions—the conventional model of a profit-maximizing monopoly firm with an added restriction limiting its rate of return—a public utility may not only fail to suffer a decline but actually experience an increase in profit if suppliers of capital goods collude to raise their prices. There are many ways in which the model and its analysis may be extended. Instead of just two inputs, there can be many; instead of nondepreciating, nonchanging capital goods, varying rates of depreciation and obsolescence can be introduced; instead of specified prices for inputs and a constant interest rate, one can make

allowances for market imperfections and other considerations that make these functions of quantities purchased; instead of the particular valuation formulas for determining the "fair rate of return," we can have many of its variants. No effort has been made here to incorporate such refinements, although the analytical framework for dealing with them has been presented. We do not need the complications for the purposes of this article. We study the simplest, standard model of a monopoly firm, one everywhere used by economists to explain and predict a wide variety of phenomena, to demonstrate our propositions. It would be easy to show our results under more specialized assumptions, and admittedly the critical ranges along the T_K-curve for which higher prices of capital goods fail to reduce profit may be sensitive to them. It is possible even to contrive special cases in which our results will be "unlikely." However, we show that, without first setting up a straw man, our propositions are logical consequences of profit-maximization.

Placed in the context of the electrical-machinery conspiracy cases, our analysis provides some understanding of and explanation for events that did in fact occur. At the same time the analysis has predictive power. It suggests, for example, in what types of regulatory situations the conspirators can be anticipated to have been most predatory and where least. And when the numerous litigations that have grown out of the conspiracies are concluded, much of the data will be in the record or will, it is hoped, be made available to test this and other predictions of the analysis.[16]

There are also a number of implications for public policy that follow from the analysis. In the aftermath of the electrical cases, there is here provided some guidance for the estimation of damages incurred and of their distribution between owners of utilities and their customers, on the one hand, and among customers, on the other. More generally, the analysis suggests that the capital-goods suppliers of other rate-of-return regulated industries may turn out to be fertile hunting ground for antitrust law violations. In the same vein it suggests that collusion may also be attractive in markets supplying "variable inputs" to firms in industries where operating margin rather than rate of return is the regulatory parameter; for by turning the analysis around one can show that in this regulatory situation in some circumstances buyers may welcome, or at least not object to, increases in wage rates and the prices of other variable inputs.

For the reader on search for a villain to share the stage with the electrical-equipment manufacturers, I suggest it is not that electrical

[16] The indictments involved a score of products with annual sales around $2 billion. Detailed information, including an almost complete enumeration of all transactions for a period of a dozen years, may ultimately be available to interested scholars.

utility which may have had benefit from the conspiracies, but the regulatory process itself. I have demonstrated that systems for regulating public utilities, widely adopted and defended, can create the environment conducive to conspiratorial practices. Their burdens, which fall on the supposed beneficiaries of regulation, deserve to be weighted prominently in judging the social desirability and effectiveness of a regulatory process.

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LONG CYCLES—FACT OR ARTIFACT?

By IRMA ADELMAN*

Both economists and economic historians generally agree that the economic growth of advanced capitalist economies did not proceed in a completely smooth, monotonic fashion. On the contrary, the evolution of industrial societies appears to have been characterized by recurrent waves of acceleration and deceleration in the levels and in the rates of growth of output and of other measures of economic performance. These waves, which vary between 10 and 20 years in duration, seem to emerge from the historical record after the effects of primary trends and short cycles have been largely eliminated by the use of various smoothing procedures.

Long swings of this nature have been isolated in many economic variables, including, among others, the outputs and prices of individual commodities [4]; the rates of growth of production in major industries and industrial groups [4]; the rate of growth of total industrial production and GNP [1]; construction activity [10]; immigration [14]; the rates of growth of the labor force and of population [14]; total gross and net investment [1]; and the rate of increase of productivity per man-hour and per unit of resources employed [1].

Nevertheless, the question of whether or not these longer movements constitute a class of economic phenomena independent of (though perhaps interacting with) the shorter cyclical fluctuations is not yet settled. Indeed, this issue has plagued every serious student of the longer swings ever since their existence was first recognized.

The answer to this question hinges upon two issues: (1) the nature of the forces which are responsible for the generation of the longer swings; and (2) the extent to which the smoothing procedures themselves are responsible for the apparent cyclical movements.

With respect to the first issue, one extreme point of view is that the

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amplitudes and durations of the longer swings have nothing to do with the structure of the economy, but are merely reflections of more or less randomly generated runs of abnormally severe recessions and unusually vigorous booms. If this position is correct, the detailed analysis of the longer waves from an economic point of view would appear to be of dubious value. At the other extreme, the existence of the longer swings is attributed solely to the operation of *economic* forces upon an interrelated economic mechanism constrained by certain physical and sociocultural factors which tend to delay and stretch out some types of economic response over extended periods of time. Under these circumstances, the analysis of long cycles would obviously constitute an important branch of both economic theory and economic history.

Actually, the position adopted by most writers lies somewhere in between the two extreme points of view just sketched. For example, Abramovitz concludes that:

> Although many features of the long swings in economic development can now be described, the cause of these fluctuations is still to be determined. It is not known whether they are the result of some stable mechanism inherent in the structure of the U.S. economy, or whether they are set in motion by the episodic occurrence of wars, financial panics or other unsystematic disturbances. Their pronounced uniformities, however, make it likely that continued study of long swings will shed light on the process of economic growth and on the origins of serious depressions [1, p. 412].

In an earlier paper [2] the author attempted to look into the question of the origin (and hence, indirectly, the significance) of the long swings. Specifically, in a simulation of the ordinary business cycles of the U.S. economy by a randomly shocked Klein-Goldberger model [13], long swings were generated which corresponded in all important respects to the extended waves observed in the U.S. economy.

The only major discrepancies found were the smaller absolute amplitudes of the Klein-Goldberger long cycles and the failure in the model economy of the retardation phase of the reference cycle to exceed the duration of the acceleration phase. The primary cause of the first of these differences is probably the omission of unusually strong shocks, such as that of World War II, from our data. And the second reflects either deficiencies of the Klein-Goldberger model, structural changes in the behavior of the system since 1929, or both.

One interpretation of the remarkable agreement between Klein-Goldberger long cycles and those of the U.S. economy is that the *purely* random explanation of the long cycle is not valid, and that the interactions among the several economic variables, as represented by, say,

the Klein-Goldberger model, are necessary in order to explain the observed lead-lag relationships and other properties of the long swings. On the other hand, in view of the paucity of data, both the U.S. long swings and those of our model are usually consistent with a random distribution of long-cycle durations.

An alternative point of view, therefore, is that the long swings *are* random in origin and that the observed lead-lag relationships are either accidental or else they merely reflect the lead-lag relationships found in the ordinary business cycles (regardless of their origin). An additional argument in favor of this conclusion is the fact that the smoothing procedures employed for the elimination of the effects of the shorter business cycles are common to both the empirical investigation of U.S. historical experience and to the Klein-Goldberger simulation experiment. The possibility therefore remains that the apparent longer swings are merely the combined results of the effects of shorter cycles and of the systematic biases induced by the smoothing practices.

The purpose of the present investigation is to determine whether the smoothing biases constitute in themselves a sufficient explanation for the existence of the long cycles. In this paper, the statistical technique of spectral analysis[1] is applied to historical data in an effort to investigate the existence and nature of the longer cycles. Since the spectral-density technique leads to the *simultaneous* determination of cycles of all durations, it does not require the elimination of shorter cycles from the series before cycles of longer duration can be studied. This feature of the present analysis is a consequence of the powerful statistical tools employed and represents an important advance over traditional economic procedures for time-series decomposition.

Since the theory of spectral analysis has been developed almost exclusively for stationary processes, one might expect, at first sight, that it could not be applied to economic time series. There exist schemes, however, to transform an economic time series, which represents a nonstationary stochastic process, into an equivalent series which is approximately stationary. The transformed series can then be analyzed by the usual spectral techniques.

In any event, the very fact that the time-series-analysis practices employed in the present investigation are very different in nature from the ones traditionally used by economic historians and business-cycle analysts offers an independent check upon the validity of their respective results.

[1] Good general discussions of the techniques of spectral analysis are to be found in references [3] [23]. Since most of these references require a certain degree of mathematical sophistication, a wholly nonmathematical exposition is presented in Section II.

I. *Traditional Techniques for the Analysis of Long Swings*

As has been indicated above, the techniques which have been utilized in the past for the analysis of long swings employ some sort of smoothing procedure to mask the effects of the short cycles. Kuznets, in his pioneering work on production and price series in the United States [16], used simple moving averages to eliminate trend and to smooth out the short cycles. His subsequent work on the national product [15] is based upon rates of change between overlapping decade averages. Both of these procedures are open to the charge that, unless the period chosen for the moving average corresponds precisely to the frequency of the short cycles, they will tend to introduce spurious cyclical fluctuations into the basic series.[2]

Burns's technique is open to similar objections. In his study of a large number of production series [4, p. 175], he first calculated ten-year growth rates at five-year intervals. The deviations of these rates of growth from the trend rate of growth formed the raw material for his study of "trend cycles."

The most sophisticated smoothing methods which have so far been applied are those introduced by Abramovitz [1]. He first calculates the average value of each series during successive reference cycles measured both from trough to trough and from peak to peak. The trend is then removed by computing the annual percentage rates of growth of each variable between average reference cycle standings. The trough-to-trough percentages are intermingled with the peak-to-peak percentages to yield a series of rates of growth between overlapping business-cycle periods. Oscillations in these rates of growth are taken to indicate the existence of alternating phases of acceleration and retardation in the rates of expansion of economic quantities. Incidentally, the Abramovitz technique was utilized in the Klein-Goldberger simulation experiment referred to above.

While the Abramovitz analysis exhibits no obvious systematic bias, the approach is too complex to permit ready analytic evaluation. Indeed, no a priori formula for his approach can be written down, and hence a direct assessment of the effects of his technique is not possible. Since, however, it has not been demonstrated that the method does not introduce spurious long cycles to a greater or lesser extent, it would be highly desirable to provide an independent check on the existence of such oscillations.

[2] In spectral terms, one can interpret the moving-average approach used by both Kuznets and Burns as a filtering process. (See Section II.D.4 below.) The effects of their smoothing procedures can therefore be evaluated by computing the transfer functions of the respective filters. It is not necessary to do this, however, since the difference equations implied by their smoothing processes have been solved exactly [20] and yield a ten-year cycle.

II. *The Technique of Spectral Analysis*

A. *Time Series and Fourier Series*[3]

Before we discuss the philosophy and techniques of spectral analysis, it may prove worthwhile to spend a few moments on another method of time-series analysis. Basically, there are two ways of looking at a time series. The more obvious one is that the series is a sequence of values of the variable as a function of time. The other is that the value of a time series at each time is the summation of a particular set of sinusoidal waves with frequencies which are integral multiples of some fundamental frequency. Each of these waves is characterized by an amplitude (the maximum value of the oscillation), a frequency (the fraction of a cycle completed in one time period), and a phase (the fraction of a cycle of a given frequency through which the sinusoidal wave must be displaced in order to have the value zero and positive slope at a particular point of time). For each frequency component it is possible to combine the amplitude and phase information into a single complex number, whose absolute value represents the amplitude, and the ratio of whose components determines the phase of that wave. Thus, if one specifies for a given fundamental frequency the complex amplitude $C(n)$ of the wave for each mutiple of the fundamental frequency, one has determined completely the time series $X(t)$. The values $C(n)$ are called the complex Fourier coefficients and are a representation of $X(t)$ in terms of frequency. The $C(n)$ constitute the complex spectrum of $X(t)$. The absolute values of the squares of $C(n)$ form the power spectrum of $X(t)$.

The Fourier-series approach has proved to be an extremely useful technique for the analysis of periodic time series. Attempts to apply these methods to economic time series, however, have met with a notable lack of success, most likely because actual economic time series are not strictly periodic in nature. Rather, after the removal of trend effects, they appear to consist of periodic functions upon which are superimposed strong stochastic variations. The reason that spectral analysis offers hope of being more appropriate for economics than Fourier series is that spectral analysis has been developed specifically for the study of functions of precisely this character.

B. *Basic Postulates*

The fundamental assumption underlying the development of spectral analysis, as contrasted with Fourier analysis, is that a time series is merely a single realization of a random function [23, Ch. 1, p. 2].

[3] A good exposition of Fourier analysis is given in [6].

That is, the variable $X(t)$ is probabilistic in nature, and the observed value of X at a particular time t is a sample chosen in some way from a universe containing all permissible values of X at time t. As t varies from the beginning to the end of the time interval in question, the sequence of values assumed by the variable X traces the observed time series, which is one sample function out of a whole ensemble of possible time sequences. Given the random function from which $X(t)$ is derived, then, the probability of occurrence (or the probability density) of any particular realization can be determined.

From a purely statistical point of view, however, the task of the time-series analyst is just the opposite—to infer from a single sample realization the relevant features of the random function from which this particular realization originated. Clearly, if the nature of the random process which gives rise to the sequence $X\ (t)$ is not constrained in some manner, the reconstruction of the random function is a hopeless task. In order to bypass this difficulty, the theory of spectral analysis specifies that the random function must be stationary and ergodic in time. In other words, it is assumed that the expectation value of all possible values of $X(t)$ at time t is independent of time and that the expectation value of the covariance[4] of $X(t)$ and $X(t+T)$ is a function only of the time difference T.

Under these circumstances it can be demonstrated that the mean value of a particular realization of $X(t)$ approaches, as the number of observations becomes large, the ensemble average at any point of time t. Similarly, the corresponding statement is valid for the covariance function of $X(t)$. This theorem, which is known as the ergodic theorem, has important practical implications, as it permits one to estimate from a single realization of the random process the mean and covariance functions of the stationary random function $X(t)$. If it is assumed also that the stationary random function is normally distributed, the mean value and the covariance function suffice to specify $X(t)$ completely.

C. *Spectral Representation of the Random Time Series*

Spectral-density analysis constitutes, in essence, an extension of Fourier analysis to the treatment of probabilistic processes. Basically, this approach assumes that a time series can be represented as a sum of individual sinusoidal waves whose frequencies are all multiples of a given fundamental frequency, and that the amplitudes and the phase displacements associated with each multiple of the fundamental frequency are random variables. The main purpose of spectral-density analysis is to determine the average value of the amplitude associated

[4] The convariance function of $X(t)$ and $X(t+T)$ is $R(T) = E[X(t) \cdot X(t+T)]$.

with each frequency of oscillation, and thereby to separate important cyclical components from insignificant ones.

As in the case of Fourier analysis, the power spectrum of a series is a function which specifies, for each cyclical frequency, the absolute value of the square of the complex amplitude. Since the expression for the power at a particular frequency is identical to the equation for the variance of that frequency component, the power spectrum might equally well be called the "variance spectrum." Furthermore, in view of the fact that each component of the spectrum is linearly independent of every other component, the power spectrum also specifies the contribution of each frequency component to the total variance. Spectral analysis is thus, in essence, an analysis of the variance of a time series in terms of frequency [21].

One may therefore define a spectral function which indicates, for each frequency, the percentage of the over-all variance of a series which is attributable to cycles of that frequency. This spectral function is known as the spectral density of a time series. For stationary random processes the spectral density of a time series can, in principle, be obtained from the original time series simply by means of a Fourier-series transformation of the normalized autocovariance function of $X(t)$.

Since spectral-density analysis is based upon an explicitly formulated probabilistic model, it permits us to apply statistical significance tests to the individual spectral components.[5] That is, it enables us to answer the question: Is the contribution of cycles of a particular duration significantly different from zero?

D. Some Practical Considerations[6]

1. *Nonstationarity.* A crucial problem which arises whenever one attempts to apply the spectral-density approach to economic time series is that the statistical model has been developed only for stationary random processes. Since processes whose expected value and covariance are time-dependent cannot be treated in the usual formulation of this model, the analysis of economic activities, which are generally characterized by a secular trend in both mean and variance, requires some modification of the theory. The obvious technique for handling this difficulty, which is to operate upon first differences of the data, reduces the signal-to-noise ratio and therefore does not always lead to useful results. An alternative method that has been used to eliminate trend, which is the one adopted in the calculations that follow, is to fit a least-square linear trend to the logarithms of the original data. The

[5] Some relevant statistical tests are given in [3, pp. 22-23].

[6] The organization of the exposition of this section is patterned after that in [18].

deviations from this fitted trend are then used as input data to the spectral-density analysis. Although both approaches have advantages, the second method was chosen because it was felt to be closer to the basic purpose of this analysis—to decompose oscillations around a trend. The logarithmic transformation is used to make the autocovariance functions more nearly independent of time.

2. *The Sample Size.* A second difficulty which almost always arises in the application of spectral analysis to economic time series is a very severe limitation on the number of observations available for study. This problem is particularly serious in the context of the investigation of long cycles. Continuous aggregative yearly economic statistics of reasonable reliability are available only from 1889 on. Since the cycles with which we are concerned in this paper are typically 10 to 20 years in length, the effective number of cycles available would appear to be at most seven. This number of cycles is far too small for an accurate statistical analysis of the cycle length. However, to the extent that business cycles constitute movements which affect the entire economy, as distinct from the summation of unrelated oscillatory movements in individual economic time series, they represent fluctuations "occurring at about the same time in many economic activities" [20, p. 3]. That is, they pervade virtually all parts of the economy and exhibit more or less systematic lead-lag patterns. We would therefore expect the qualitative characteristics of the power spectra of the observed variation around the trend of each of the several indicators of economic performance to be similar in nature. The existence of such a family resemblance, while not increasing the effective number of cycles available, will tend to reinforce the extent of one's belief in the qualitative validity of the results. Even this reinforcement one must accept with caution, however, as the existence of common procedural biases in the several series and/or in the estimation of their spectra would also account for at least some of the similarity in parallel series.

3. *Spectral Averages and Spectral Windows.* The technique for the derivation of the power spectrum based upon a Fourier transformation of the autocovariance function is appropriate only for the investigation of time series whose components have frequencies, amplitudes, and relative phases which do not vary statistically. In order to obtain statistically consistent results when the time series under consideration are not strictly periodic, it is necessary to estimate the *average* power in a frequency band centered around the frequency in question, rather than the power associated with a precise frequency. In principle, one would like to minimize the width of the frequency band. However, it can be shown [19, p. 180] that, no matter how many observations are taken, the variance of any estimate of the average power increases as

the width of the frequency band over which the power is measured is reduced.

This fundamental indeterminancy principle implies that, in practice, a compromise must be made between the accuracy with which the average spectrum is estimated and the accuracy with which the frequency interval to which the spectral estimate applies is specified. The nature of this compromise depends, to some extent, upon the precise average chosen [11, pp. 145-48]. In practice, the averaging is accomplished by weighting the covariances before deriving the spectral estimates by Fourier transformation. The weighting function selected for this purpose is called the *lag window* in the time domain; its Fourier transform, the representation of the lag window in the frequency domain, is known as the spectral window.

An ideal spectral window would (1) assign equal weight to power at all frequencies in a band of width equal to the fundamental frequency centered about a particular frequency, and (2) assign zero weight to power outside that frequency interval. While it is mathematically impossible to devise a lag window of such a shape with a finite amount of data, several approximations to this ideal have been proposed.

In order to discuss the properties and effects of the lag window profitably, we must first define what is meant by the term "number of lags." The autocovariance function, as defined in footnote 4, is calculated by multiplying the value of a series at one time by its value some number of periods later, and then summing over all points of time for which both factors are defined. In principle, the maximum possible difference in time between the two factors is one less than the number of data points for which the series is specified. However, with a finite amount of data, the smaller the time difference between the two factors, the larger the number of points which can be included in the summation. On the other hand, the larger the time difference, the better the lag window approximates the ideal. The largest value of the time difference actually used in the calculation of the autocovariance function is called the number of lags.

The number of lags is a crucial parameter, as it determines, among other things, the bandwidth of the lag window and the number of degrees of freedom of the estimate, both of which are inversely proportional to the number of lags. Since the effective number of data points increases as the number of lags is reduced, the choice of the number of lags represents a compromise between the reliability with which one can specify the general shape of the power spectrum and the accuracy to which one can determine the frequencies at which the power is concentrated. A relatively small number of lags (15) was chosen for the

present work, because it was felt that, in the investigation of the existence of long swings, the localization of the precise periods of the cycles involved is far less important than the determination of the relative amounts of power in the long and short cycles. For purposes of comparison, preliminary calculations were also made with other choices of the number of lags.

The lag window used in the present investigation was suggested by Parzen.[7] It was selected because, for a given number of data points and a given number of lags, this window has the property of having the smallest variance and the largest bandwidth of all the windows which have been suggested so far. It also introduces the smallest dis-

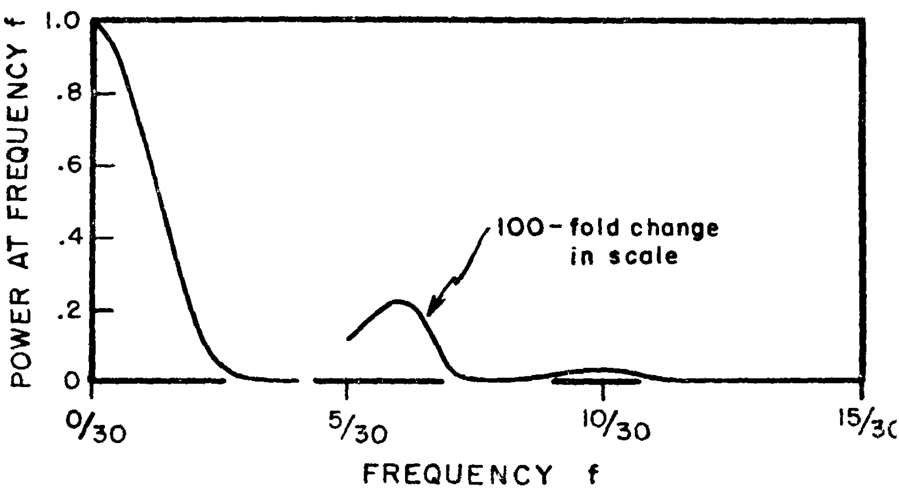


FIGURE 1. NORMALIZED PARZEN WINDOW (15 lags)

tortion of spectral estimates at distant frequencies. This choice of window is, of course, consistent with the criteria given in the previous paragraph.

Figure 1 shows the Parzen spectral window with 15 lags, which is the window used in the present analysis.[8] The window is symmetrical about the origin. On the horizontal axis is measured the distance in fractions of cycles per year from the frequency about which the esti-

[7] Specifically, if m is the number of lags for which the autocovariance function is computed, the weight assigned to the autocovariance computed with a lag of k is given by

$$\begin{array}{ll} 1 - 6k^2/m^2(1 - k/m), & 0 \leq k \leq m/2 \\ 2(1 - k/m)^3, & m/2 \leq k \leq m. \\ 0, & k \geq m \end{array}$$

[8] The formula for the computation of the spectral windows appears in [19, p. 146].

mate is centered to the frequency of interest. The vertical axis indicates the relative weight accorded to power at the latter frequency. Thus, if the true power were concentrated in a small frequency interval Δf about some particular frequency f_1, the lag window of Figure 1 would produce an apparent spectrum consisting of a set of lines (of finite widths) whose relative amplitudes can be determined from the figure. In Table 1 several of the numerical values are given. True power at any other frequency, of course, would be distributed among its neighboring frequencies in a similar (additive) manner.

4. *Filtering.* As was mentioned earlier, all lag windows devised for use with a finite amount of data must inevitably assign some nonzero weight to frequencies outside the desired interval. This deficiency is

TABLE 1

Frequency	Amplitude
f_1	1.0
$f_1 \pm \frac{1}{30}$	0.658
$f_1 \pm \frac{2}{30}$	0.165
etc.	

particularly serious in the case of economic time series, as a failure to remove trend completely (which is not unlikely in the case of real world data) will be reflected in the existence of apparent high power at frequencies close to zero. In the context of the present investigation of the long-cycle (low frequency) components of economic series, this difficulty is exacerbated.

To reduce the distortion of the spectrum due to leakage of power from frequencies below those of interest it is desirable to base the spectral analysis upon series from which most of the low-frequency power has been removed. This can be accomplished by subjecting the input data to a mathematical filtering process, which selectively reduces the power in the undesired portion of the frequency range.[9] The spectral analysis is then applied to the filtered data. Various filters have been devised for this purpose. Common to all of these is the disadvantage that some, usually significant, portion of the original data is removed

[9] Mathematical filters, just like electrical filters, can be designed to pass or exclude arbitrary finite or infinite portions of the spectrum.

in the filtering process. The filter used in the present investigation, which was suggested by Parzen,[10] is designed to filter out most of the power at frequencies lower than 1/18 of a cycle per year.

In Figure 2, the filtered (solid line) and the unfiltered (dashed line) power spectra of GNP are plotted for comparison. The vertical scale is the logarithm of the power associated with the frequency given on the horizontal axis. A glance at the two curves suggests that the filter used has indeed accomplished its purpose. At the first two frequencies, the

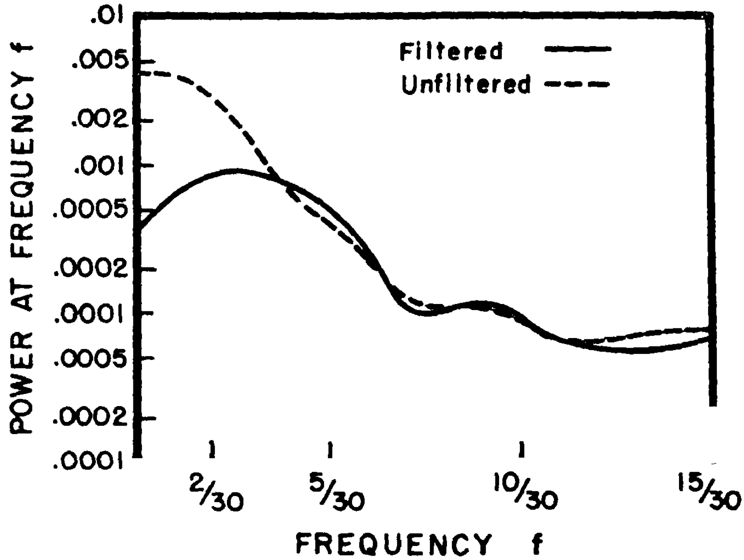


FIGURE 2. POWER SPECTRUM OF NATURAL LOGS OF DEVIATIONS FROM TREND OF GNP

power of the filtered spectrum is much smaller than that of the unfiltered one; in the 10-15 year cycle range the filtered spectrum is significantly lower than the unfiltered, primarily because the distortion from leakage of power from the lower frequencies through the window has been greatly reduced. This should lead to a fairly conservative estimate of the power in the 10-15 year region. The relatively minor differences between the two spectra at high frequencies are probably due mostly to sampling fluctuations, as the filtered series is based on 18 fewer data points than the unfiltered series.

[10] The filtered series $Z(t)$ is formed from the original data $X(t)$ by setting

$$Z(t) = X(t) - \sum_{T=-9}^{T=9} \frac{\text{Sin } \pi T/9}{\pi T} X(t+T).$$

The term $(\text{Sin } \pi T/9)/\pi T$ is a typical term of the Fourier transform of a rectangular frequency function which has an amplitude of $+1$ in the open interval $\pm 1/9$ cycle/year and is zero outside that interval. See [3, p. 68].

Some additional insight into the effects of the filtering process may be gained by looking at the graph of the transfer function of the filter (i.e., the squared modulus of the frequency response function of the filter). It can be shown that the power spectrum of the output from a linear filter is the product of its input spectrum and the transfer function of the filter. Division of the filtered spectrum by the transfer function of the filter therefore results in an estimate of the power spectrum of the original (unfiltered) series.

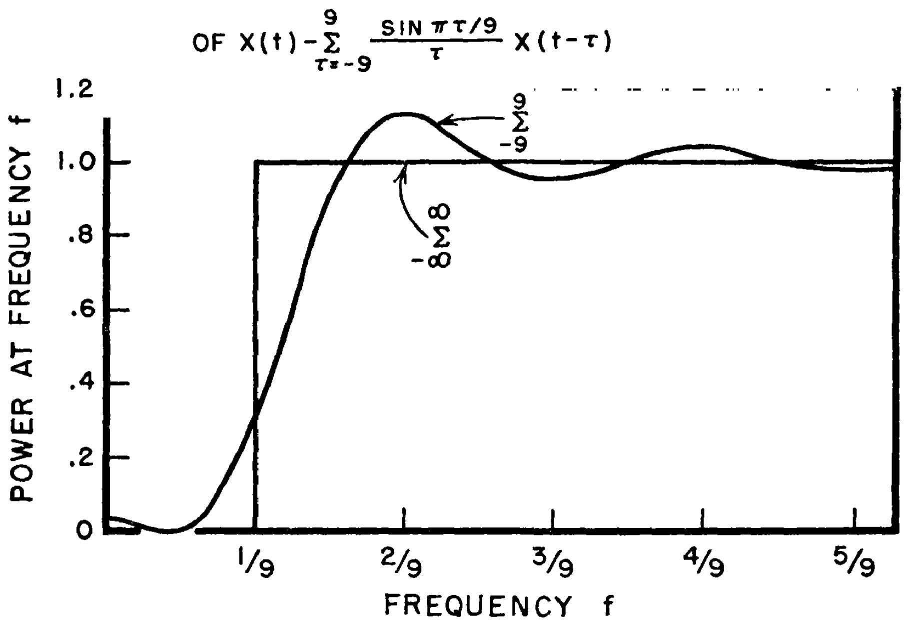


FIGURE 3. POWER TRANSFER FUNCTION

The dashed line in Figure 3 represents the transfer function of an ideal filter, while the solid line indicates the transfer function of the filter used in the present investigation. One can see that, in contrast to an ideal filter, the filter used in this work permits some power to pass through at the very low frequencies and multiplies the power just above the "cutoff" frequency by a factor close to unity.

III. *Empirical Results*

Figures 4 to 11 portray typical power spectra calculated for output, investment, consumption, employment, capital stock, productivity of labor, productivity of capital, and the wholesale price index. Many series pertaining to each economic variable were used (see the Appendix for the data sources and precise identification of each series). How-

ever, since all the power spectra were qualitatively quite similar, only a single representative spectrum is plotted for each type of series. The power spectra plotted in Figures 4 to 11 were derived by first calculating the filtered spectrum of each individual time series and then dividing the filtered spectra by the transfer function of the filter.

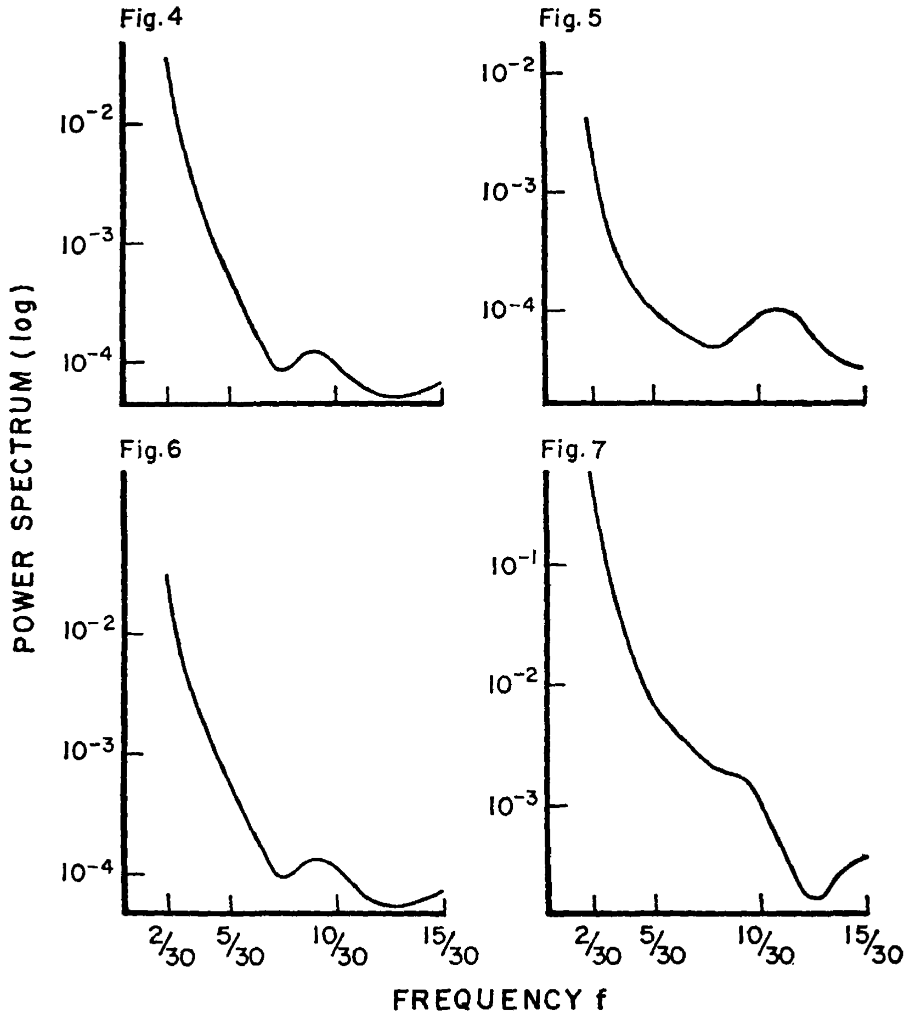


FIGURE 4. POWER SPECTRUM OF NATURAL LOG OF DEVIATIONS FROM TREND OF OUTPUT SERIES

FIGURE 5. POWER SPECTRUM OF NATURAL LOG OF DEVIATIONS FROM TREND OF PRODUCTIVITY OF LABOR

FIGURE 6. POWER SPECTRUM OF NATURAL LOG OF DEVIATIONS FROM TREND OF PRODUCTIVITY OF CAPITAL

FIGURE 7. POWER SPECTRUM OF NATURAL LOG OF DEVIATIONS FROM TREND OF INVESTMENT

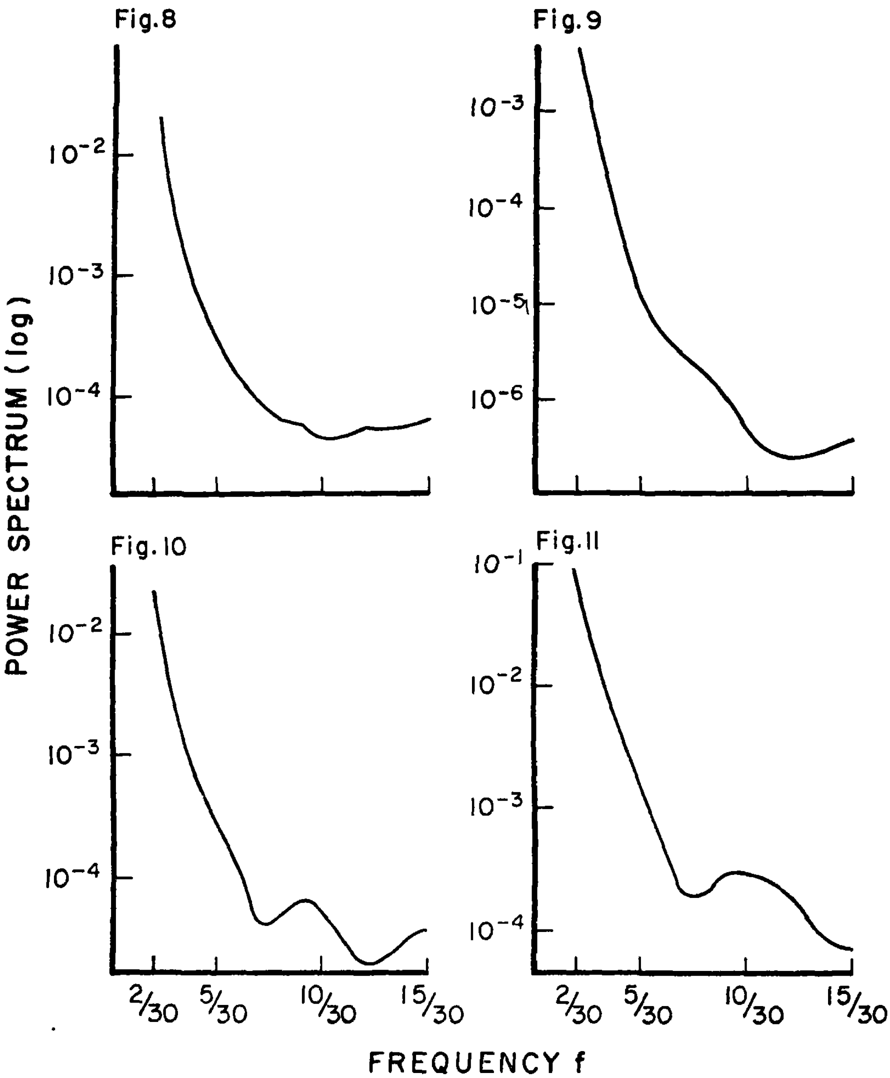


FIGURE 8. POWER SPECTRUM OF NATURAL LOG OF DEVIATIONS FROM TREND OF CONSUMPTION

FIGURE 9. POWER SPECTRUM OF NATURAL LOG OF DEVIATIONS FROM TREND OF CAPITAL STOCK

FIGURE 10. POWER SPECTRUM OF NATURAL LOG OF DEVIATIONS FROM TREND OF EMPLOYMENT

FIGURE 11. POWER SPECTRUM OF NATURAL LOG OF DEVIATIONS FROM TREND OF WHOLESALE PRICE INDEX

The spectra for a number of other economic indicators were also calculated, but were excluded from later analysis because of some suspicion as to their reliability. Two of these series are worthy of special comment. The construction spectrum was omitted because it showed an unusually large amount of distortion at the higher frequencies due to the filtering process. The power spectrum for population (Figure 12), which is of great interest in the context of long swings, suggests that any genuine cyclical phenomena which may exist in population are longer in duration than 15 years. More work is required on this series before any more detailed conclusions can be drawn.

The filtered spectra of Figures 4 to 11 offer no evidence for the exis-

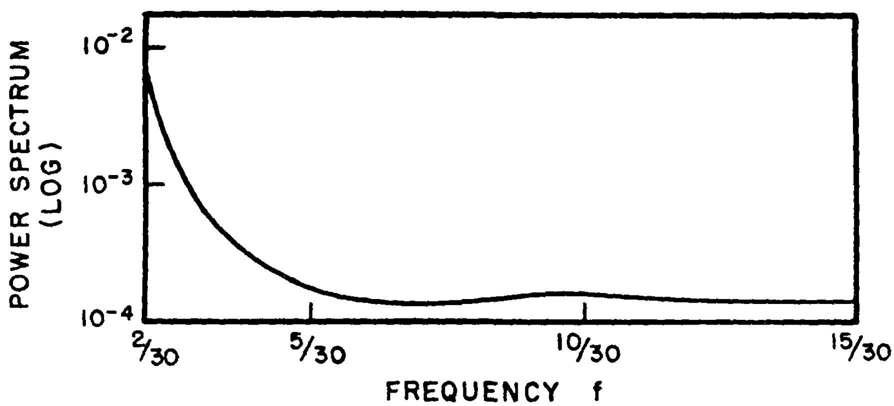


FIGURE 12. POWER SPECTRUM OF NATURAL LOG OF DEVIATIONS FROM TREND OF POPULATION

tence of a long-cycle component in the business fluctuations of the U.S. economy since 1890. Rather, one can account for the entire variance in the long-wave regime by attributing it solely to leakage from power at frequencies lower than 1/18 of a cycle per year. More specifically, when the effects of random fluctuations are smoothed out by spectral techniques, the power which remains in the long-swing domain appears to be traceable to the difficulty of removing the entire trend from the data which were analyzed. In view of this result, it is likely that the long swings which have been observed in the U.S. economy since 1890 are due in part to the introduction of spurious long cycles by the smoothing process, and in part to the necessity for averaging over a statistically small number of random shocks.

The evidence of the present spectral analysis is not at all inconsistent with certain other observations on the existence of long cycles:

1. The distribution of long-cycle durations found by Abramovitz

and others is consistent with a random distribution of frequencies [2, p. 175].

2. The beginning of the long cycles is, in almost all instances, directly associated with a strong exogenous shock, such as a war or a financial crisis [1, p. 412].

3. Despite the fact that most of the economic interactions which have been considered necessary for the generation of long cycles in the U.S. economy are absent in the Klein-Goldberger model, it has proved possible, by imposing a series of random shocks upon that model, to generate fluctuations which reproduce, in almost all important respects, the characteristics of the long swings observed in the U.S. economy [2].

These considerations together with the results of the present spectral analysis suggest that, unlike the case of the usual business cycle, the apparent frequencies of response of the economy to a large shock depend upon the nature, magnitude, and timing of the shock. One is therefore strongly tempted to conclude that the economic mechanisms inherent in the modern U.S. economy do not tend to generate long cycles.

Appendix

Output Series[a]
(Indices, 1929 = 100)

1. Net National Product, Kuznets
2. Gross National Product, Kuznets
3. Net National Product, Department of Commerce
*4. Gross National Product, Department of Commerce
5. Real Gross Product, Private Domestic Economy, Nonfarm
6. Output in Mining
7. Net Output in Agriculture
8. Output in Manufacturing

Employment Series
(Indices, 1929 = 100)

1. Number of Persons Employed in the Production of the Net National Product, Kuznets
2. Number of Man-hours Worked for the Production of the Net National Product, Kuznets
3. Number of Persons Employed in the Production of the Real Gross Product, Private Domestic Economy, Department of Commerce
*4. Number of Man-hours Worked for the Production of the Real Gross Product, Private Domestic Economy, Department of Commerce

[a] Starred series are those for which the power spectrum is reproduced in Figures 4 to 12.

5. Number of Persons Employed in the Production of Nonfarm Real Gross Output, Department of Commerce
6. Number of Man-hours Worked for the Production of Nonfarm Real Gross Output, Department of Commerce
7. Number of Persons Employed in Manufacturing
8. Number of Man-hours Worked in Manufacturing

Capital Stock Series[a]
(Hundreds of millions of 1929 dollars)

1. Capital Stock, National Economy
*2. Capital Stock, Domestic Economy
3. Capital Stock, Total Structures

Productivity of Labor[a]
(Indices, 1929 = 100)

1. Output per Person, Private Domestic Nonfarm, Department of Commerce
2. Output per Man-hour, Private Domestic Nonfarm, Department of Commerce
*3. Output per Unit of Labor Input, Private Domestic Nonfarm, Department of Commerce
4. Output per Person, Gross National Product, Department of Commerce
5. Output per Man-hour, Gross National Product, Department of Commerce
6. Output per Unit of Labor Input, Gross National Product, Department of Commerce
7. Output per Person, Net National Product, Kuznets
8. Output per Man-hour, Net National Product, Kuznets
9. Output per Unit of Labor Input, Kuznets
10. Output per Man-hour, Manufacturing
11. Output per Unit of Labor Input, Manufacturing

Productivity of Capital[a]
(Indices, 1929 = 100)

1. Output per Unit of Capital, Net National Product, Kuznets
*2. Output per Unit of Capital, Gross National Product, Department of Commerce
3. Output per Unit of Capital, Private Domestic Nonfarm Gross Product, Department of Commerce

Investment Series[a]
(Hundreds of millions of 1929 dollars)

1. Gross Private Domestic Investment, New Construction and Equipment, Kuznets

Sources:

[a] [5]; period 1889-1957.

*2. Gross Private Domestic Investment, New Construction and Equipment, Department of Commerce

Consumption Series[a]
(Hundreds of millions of 1929 dollars)

*1. Total Consumption Expenditures, Kuznets

Wholesale Price Series,[b] Bureau of Labor Statistics
(Indices, 1926 = 100)

*1. All Commodities
2. Hides and Leather
3. Textiles
4. Fuel and Lighting
5. Metal and Metal Products
6. Building Materials
7. Chemicals and Allied Products
8. House Furnishings
9. Farm Products
10. Foods

Population[c]
(Thousands of people)

*1. Total Population Residing in the United States

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THE MONETARY INTERPRETATION OF HISTORY

A Review Article

By JAMES TOBIN*

This monumental "monetary history of the United States"[1] since the Civil War is at the same time a critical history of monetary events, institutions, and policies and a monetary interpretation of the general economic—and even political—history of the century. I shall discuss these two aspects of the work in turn, although it is impossible to keep them entirely separate. In a sense the first aspect is the determination of the stock of money, M, while the second concerns the stability of the velocity of money, V. In the third and final section I shall review some of the judgments of the authors concerning particular episodes in the history of monetary policy.

I. *The Stock of Money*

Milton Friedman and Anna Schwartz (F&S henceforth) provide a statistical account of the stock of money in the United States since 1867. Much of this is new, the product of long and painstaking statistical research. The profession is greatly indebted to them for constructing monetary series homogeneous in concept and definition over so long a span of time. The numerical account is dexterously and gracefully interwoven with a history of monetary institutions, legislation, policies, personalities, and politics. The resulting narrative is fascinating and absorbing, and it is written in a consistently lucid and lively style.

What is money? The "money" whose stock F&S trace and explain consists of currency and commercial bank deposits held outside the federal government and the banks. The main questions raised by this definition are these: Why are time and savings deposits in commercial banks, which are not means of payment, included? If they are included, why are similar claims on other financial institutions—notably deposits in mutual savings banks and shares in savings and loan associations—excluded?

On the first question, a decisive practical answer is that it is evidently impossible to distinguish time from demand deposits in commercial banks prior

* The author is professor of economics at Yale University. A version of this review article was given at the American Bankers Association Conference of University Professors, Princeton, N.J., September 1, 1964. In revising the paper, the author has benefited from the discussion at the conference, which included comments by James Duesenberry, Allan Meltzer, Milton Friedman, and Anna Schwartz. He is especially grateful to Friedman for providing a written version of his remarks. L. Charles Miller and Marshall Pomer carried out the calculations.

[1] Milton Friedman and Anna Jacobson Schwartz, *A Monetary History of the United States 1867-1960*. National Bureau of Economic Research, Studies in Business Cycles, No. 12. Princeton: Princeton University Press, 1963. Pp. xxiv, 860. $15.00.

to 1914. But F&S do not stand on this answer. They do not think that their inability to exclude deposits not subject to check impairs the utility or relevance of their series for the stock of money. They cannot contend, of course, that their *M* measures the stock of means of payment, but they do not regard this as a defect.

More basic, in their view, is a concept of money as "a temporary abode of purchasing power enabling the act of purchase to be separated from the act of sale" (p. 650). I am not sure what this means; on its face the concept seems to allow all forms of wealth, all stores of value, to qualify as money. Clearly purchasing power can find temporary abodes other than currency and commercial bank deposits, for example in other savings institutions.

F&S recognize that, once the means-of-payment criterion is dropped, drawing the lines that define "money" is a matter of expediency. What statistical quantity works best? That is, what measure bears the closest and most predictable relationship to measures of economic activity? This is fair enough scientific procedure. But such open-minded pragmatism in the concept and definition of money is an unconvincing prelude to policy conclusions which stress the overriding importance of providing money in precisely the right quantity. Sometimes Friedman and his followers seem to be saying: "We don't know what money is, but whatever it is, its stock should grow steadily at 3 to 4 per cent per year."

F&S are entitled to use words the way they please, and their "money" is a very worthwhile magnitude to measure. Being an aggregative economist by nature myself, I am not as disposed as many critics might be to point out how much information any particular aggregate conceals. All global measures do conceal information. That is their virtue as well as their vice, and the task of science, in economics as elsewhere, is to find and devise aggregates which retain mostly essential information and discard mainly irrelevant information.

Nevertheless the central place which F&S give their money stock in theoretical analysis, historical interpretation, and policy recommendation invites critical scrutiny. Imagine a balance sheet expressing on one side the financial claims of the rest of the economy on the federal government (including the Federal Reserve) and the commercial banking system and on the other side the debts of the public to the government and the banks. (The balancing item is that amount of private net worth represented by the net debt of the central government to the public.) Both the government and the commercial banks have demand liabilities to the public, currency and demand deposits. Both have time liabilities to the public, securities and deposits. The total on which F&S focus is the sum of the government's demand liabilities to the public and all of the commercial banking system's deposit liabilities, time as well as demand. In their view, this seems to be the only feature of the consolidated balance sheet which matters.

Do F&S really think that the composition of this magnitude is of no consequence? Do they, for example, expect the velocity of a given *M* to be the same after a shift from demand to time deposits? And is their answer to this question the same whether such a shift is the autonomous result of a

change in preferences or the induced effect of an increase in time-deposit interest rates? In special cases they recognize that compositional shifts are not neutral. They argue, for example, that shift to currency induced by bank failures will raise velocity—that is, it will reduce the demand for money because currency is an imperfect substitute for the safe deposits it replaces. But this attention to special cases suggests that there may be general and systematic compositional effects which the authors have ignored. I shall return to this question in Part II in discussing the stability of velocity.

What about the liabilities omitted from M, the interest-bearing government debt held by the public? Are its size and composition of no monetary consequence? F&S tend to take an extreme either/or black-or-white view. Generally they do not regard this debt as money or as affecting the significance for economic activity of the liabilities that are money. But there is an important exception. In 1942-51, Federal Reserve support of government security prices made them the equivalent of money, indeed of high-powered money. The true money stock should include these securities, valued at their support prices. By the same logic the 1951 Accord would abruptly shrink money to its usual constituents.[2]

I think most readers will agree with me that this is farfetched. There is uncertainty about government security prices in normal times, but it does not prevent them from being good substitutes for bank deposits. This is especially true of short maturities, but it is true of any maturities the holders can match with their own future-payments schedules. Uncertainty was doubtless reduced, but it was not eliminated, by the Fed's wartime support commitment. There was considerable doubt, justified in the event, that the policy would be permanently continued. There is no evidence—either in interest rates on government obligations or in velocity figures—of such radical and abrupt revisions of public attitude towards government securities.

Moreover, I cannot see the logic which makes F&S so anxious to assimilate completely to money marketable government securities temporarily supported at par, and so reluctant to assimilate to money the liabilities of thrift institutions which are always "supported at par."

Finally, are F&S justified in neglecting the asset side of the consolidated balance sheet of the government and the commercial banking system? The authors are strongly opposed to giving attention to "credit" as against "money." The word "credit" in this dichotomy has a host of meanings, whose only common bond is concern for features of bank operations and financial markets other than the quantity of money. In Federal Reserve history, credit policy was long associated with the real-bills fallacy written into its very charter. According to this doctrine, the Federal Reserve could and *should* enable the banking system to meet the legitimate needs of trade and industry to finance productive activity, so long as its credit was not used for speculation

[2] "The support program converted all securities into the equivalent of money" (p. 563). The authors are more cautious in discussing, on page 598 and page 625, the consequences of the Accord.

or for unsound accumulation of inventories. This hardy tradition survives to this day insofar as the state of confidence or anxiety over the quality and direction of credit influences general monetary policy. F&S are properly critical of this tradition and of the surprisingly complete neglect of the quantity of money in prewar Federal Reserve theory.

The real-bills "credit" tradition also neglects interest rates. For example, the credit situation is judged satisfactory if all borrowers of good credit standing are being accommodated, regardless of the rates and other terms prevailing in the market. All that matters is equality of supply and demand, the absence of queues, regardless of the price and quantity at which the market is cleared.

I stress this point because F&S lump under the same heading—that of excessive attention to "credit"—all concerns with the interest-rate effects of monetary measures. Indeed they blame Keynes for elevating "credit" above "money" because of the role of the long-term interest rate in the *General Theory*—even though the only way to get at the interest rate in the Keynesian model is to manipulate M.

Personally I think that interest rates rank high among the gauges that measure the impact of monetary policies and conditions on economic activity, and that central bankers surely ought to consider the interest-rate effects of their policies. But whether this view is right or wrong, it is certainly not cut from the same cloth as the real-bills and credit-quality fallacies.

Interest rates aside, does the composition of bank assets make no difference? Will the effect on economic activity be the same whether a given increase in the money stock reflects (a) commercial loans by banks to private-business borrowers, or (b) exchange of bank certificates of deposit for Treasury bills previously held by the public? The monetization of commercial loans (or really indirectly of the inventories of goods which they finance) seems to me to be alchemy of much deeper significance than semimonetization of Treasury bills. By this I mean simply that I would expect (a) to stimulate more spending on GNP than does (b). If so, the same M packs a bigger wallop if it is the counterpart of operations like (a) than if it is the result of asset swaps like (b). You will never detect the difference if you confine your attention to the liabilities of the banking system.

The proximate determinant of the stock of money. F&S explain the irregular growth of the stock of money, M, in terms of three "proximate determinants": (1) the stock of high-powered money, H, i.e., currency and Federal Reserve deposit liabilities held outside the federal government; (2) the ratio D/C of the public's commercial bank deposits, D, to the public's holdings of currency, C; and (3) the ratio D/R of deposits owned by the public, D, to the total high-powered money reserves, R, of the commercial banking system. It is purely arithmetic tautology to express the stock of money in terms of these three factors and to explain its development over time by variations in these "proximate determinants." Since $M = D + C$ and $H = R + C$,

$$M = H\left\{\frac{\frac{D}{R}\left(1+\frac{D}{C}\right)}{\frac{D}{R}+\frac{D}{C}}\right\}.$$

F&S breathe life into this tautology as they trace the three factors over the century.

The concept of "high-powered money" is indispensable to understanding a monetary and banking system like that of the United States. The essential feature of the system is the commitment of the banks, with only fractional reserves of currency, to maintain convertibility at par and on demand between their deposit liabilities and currency. Since 1914 the major portion of bank reserves has not been literally in the form of currency but rather in deposits in Federal Reserve Banks. But this difference of form is inconsequential, because the Federal Reserve maintains for banks two-way convertibility between these deposits and currency.

In the United States today the government—i.e., the Treasury and the Federal Reserve together—determines the quantity of high-powered money. The behavior of the public and the banks determines how much of this stock is in circulation as currency and how much serves as reserves for multiple creation of deposits by the banking system. The authors do not say so explicitly, but the logic of the three "proximate determinants" evidently is this: the stock of high-powered money is determined by the government, the deposit-currency ratio by the public, and the deposit-reserve ratio by the banks. Of course this is an oversimplification, because each of the three sectors can, at least indirectly, affect all of the determinants. For example, the government, by determining reserve requirements, strongly influences the deposit-reserve ratio which the banks seek and achieve. Nevertheless I agree that this is an illuminating way to discuss the determination of the stock of money.

For the Federal Reserve era, I would find it more illuminating, and more in keeping with the spirit of the scheme, to exclude from high-powered money reserves borrowed by member banks from the Federal Reserve Banks. Then member-bank decisions to use the discount window would affect the deposit-reserve ratio rather than the stock of high-powered money. The amount of borrowing is of course influenced by Federal Reserve policy in setting the discount rate and administering the discount window. But the initiative is the banks', and it seems to me more natural to regard decisions to borrow reserves in the same light as decisions to hold smaller excess reserves. This, however, is a matter of taste and analytical convenience rather than of principle.

The stock of high-powered money. The federal government's control—even its proximate control—over the stock of high-powered money was considerably tightened by the establishment of the Federal Reserve System in 1914. In earlier times national banks could expand the supply of high-powered money on their own initiative by issuing national bank notes; they never exploited their note-issue privilege to the legal maximum permitted. (Since note issue

appears to have been profitable, the authors frankly say that this is a puzzle they cannot explain [p. 23].) Private gold transactions provided another gap in government control of high-powered money. Banks needing reserves, for example, could buy gold in London. For the system as a whole this expedient was limited by the "gold points" in periods when the United States was on the gold standard. But in the greenback period, before 1879, the banks could in effect increase their high-powered money reserves by depreciating the dollar relative to gold and sterling. I am indebted to F&S for these points, but I do not think they stress sufficiently their implications—i.e., the government's control over the stock of high-powered money was relatively loose over much of the period which they cover.

Governmental actions to increase the stock of high-powered money have been of three kinds: (1) purchase and monetization of gold, (2) printing or coining of currency to meet other government expenditure, including the purchase of silver, and (3) the extension of Federal Reserve credit through open-market purchases of securities, purchases of acceptances from banks, or discounting of paper for member banks. The reverse actions, of course, diminish the stock. Although these various sources of high-powered money are arithmetically equivalent, given the values of the two ratios, in their effects on the stock of money, I think it is misleading to regard them as economically equivalent.

In some cases high-powered money is created as a by-product of income-generating expenditures by the government or by foreign purchases of U.S. exports. In others, high-powered money arises simply from exchanges of assets between the government or central bank and private banks or individuals. In the former cases, the new high-powered money also reflects additions to private net worth; in the latter cases, it does not. Some of the expansionary economic consequences of growth in high-powered money may be due to income and wealth directly generated. These cannot be duplicated by purely monetary policy, which is confined to the third of the three ways of engineering increases in high-powered money. You cannot repeat the consequences for employment, income, and spending of purchasing a million dollars of newly mined gold by purchasing a million dollars of old government securities—even though both operations increase high-powered money by a million dollars.

The deposit-currency ratio. Over the long run this ratio reflects the habits, institutions, and preferences of the community with respect to the use of currency and bank deposits. Over most of the short runs in this history, variations in the ratio reflect the fluctuating confidence of the public in banks' ability to maintain convertibility of their deposits into currency.

Secularly, banks gained steadily and dramatically at the expense of currency until 1929, although most of the gains after 1915 were in time deposits rather than in the ratio of demand deposits to currency in public use. But the banks have never restored the position they lost in the Great Depression and World War II. Although heavier income taxation has presumably promoted the use of currency, the success of deposit insurance and the general growth of income and wealth should have favored the use of banks. Bankers might

well ask themselves why their liabilities are not as preferred a medium of exchange as they were thirty-five or forty years ago. Could service charges be a factor?

Much monetary history concerns the maintenance of convertibility between deposits and currency, and it is a dismal record of panics, crises, failures, and lessons never learned. The worst episode was of course that of 1930-33. The Federal Reserve System, established precisely to defend the monetary and banking structure against an "internal drain," failed utterly to do so. F&S make a convincing case that a better job would have been done without the Fed. Following earlier precedent, the banks would have stayed open while the conversion of deposits into currency was temporarily restricted. This would have been done, the authors think, as early as 1930; and this timely therapy would have prevented the subsequent disastrous scrambles for liquidity.

However this may be, the authors are surely right to regard federal deposit insurance as the real remedy, and therefore as the most important banking reform since the National Banking Act nationalized the issue of currency. Ironically enough, deposit insurance was stubbornly opposed as unsound by the banking fraternity it has so greatly benefited.

The deposit-currency ratio is broadly descriptive of the community's balance of preference between currency and deposits and of the state of confidence in banks. It is often used in another sense, as a parameter in calculation of the increase in money stock to be expected from a dollar's increase in high-powered money. I have doubts about this use of the ratio. It implies behavior that does not seem plausible, namely that currency and deposits are rigidly complementary—for every X dollars the community adds to its deposits, it will acquire also one dollar of currency. It is more likely that the demand for stocks of currency varies in the short run with money income, or more precisely with the volume of retail and wage transactions in which currency is used. If so, an increase in deposits will bring in its wake an increase in demand for currency only to the extent that it increases economic activity. Likewise increases in activity against an unchanged volume of deposits will tend to increase the demand for currency and to pull down the deposit-currency ratio. This hypothesis is borne out by the cyclical behavior of the ratio, so far as this can be divorced from fluctuations of confidence in banks. The ratio tends to fall prior to cyclical peaks, and thanks to the "return flow" of currency to banks to rise prior to troughs.

The deposit-reserve ratio. This ratio reflects both legal reserve requirements and voluntary precautions against deposit withdrawal. I should perhaps repeat the authors' warning that both the numerator and denominator of the ratio exclude interbank deposits. Hence the use of correspondent balances as required or voluntary reserves tends to raise the ratio, and their replacement by high-powered money (as for reserves required of all Federal Reserve member banks) to lower it. In a sense the ratio measures the degree of protection the banking system as a whole has against withdrawals of currency. But this is so only on the assumption, contrary to fact, that in an emergency reserves would not be immobilized by legal requirements. It would have been better if the authors could have provided statistical series distinguishing be-

tween required and excess reserves, but evidently this was not technically possible.

Much of the short-run variation in this ratio, as in the previous one, is connected with the state of public confidence in banks. When confidence weakened, banks sought to protect themselves by increasing their reserves. Here was another element in the inherent instability of the system prior to deposit insurance. In classic banking crises banks and public joined in a mad scramble for high-powered money. There was never enough to go round.

Banks' demand for excess reserves—and banks' willingness to borrow reserves—depend on their assessments of the risks of deposit withdrawals, their appraisals of the possibilities and costs of obtaining reserves in emergency, and the earning opportunities on nonreserve uses of funds. F&S are not inclined to stress the importance of interest rates in the liquidity preferences of banks—or, as we shall see later, in the liquidity preferences of the public.

The issue is clearly posed by the excess reserves of the banks after 1933. These reached $2.5 billion, 42 per cent of total reserves, in 1936. Even after the doubling of reserve requirements in 1936-37, excess reserves were $1.2 billion, 18 per cent of total reserves. And they grew again to $6.3 billion, 48 per cent of total reserves, in 1940. On one common interpretation, this accumulation meant that the banks were "loose," in the sense that gains or losses of reserves, over a considerable range, would affect very little or not at all their holdings of nonreserve assets. F&S, on the other hand, believe that the banks were about as "tight" as ever. True, their demand for excess reserves had greatly increased as a result of the 1930-33 experience; but it was just as important to satisfy this demand as it had been to satisfy the more modest reserve demands of the previous decade. Banks would respond to losses of reserves by reducing their other assets and their deposits, and to gains in reserves by significant increases in loans and investments and deposits.

Another way to put this difference of opinion is as follows: According to the first, or Keynesian, interpretation, banks were by the mid-'thirties moving along a fairly flat liquidity preference curve. Having invested in short-term Treasury and commercial paper until the rates were virtually zero, they would hold in cash any further accretions of reserves. The more high-powered money, the lower the deposit-reserve ratio—and these two proximate determinants certainly do show strong negative correlation in this period. (It is, of course, not at all inconsistent with this interpretation to agree with F&S that the unhappy events of the early 1930's had also moved the banks' liquidity preference curves bodily to the right [Chart 44, p. 537].)

According to the authors' interpretation, this correlation is a coincidence. The decline in the deposit-reserve ratio is due much more to shifts of the banks' cash preference schedule than to movements along it. While shifting, the schedule remained steep. One shift was a result of the 1930-33 experience, the bank runs and the demonstration that the Federal Reserve was no help. A second shift in the same direction was the result of the 1936-37 increases in reserve requirements, reinforced by the 1937 economic contraction. This shift reflected not just the increase in required reserves but, more important and more permanent, an increase in demand for excess reserves to hold against the

possibility that the Fed might again raise requirements. F&S believe that these shifts proceeded at their own pace, largely independent of the growth of the stock of high-powered money. The banks and the money supply were never out of the control of the Fed. By changing the supply of reserves, or reserve requirements, the Fed could at any time alter the deposits and earning assets of the banks by the usual multiple-expansion process.

For these reasons F&S regard gold sterilization, failure to engage in open-market operations, and the raising of reserve requirements as disastrous errors of policy. On the other interpretation, they were mistakes all right, but relatively harmless ones.

I find the interpretation of F&S unconvincing. I do not see why shifts in preference resulting from discrete events should proceed so smoothly, and in particular with such striking negative correlation with the growth of high-powered money. Did bankers never take heart again, even when the deposit-currency ratio was rising and bank runs seemed to be a thing of the past? It may be that the introduction of variation of reserve requirements into the Federal Reserve's tool kit occasioned an increase in the demand for excess reserves. If so, it would be more reasonable to expect this to occur in 1935 when the legislation was passed but the powers were yet to be used, rather than after 1937 when requirements were already at, or very near, the maximum permitted by Congress.

No doubt the depression led to an increase in banks' demand for safe short-term assets, whether excess cash reserves or Treasury bills. As banks' excess cash spilled into short-term securities, short-term rates were driven almost to zero. In these circumstances Federal Reserve open-market purchases of bills were useless—almost like trading cash for cash. Surely there is this much truth, at least, in the Keynesian interpretation. Long-term rates and commercial loan rates were sticky, and it might have been hoped that banks' accumulation of excess reserves and low-yielding short-term securities would eventually put pressure on these other rates. But their differentials above short-term rates stubbornly reflected the lessons of the depression regarding risks of illiquidity and default. In spite of the substantial liquidity of the banks and the public, they did not give way until war altered the whole economic climate. Open-market purchases of long-term securities might have helped to depress their rates and to push banks and other financial institutions into more private lending. As it was, the gold-sterilization policy kept these institutions supplied with safe income from government bonds during much of this period. The Federal Reserve Board does not deserve the scorn with which F&S treat their statement that in the circumstances of 1939 open-market purchases were more important for their direct effects on the capital market than for their influence on member-bank reserves (p. 534).[8]

[8] In an interesting statistical study of the period, George Horwich has compared the responsiveness of bank earning assets to external loan demand, represented by personal income, on the one hand, and to effective reserves, on the other. His results, which are the more convincing because a similar test comes out the other way round for the 'fifties, support the "Keynesian" rather than the F&S interpretation of the 'thirties. "Effective Reserves, Credit, and Causality in the Banking System of the Thirties," in D. Carson, ed., *Banking and Monetary Studies*, Homewood, Ill. 1963, pp. 80-100.

II. *The Velocity of Money*

The trend in income velocity of money. Before World War II, the income velocity of F&S money showed a sharp downward trend. Actually velocity did not begin its decline until after 1880, when it reached a record high of 4.97. It had fallen to 1.91 by 1914. No trend is apparent from 1914 to 1929. Velocity generally declined from 1929 to 1946. There were wartime bulges in 1918-19 and 1942-44. Since 1946 the trend has been upward, but V remains lower than in the 1920's or in 1914.

F&S believe that the normal trend of velocity is down. The reason is that the services of money stocks are a luxury, with income elasticity greater than one. As its per capita income rises, society devotes an increasing fraction of its income to purchase of the services of stocks of money. That is, society increases its holdings of money relative to money income.

Irving Fisher would be surprised to read this theory and history of velocity. He would have expected an account of the demand for money to be closely tied to its function as means of payment. He would have wished to hear about the frequency and timing of wage payments and bill settlements, the speed and cost of communications, the trend of industrial integration, the scope of the barter and subsistence economy relative to the money economy, the volume of total transactions relative to income-generating transactions, and so on.

F&S provide no such discussion, and not even an excuse for omitting it. Partly, I suppose, this is because their money includes time deposits, which are not means of payment. Mainly, I think, it is because they don't regard the properties of money as particularly relevant to an explanation of the demand for money. Here is a consumer good much like any other. Empirically, it turns out—but for reasons that have almost nothing to do with the distinctive properties of money—that this commodity is a luxury, i.e., has an income elasticity greater than one. Like butter, or automobile mileage, or cameras. The big difference is that the supply of money does not respond to changes in income in accordance with its income-elasticity, but instead makes income dance to its tune.

The treatment of money as a luxury consumer durable good seems to me a strained analogy. For one thing, it does not apply very easily to business firms, which hold most of the money stock. In any case, there are no identifiable services yielded by ownership of a stock of money. I don't believe F&S are referring to the joys of numismatics or to the satisfactions of a miser. The services of a stock of money are indirect. They can be measured only by comparing the implications of holding on average large stocks of money and small stocks of other assets with the alternative policy of holding on average smaller stocks of money and larger stocks of other assets. Possible advantages of the first policy over the second are smaller costs in effort or in fees and smaller risks of loss when it is necessary or expedient to make payments. The disadvantages of the first policy are the sacrifice of earnings when alternative assets yield more than monetary assets. Individuals and business firms presumably adjust their money holdings until these advantages and disadvantages balance at the margin.

It is certainly not obvious why this process should lead to a secular decline in velocity. If this is the model F&S have in mind, then they should tell us how time and the growth of incomes have altered the relevant costs, risks, and yields. But then they would have to assign much greater importance to interest-rate differentials as a determinant of velocity than they are prepared to do. They prefer to leave the alleged downward trend in velocity unexplained—for saying that money is a luxury is just another way of saying that its velocity declines with income.

This inadequacy of theory would not be so damaging if the empirical evidence for the downward trend were more convincing. On F&S's interpretation of the series, there is a downward trend from 1869 to 1946. This leaves the period since 1946 as the principal aberration to be explained; but it also leaves the 1920's as a disconcertingly long period during which the trend was interrupted. For the most recent period, the authors' hypothesis is that the demand for money has been reduced by the favorable postwar experience of economic stability.[4] Another *ad hoc* shift in preferences! Since adaptation to this experience will sooner or later be complete, the authors expect a resumption of the normal downward trend.

To me it seems strange to rely on a trend which regards the 1930's and 1940's as normal and the 1920's and 1950's as abnormal. The only convincing trend evident in the velocity series is 1880-1915. Since 1915 the series is dominated by fluctuations associated with wars and depression. Latané has shown that the velocity of money excluding time deposits since 1909 can be explained by interest rates, independent of the trend of real income.[5] F&S state that the two velocity series differ since 1915 only in periods when identifiable special circumstances affected the demand for time deposits. So it is not the difference in the denominators of the two velocity ratios which is at issue. F&S object that interest rates cannot explain low velocity in 1932-33 and in 1946. But given the wealth of particular explanations their own book provides for abnormally high demands for money in those years, this criticism of Latané's results does not seem very damaging. F&S are left, therefore, with the assertion that Latané and other "Keynesians" cannot explain, via interest rates, the downward trend in velocity before 1915.

Actually, the long-term interest rate "explains" 60 per cent of the variation of velocity from 1869-1914 (Table 1, regression 1.7). It is true that simple trend explains much more (92 per cent, regression 1.5). After 1914, trend explains only 51 per cent; short-term interest rate 56 per cent; long-term interest rate 45 per cent (regressions 1.8, 1.9, 1.10). The introduction of either interest rate into a multiple regression (regressions 1.12 and 1.13) lowers the absolute size of the trend coefficient.

The interest-rate explanation, although it succeeds well enough so that the authors have no right to dismiss it, is at a disadvantage in respect to the velocity of F&S money. For F&S money includes time deposits, which bear an

[4] F&S recognize that this interpretation must be "highly tentative" and "await further evidence" (p. 675).

[5] H. A. Latané, "Income Velocity and Interest Rates—A Pragmatic Approach," *Rev. Econ. Stat.*, Nov. 1960, pp. 445-49.

interest rate that can be expected to be correlated with market rates. The proper variable would be the differential of the market rate from the rate on time deposits. It is not surprising that interest rates are more closely related to the velocity of money exclusive of time deposits (regressions 2.9, 2.10, 2.12, 2.13).

You have your choice. The F&S income-luxury theory seems to work up to World War II but has to rely on considerable *ad hoc* explanation since. The Keynes-Latané interest rate theory of velocity seems to work since 1909 or so, but needs help for the preceding period.

This help is not hard to find. The downward trend in velocity coincided with a strong upward trend in the public's holdings of deposits relative to currency, the deposit-currency ratio already discussed. Neither trend started until about 1880. The correlation between these two variables before 1915 is .90.[6] The trend toward deposits, after interruption during World War I, seems to have continued until 1930, thanks to growth of time deposits. During the same period 1880-1915, commercial bank deposits grew relative to mutual savings banks. Mutual savings banks were almost as important as commercial banks around 1880, when the decline in velocity began. Their deposits were 80 per cent as large as those in commercial banks in 1877, 60 per cent as large in 1880, only 25 per cent as large as their rivals in 1915. During these years, of course, the territory covered by mutual savings banks became a smaller part of the continental economy.

I suspect, therefore, that over this period there was a considerable thrift element in the accumulation of commercial bank deposits. The character of F&S money changed radically as currency became less important. Some deposits replaced currency as stocks of means of payment. Other deposits replaced mutual savings deposits; commercial banks became the principal available savings institutions.

Perhaps 1880-1915 was the great day for commercial banking, and the decline in velocity reflects its successful spread. Similarly savings and loan associations have been the spectacular success of the period since World War II. Their spread has helped to increase the velocity of money, just as the spread of commercial banks increased the velocity of currency. To the extent, however, that the spread of SLA's has also taken business from the security markets, the velocity of money-plus-SLA shares has declined, just as the velocity of currency-plus-bank deposits did before 1915.

The regressions of Table 1 include both a cyclical variable $\Delta Y/Y$ and interest rates. They indicate that the association of velocity with interest rates is not due simply to the fact that both are pro-cyclical. Inclusion of interest rates invariably reduces or eliminates the apparent significance of the cyclical variable. In the case of V_2, the velocity of money without time deposits, the

[6] In this correlation for 1869-1914, the velocity series is from F & S (Table A-5, p. 774). The series for D/C is given in Table B-3, pp. 799-801. For years after 1907, where monthly observations are given, the observation for the year is the average of the 12 months. For previous years, for which observations are available for only one or two months a year, estimates were made for missing months by linear interpolation, and yearly estimates by averaging the resulting monthly series.

TABLE 1—REGRESSIONS OF VELOCITY

Dependent Variables: V_1 velocity of F&S money, which includes time deposits, V_2 velocity of F&S money less time deposits

Regression Number	Period	Dependent Variable	Constant Term	Coefficients (and their ratio to their standard errors) of				Proportion of Variance of V Explained	Standard Error of Residual
				Year-1914	Rate of Change of Money Income	Short-term Interest Rate	Long-term Interest Rate		
			1	t	$\Delta Y/Y$	R_1	R_2	R^2	
1.1	1869–1959	V_1	2.43	−.035 (−20.5)				.82	.42
1.2	1869–1959	V_1	2.38	−.034 (−20.2)	−.220 (−0.5)			.82	.42
1.3	1869–1959	V_1	2.44	−.034 (−14.6)	−.221 (−0.5)	−.000 (−0.0)		.82	.43
1.4	1869–1959	V_1	1.74	−.030 (−14.0)	−.005 (−0.0)		+.175 (3.0)	.84	.41
1.5	1869–1914	V_1	1.70	−.064 (−22.3)				.92	.26
1.6	1869–1914	V_1	2.18			+.192 (2.1)		.10	.87
1.7	1869–1914	V_1	0.13				+.707 (8.0)	.60	.58
1.8	1915–1959	V_1	2.06	−.016 (−6.7)				.51	.21
1.9	1915–1959	V_1	1.35			+.121 (+7.4)		.56	.20
1.10	1915–1959	V_1	0.83				+.235 (+5.9)	.45	.22
1.11	1915–1959	V_1	2.06	−.016 (−6.6)	−.009 (−0.0)			.51	.21

TABLE 1—(*Continued*)

Regression Number	Period	Dependent Variable	Constant Term	Coefficients (and their ratio to their standard errors) of: Year-1914	Rate of Change of Money Income	Short-Term Interest Rate	Long-Term Interest Rate	Proportion of Variance of V Explained	Standard Error of Residual
			1	t	$\Delta Y/Y$	R_1	R_2	R^2	
1.12	1915–1959	V_1	1.59	−.009 (−3.7)	+.438 (+2.0)	+.096 (+5.5)		.72	.16
1.13	1915–1959	V_1	1.43	−.010 (−2.7)	+.224 (0.8)		+.130 (+2.2)	.56	.20
2.8	1915–1959	V_2	3.26	−.034 (−11.3)				.64	.33
2.9	1915–1959	V_2	1.81			+.241 (+8.9)		.65	.33
2.10	1915–1959	V_2	0.49				+.551 (+10.1)	.70	.31
2.11	1915–1959	V_2	3.32	−.034 (−9.9)	−.932 (−2.5)			.70	.30
2.12	1915–1959	V_2	2.59	−.022 (−6.5)	−.207 (−0.6)	+.150 (6.0)		.83	.24
2.13	1915–1959	V_2	1.70	−.018 (−3.8)	−.293 (−0.8)		+.335 (+4.3)	.78	.27

Sources: V_1 and V_2, F&S Table A-5, p. 774. $\Delta Y/Y$ derived from F&S money income series, described on page 775. R_1 (commercial paper rate) and R_2 (basic yield on long-term corporate bonds) are series charted and described in F&S, pp. 640–641. I am grateful to the authors for providing me with the series Y, R_1, and R_2.

cyclical variable $\Delta Y/Y$ has a negative coefficient. This suggests that the pro-cyclical behavior of V_1, the velocity of F&S money, is wholly due to time deposits. Most of it is in any case explained by one or the other interest rate. More generally, in boom times deposits lose out to other thrift accounts, to securities, including equities, and to real investments, while in recessions these alternatives become relatively less attractive. When a phenomenon is so simply explained, is it necessary to construct an elaborate theory, in which estimates of unobserved variables like permanent income and permanent prices are invested with an altogether spurious reality?

Cyclical fluctuations in velocity. Whatever its secular trend, velocity increases in cyclical expansions and declines in contractions. Several interpretations are consistent with this observation. One is that the cycle is nonmonetary: Money income is driven along its cyclical course by exogenous factors, the money supply is sluggish, and the cyclical behavior of velocity is the arithmetic result. F&S present convincing narrative evidence that at least on some crucial occasions this has not been true.

A second interpretation (not necessarily at odds with the first) is that velocity follows the pro-cyclical movement of interest rates. This has the scientific virtue of providing a unified theoretical and statistical (see, for example, the findings of Latané previously discussed) explanation of both trend and cycle in velocity.

A third explanation is Friedman's. The demand for real balances in the form of money grows with *permanent* real income—indeed, as we have already seen, more than proportionately. Measured incomes run ahead of permanent incomes in cyclical upswings, making the velocity statistic high, and behind in downswings, making it low. Some day perhaps Friedman will tell us what transient income is used for. We know from his work on the consumption function that none of it is consumed. We now know that none of it is saved in monetary form. Does it all go into the stock market? A priori I should have thought that money balances were a likely repository of windfalls. After all, didn't F&S define money as a "temporary abode of purchasing power . . ."?

The trend-cycle explanation of velocity leaves many episodes unexplained, and F&S provide a fascinating account of the special forces operating on the demand for money from time to time. A recurrent favorite is price expectations. When the public expects prices to fall, their demand for money will be increased.

This is F&S's explanation of one of the most puzzling phenomena in their narrative, the extraordinarily high demand for money, and for other liquid assets, between 1946 and the Korean conflict. (The low velocity of money in those years is the more surprising to F&S because they believe that their figures *over*state the true velocity. They overstate it for a reason I have already commented upon, namely that in their view "money" ought to include marketable government obligations during the period the Fed was committed to support their prices.) It is easy to forget that before Korea the economy had apparently reached a noninflationary equilibrium with a high degree of liquidity and interest rates on government securities not exceeding 2½ per cent.

I am inclined to agree with F&S's interpretation of the 1946-50 period, the more so because there is survey evidence, thanks to George Katona, that the public actually held the expectations attributed to it. There are other instances as well in which the imputation of price expectations up or down seems a reasonable way to make sense of otherwise puzzling movements of velocity. But F&S bring in these expectations only when they are obviously needed, leaving the reader with the uneasy feeling that their introduction at other times might be embarrassing. More important, I find it hard to reconcile their attention to price expectations with their neglect of explicit interest rates.

The real rate of return on money consists of two parts: the gain or loss in purchasing power due to change in prices, and the nominal or own-rate of return, zero for currency and interest net of service charges for deposits. A similar real rate of return can be computed for other assets. From a theoretical standpoint it is hard to understand why F&S are attributing such strategic importance to the first component of real rate of return and so little to the second. Inflationary or deflationary expectations affect the real rate of return on money, but no differently than they affect the real rate of return on a host of other assets fixed in ultimate money value. (Incidentally F&S refer to inflation—presumably unanticipated inflation—as a tax on money. In fact it is a tax on all fixed-money-value assets, and a subsidy on all fixed-money-value debts. The excess of the first over the second is not identical with the stock of money except by accident.) However, these other fixed-money-value assets differ from money in the flexibility of their nominal yields. These can and do rise or fall to compensate at least partially for generally held inflationary or deflationary expectations. Nominal yields on monetary assets, even time deposits, respond more slowly, if at all, because of institutional and legal ceilings.

Consequently, inflationary expectations will affect the demand for money differently, depending on the extent to which nominal yields on other fixed-money-value assets rise to compensate for these expectations.

In 1946-50, for example, interest rates on government bonds did not decline as a result of deflationary expectations. The Fed simply had to make fewer purchases in order to keep rates at the support levels. The public's demand for bonds was greater, and its demand for money less, than if interest rates had been free to adjust downward to expectations of postwar deflation.

The stability and independence of velocity. The main conclusion of F&S is that although the stock of money has been determined by a variety of forces in the nine decades they review, its relationship to other economic variables has been stable. Has this been so? The annual percentage change in the money supply explains only 31 per cent of the variation in the annual percentage change in money income.[7]

Has velocity been stable and predictable? F&S point out (p. 682) that the

[7] Regression of $\Delta Y/Y$ on $\Delta M/M$, 1869-1959, where Y is F&S money income series (derivation described p. 775), and M is F&S money (series given in Table A-1, pp. 704-20). Annual observations of M are averages of monthly figures. Monthly figures are not provided for all months prior to May 1907. They were estimated by linear interpolation between the months for which observations are given.

year-to-year change in velocity was less than 10 per cent in 78 of the 91 years and that velocity fell within 90-110 per cent of trend in 53 of 92 years. Since a 10 per cent difference in money income is the difference between inflation and recession, these figures are not very reassuring. I have regressed F&S velocity against the factors they use to explain it: time trend, and percentage increase in money income, the latter representing a cyclical variable. The residual standard deviation of velocity is .42, one-sixth of its mean value (regression 1.2, Table 1).

But no one, even the most skeptical of the importance of monetary factors, is surprised to find a fairly close statistical relationship between the course of economic activity and the money stock. The direction of influence in this correlation is something else again. For example, F&S cite the contractions of money stock that have accompanied major business contractions. We know, however, that some events—e.g., loss of foreign demand for U.S. products—could contribute both to business contraction and to monetary contraction. Furthermore, business contractions themselves set in motion forces which reduce the money stock—banks, business borrowers, and depositors all become more cautious. The same may be true historically of the monetary authorities, reacting, however mistakenly, in response to "needs of trade" or defending the dollar against external drain. An inspection of F&S's own figures will show that it is adverse changes in the two ratios, deposit-reserves and deposit-currency, more than changes in high-powered money, which are proximately responsible for contracting the money stock. So the sins of the monetary authorities are generally ones of omission rather than commission.

Of course, F&S are aware of the two-way nature of the relationship between money and economic activity. They say:

> Apparently, the forces determining the long-run rate of growth of real income are largely independent of the long-run rate of growth of the stock of money, so long as both proceed smoothly. But marked instability of money is accompanied by instability of economic growth (p. 678).
>
> Changes in the money stock are therefore a consequence as well as an independent source of change in money income and prices, though, once they occur, they produce still further effects on income and prices. Mutual interaction, but with money rather clearly the senior partner in longer-run movements and in major cyclical movements, and more nearly an equal partner with money income and prices in shorter-run and milder movements—this is the generalization supported by our evidence (p. 695).[8]

[8] Elsewhere the authors have summarized their position as follows: "For major movements in [money] income, we concluded that there is an extremely strong case for the proposition that sizable changes in the rate of change in the money stock are a necessary and sufficient condition for sizable changes in the rate of change in money income. For minor movements, we concluded that, while the evidence was far less strong, it is plausible to suppose that changes in the stock of money played an important independent role, though certainly the evidence for these minor movements does not rule out other interpretations." Friedman and Schwartz, "Money and Business Cycles," *Rev. Econ. Stat.*, Feb. 1963, Supplement, p. 63.

F&S's monetary interpretation of history requires not simply that monetary contractions and major business contractions are statistically associated, but two further propositions: Preventing monetary contraction would have prevented these business contractions, and nothing else would have done so. The authors require also, of course, the corresponding propositions regarding marked expansions of money income.

I believe that this amounts to saying that velocity is independent of autonomous, policy-engineered alterations in stock of money. For if such an increase in the stock of money would, by lowering interest rates or otherwise, result in a systematic reduction of velocity, then the linkage of money and business activity is much weaker than F&S think. And the historically observed correlation is much less comfort to the monetary authorities. Results like those of Latané, already cited, relating velocity to interest rates are therefore of the greatest importance.

I do not wish to be misunderstood. F&S cite some convincing examples of monetary changes that were clearly independent of contemporary or immediately preceding economic events: the increased gold production in 1897-1914 and the sharp increases in the Federal Reserve discount rate in 1920 and again in 1931. I am willing to agree that these monetary events contributed in important degree to the economic events which followed.

Consider the following three propositions: Money does not matter. It does too matter. Money is all that matters. It is all too easy to slip from the second proposition to the third, to use reasoning and evidence which support the second to claim the third. In this book F&S have ably and convincingly marshaled evidence for the proposition that money matters. They have put to rout the neo-Keynesian, if he exists, who regards monetary events as mere epiphenomena, postscripts added as afterthoughts to the nonmonetary factors that completely determine income, employment, and even prices. But in their zeal and exuberance Friedman and his followers often seem to go—though perhaps less in this book than elsewhere—beyond their own logic and statistics to the other extreme, where the stock of money becomes the necessary and sufficient determinant of money income. Much as I admire their work, I cannot follow them there.

Remember that the difference between the propositions "Money matters" and "Money is all that matters" is also the difference between the propositions "Fiscal policy matters" and "Fiscal policy doesn't matter." If there is a tight linkage, at any moment of time, between money income and the stock of money, then pure fiscal policy—e.g., bond-financed government spending—cannot raise money income. But if the income-velocity of money is flexible, in response to interest rates, the stock of money itself, or other variables, then expansionary fiscal policy can raise both velocity and money income.[9] When F&S minimize or blur the distinction between the weak and strong propositions concerning the importance of money, they do not warn the reader how crucial are the issues of policy involved.

[9] This is a well-understood point of macroeconomic theory, explained among other places in my article "Liquidity Preference and Monetary Policy," *Rev. Econ. Stat.*, May 1947, pp. 124-31.

III. *Judgments on Monetary Policy*

Enough of the parochial disputes of monetary theorists. The notorious disagreements among economists are a source of great comfort to practical men in business, finance, and government. When the experts differ the policymaker can in better conscience do what he would have done anyway. By reputation monetary economists are especially prone to mutually cancelling differences of opinion. Many controversies on monetary theory and policy pit Friedman and his followers against the rest of the profession. But consensus among Friedman's opponents generally extends no further than the proposition that Friedman is wrong. In the course of their narrative F&S frequently pause to point out error and to allege harm resulting from Keynesian ways of thinking.

But as F&S led me through the major decisions of monetary politics and monetary policy of the past century, I did not find their Monday morning quarterbacking very controversial. I think that economists today—though they differ sharply in theoretical approach and political color—would agree very widely on the major practical and operational issues of these nine decades.

I prefer to end with this note of operational agreement rather than with theoretical discord. Laymen should not be too disheartened, or heartened, by strife among academic monetary theorists. Economists are likely to show a united front when the occasion arises to second guess the decisions of men of affairs. Friedman and Schwartz—believing so strongly in the powers for good or evil of monetary policy—are even more critical of the use of these powers than other economists.

I will give some examples, working more or less backward in time.

1. As already noted, F&S condemn the Federal Reserve and the Treasury for restrictive policy from 1933 to 1941. Many economists would assign to monetary factors much less weight than F&S do in explaining the course of economic activity in those years, in particular the recession of 1937. But few would, I think, dispute the authors' retrospective practical conclusions. The monetary authorities should have tried harder to promote expansion in 1933-36 and 1937-40—nothing would have been lost and something might have been gained. The 1936-37 increases in reserve requirements, whether or not they caused the recession, were too drastic. Throughout the period the authorities were too little concerned with deflationary risks immediately at hand and too much concerned to forestall the hypothetical future dangers of excess liquidity. Incidentally I note with some dismay that at least some members of the central banking fraternity wished to use monetary policy to keep a rein on fiscal policy. One of the reasons given for immobilizing reserves in a Federal Reserve Bank of New York memorandum (quoted by F&S, p. 523) was to remove the temptation easy money gives national, state, and local governments to "over-borrow." The same memorandum offers as another reason for restrictive policy the danger that "large excess reserves may, by causing foreign expectation of favorable conditions for speculative investment, accen-

tuate the gold *in*flow" (my italics). It appears that no matter which gold problem we may have, tighter money is the remedy.

2. Economists will differ also in the weights they assign monetary factors in the origins, severity, and length of the Great Contraction of 1929-33. But from today's vantage point no one will defend the passive acquiescence of the Fed in the monetary contraction and banking collapse. The Fed's failure to undertake an aggressive policy of open-market purchases seems incredible. Even when open-market purchases were finally carried out in the spring and summer of 1932, it was less for economic reasons than to forestall unsound inflationary actions by the Congress. The System's indifference to domestic crisis stands in contrast to its classic reflex to the U.K. devaluation in 1931; the Fed raised the discount rate two points. Even so French balances didn't stay in New York very much longer.

The traditional excuse for Federal Reserve passivity in this period has been a technical one, the alleged shortage of "free gold." The Federal Reserve Banks were required to hold gold reserves of 35 per cent against their deposit liabilities and 40 per cent against their note liabilities. The other 60 per cent of Federal Reserve Notes had to be backed either by "eligible paper" or by gold. When banks stopped discounting during the contraction, the Federal Reserve Banks ran short of eligible paper; correspondingly Federal Reserve Notes had to be covered by gold more than 40 per cent. The problem was finally resolved by the Glass-Steagall Act of February 1932, which permitted government securities to serve in place of eligible paper as collateral for notes.

F&S find no evidence that free gold was in fact an important constraint on Federal Reserve action or a central consideration in discussions within the System at the time. If it had been an effective constraint, they point out, there were several avenues of escape that could have been tried, including earlier request for corrective legislation. In fact the Administration rather than the Fed asked for the legislation, and its enactment was not followed by a change in Federal Reserve policy until six weeks later.[10]

3. Perhaps there would be less agreement among economists with F&S's verdict that monetary policy was too tight in 1928-29. A speculative stock market boom during a period of stable noninflationary economic growth presented the System with a difficult and cruel dilemma. A policy tight enough to curb the stock market would inevitably make credit costly or unavailable to ordinary business and agricultural borrowers. In a sense, of course, inflated stock prices themselves spelled easy money and encouraged real investment,

[10] In their historical research on Federal Reserve policy-making in this and other eras, the authors were limited to the materials available in the private papers of Federal Reserve officials, principally those of George L. Harrison (an official of the New York Bank 1920-40 and its governor or president 1928-40), Charles L. Hamlin (a member of the Board 1914-36), and E. A. Goldenweiser (director of research and statistics for the Board 1926-45). Now, thanks to the constructive action of the present Board of Governors in making available past minutes of the Board and the Open Market Committee, scholars will be able to provide a more definitive history of Federal Reserve policy-making. It is entirely possible that future research will alter the interpretations to which the available evidence led Friedman and Schwartz.

but only for businesses legally and institutionally prepared to issue new equities. There were not enough of these, in the short run at least, to offset the restrictive effects of tight bank credit on businesses dependent on debt finance. Personally I agree with the judgment of F&S that in 1928-29 the System should have ignored the stock market in arriving at its general credit policy and concentrated instead on easing money sufficiently to promote the continued expansion of the economy. But given this judgment, F&S are unduly doctrinaire in rejecting out of hand the use of moral suasion or selective controls against stock market credit.

4. Economists of all schools would, I think, agree with F&S that the Fed was too slow to raise the discount rate after World War I and then contributed to a drastic deflation in 1920 by raising the rate from 4 to 7 per cent in six months.

5. Preservation or restoration of the gold value of the dollar has on several occasions been accorded undeserved primacy as a goal of monetary policy, at severe cost to the domestic economy. Concerning the 1879 resumption of gold-dollar convertibility at the pre-Civil War rate, F&S's judgment in retrospect is that, given that a gold standard was to be reestablished, it would have been preferable to have resumed at a parity that gave a dollar-pound exchange rate somewhere between the pre-Civil War rate and the rate at the end of the war. However, they point out that the progress of real output does not appear to have been set back by the severe and painful price deflations which preceded and followed resumption.

Similarly, F&S have some sympathy for the silver agitation prior to 1897. But they are not, of course, for bimetallism; and they recognize also that silver agitation, so long as it led to uncertainty about the country's commitment to gold, made the gold standard even more deflationary than it would have been otherwise. The dilemma between gold standard and internal stability recurred in 1932-33, and F&S clearly would have put internal stability first. They point out that the Fed and Treasury played the gold standard game asymmetrically; gold inflows were sterilized before 1929 and after 1933, but gold outflows were not sterilized in the intervening years. To a certain extent they think this has also happened during the most recent decade.

6. I have left until last F&S's views on recent monetary policies and controversies. Federal Reserve policy since World War II gets fairly good marks from F&S. They condemn the continuation of the bond price support policy after the war, but find that it did little harm until the Korean inflation. They also condemn the 4¼ per cent interest rate ceiling on government bonds. They cannot get excited about the "bills only" controversy, partly because they agree with the policy's advocates in stressing "monetary" rather than "credit" effects, partly because such policy need not prevent the Treasury-cum-Federal Reserve from achieving any desired maturity distribution of debt held outside the government. They applaud the Fed's discovery of the quantity of money and of the principle that it should keep pace with the real growth of the economy. The period as a whole has been one of unusual stability in the rate of change of the stock of money, but the authors detect a growing variability and attribute it in part to increased self-assurance on the

part of the monetary managers. So far as month-to-month anticyclical policy is concerned, the authors find little to criticize. They feel the Fed reacted too late and too strongly at times, but they praise the shifts to ease that occurred before the cyclical peaks of 1953 and 1960.

Conclsions: I have not done justice to the scope of this book. History presents the theoretically minded scholar with one challenge after another. Here these are met with the brilliance and finesse one would expect. Examples are: the determination of the exchange rate and gold premium during the greenback era, the economics of the 1879 resumption; the silver question; balance-of-payments pressure and adjustments in the 1890's; FDR's gold purchase policy; the mechanics of Federal Reserve bond support policy during and after World War II. The reader is advised in no event to omit the footnotes, which contain many gems of monetary theory: on Gresham's law; purchasing power parity; the prohibition and regulation of interest on commercial bank deposits; the significance of the "free reserve" position of member banks; the monetary mechanics of shifts among currency, demand deposits, time deposits, and other thrift accounts.

This is one of those rare books that leave their mark on all future research on the subject.

COMMUNICATIONS

A Catenary Turnpike Theorem Involving Consumption and the Golden Rule

Catenaries One Meets

A rope will hang in the shape of a catenary, $y(x) = a_1e^{\lambda x} + a_2e^{-\lambda x}$, because even a dumb rope knows that such a shape will minimize its center of gravity. The young man on the flying trapeze, when he approaches the vertical in his swing, goes through catenary motions. The essence of the Turnpike Theorem in modern economics is the catenary property of efficient paths in closed von Neumann or neoclassical models.[1]

Although the catenary is met less frequently in physics and economics than its antipodal concept, sinusoidal vibrations, I know of two other places in economic theory where the catenary is involved. One is in the pure theory of intertemporal speculative price equilibrium.[2] The other is in connection with the famous Ramsey problem of optimal saving. This aspect of the Ramsey problem, which seems not to have been commented on in the literature, was actually one clue back in 1949 to formulating the Turnpike Theorem, even though that theorem explicitly excludes the enjoyment of consumption as the goal for maximizing (by virtue of the Neumann assumption that *all* output is reinvested in the closed system).

Here I shall briefly sketch a new kind of Turnpike Theorem in which the utility of consumption represents the *desideratum* for the problem.

[1] This theorem was first conjectured in the RAND memo, *Market Mechanisms and Maximization*, Pt. III (1949). See R. Dorfman, P. A. Samuelson, R. M. Solow, *Linear Programming and Economic Analysis*, New York 1958, Ch. 12; P. A. Samuelson, "Efficient Paths of Capital Accumulation in terms of the Calculus of Variations," in K. J. Arrow, S. Karlin, and P. Suppes, eds., *Mathematical Methods in the Social Sciences, 1959*, Proceedings of the First Stanford Symposium, Stanford 1960, pp. 77–88; R. Radner, "Paths of Economic Growth That are Optimal with Regard Only to Final States: A Turnpike Theorem," *Rev. Econ. Stud.*, Feb. 1961, *28*, 98–104; M. Morishima, "Proof of a Turnpike Theorem: The 'No Joint Production Case'," *Rev. Econ. Stud.*, Feb. 1961, *28*, 89–97, and *Equilibrium, Stability and Growth*, Oxford 1964, Ch. 6; T. C. Koopmans, "Economic Growth at a Maximal Rate," *Quart. Jour. Econ.*, Aug. 1964, *78*, 355–94; L. W. McKenzie, "Turnpike Theorems for a Generalized Leontief Model," *Econometrica*, Jan.–April 1963, *31*, 165–80, and "The Dorfman-Samuelson-Solow Turnpike Theorem," *Internat. Econ. Rev.*, Jan.1963, *4*, 29–43.

[2] For a complete treatment of the simplest ideal model, see P. A. Samuelson, "Intertemporal Price Equilibrium: A Prologue to the Theory of Speculation," *Weltwirtschaftliches* Archiv, Band 79 (1957), pp. 181–221, particularly p. 217 of the Mathematical Appendix where equations (4) and (5) possess the catenary property in the neighborhood of equilibrium. Actually, catenaries have a great future in economics. For a theorem of Poincaré guarantees that the characteristic roots near an equilibrium motion of a Hamiltonian maximum problem come in pairs with opposite signs: half the time, so to speak, a time-reversible system will have pure imaginary roots and give rise to sinusoidal vibrations and half the time will have the real roots of a catenary.

How the World Ends

Frank Ramsey, living in a happier age and being a Cambridge philosopher, assumed society would last forever and seek to maximize the utility of its consumption over all infinite time.[3] With a fixed population, his problem was the following:

$$\text{(1)} \qquad \underset{\{k(t)\}}{\text{Max}} \int_0^\infty u[c(t)]dt = \int_0^\infty u[f(k(t)) - \dot{k}(t)]dt$$

$$\text{subject to } k(0) = k^0$$

where

$u(c)$ = utility of consumption, a concave function with $u' \geq 0$, $u'' \leq 0$,
$f(k)$ = annual output, producible by capital and (fixed) labor,
$\dot{k}$ = net capital formation (after depreciation), equal to $f(k) - c$.

In order that the infinite integral be convergent, Ramsey must stipulate some "Bliss" saturation assumptions. For the present catenary theory, I shall concentrate on the Schumpeter case where capital gets saturated at a finite level, producing a zero own-rate-of interest. Mathematically, I assume

$$f(k) \text{ a concave, nonnegative function, } f''(k) < 0$$

$$\text{(2)} \qquad f(k^*) \text{ a unique maximum for } 0 < k^* < \infty, \text{ where } f'(k^*) = 0$$

$$u(c) \text{ an increasing concave function for } 0 \leq c \leq c^* = f(k^*).$$

Since $u(c) \leq u(c^*)$, I can set $u(c^*) = 0$ and ensure $u(c) < 0$, $c < c^*$, thereby avoiding the need to speak of minimizing the divergence from Bliss. An alternative Bliss assumption would involve saturation of $u(c)$ or asymptotic saturation of $f(k)$. But, by a simple device,[4] the Ramsey analysis can be put on a per capita basis to handle the Golden-Rule problem; and positive exponential population growth leads to Bliss-convergence conditions like those of (2).

Figure 1 plots the concave production function $f(k)$, showing for fixed labor its Bliss maximum b at k^*. (The upper curves should be ignored at this reading.) Figure 2 plots the concave utility function $u(c)$. Note that its origin constant has been adjusted so that, at its Bliss level $c^* = f(k^*)$, $u(c^*) = 0$, with $u(c) < 0$ for all other feasible consumption levels. By this device, we ensure that the maximum of our integral is finite, being equal or less than zero. (At this stage, ignore the per capita symbols C/L.)

The solution to Ramsey's problem is well known. If society's initial k^0 is at the Bliss level k^*, it will consume all its income and stay there indefinitely. If it begins with $k^0 < k^*$, it will engage in positive capital formation

[3] F. Ramsey, "A Mathematical Theory of Saving," *Econ. Jour.*, Dec. 1928, *38*, 543–59.

[4] The year 1962–63 was a golden year for Golden-Rules at M.I.T. In the seminar of Robert Solow and Edmund Phelps (visiting from Yale), Christian von Weizsäcker (of Basel, Hamburg, and Berlin), Christopher Bliss (of Cambridge), and others proved all kinds of theorems. Professor Phelps reminds me that Weizsäcker and T. Koopmans had independently developed this device, as did S. Chakravarty (of Delhi) during his 1963–64 stay at M.I.T.

$\dot{k}$ at a rate which is ever an increasing function of the k^*-k discrepancy of each moment. Actually, the optimal decision rule is given by $\dot{k}=D(k^*-k)$, where $D(x)$ has the sign of x. This rule comes from the familiar energy-integral condition of the calculus of variations

$$H(k, \dot{k}) = \dot{k}\,\frac{\partial u[f(k) - \dot{k}]}{\partial \dot{k}} - u[f(k) - \dot{k}] \equiv \text{constant.} \tag{3}$$

The constant is seen to be zero if we put $\dot{k}=0$ and $k=k^*$ into this relation. This implicit relation can be solved to give $\dot{k}$ in terms of k because of the curvature conditions on u and f that ensure a maximum (namely, the good old laws of diminishing returns and of diminishing marginal utility).

Note this. If we call the Bliss level a "turnpike," a Ramsey *optimal saving program does spend most of its time in a close neighborhood of the turnpike.*

Now let us assume the world (or the program or the Monarchy) may come to an end at finite time T. As before we begin with initial capital stock k^0. But now we either assume that there must be a prescribed terminal capital stock k^T, or (if it really is the end of the world and we couldn't care

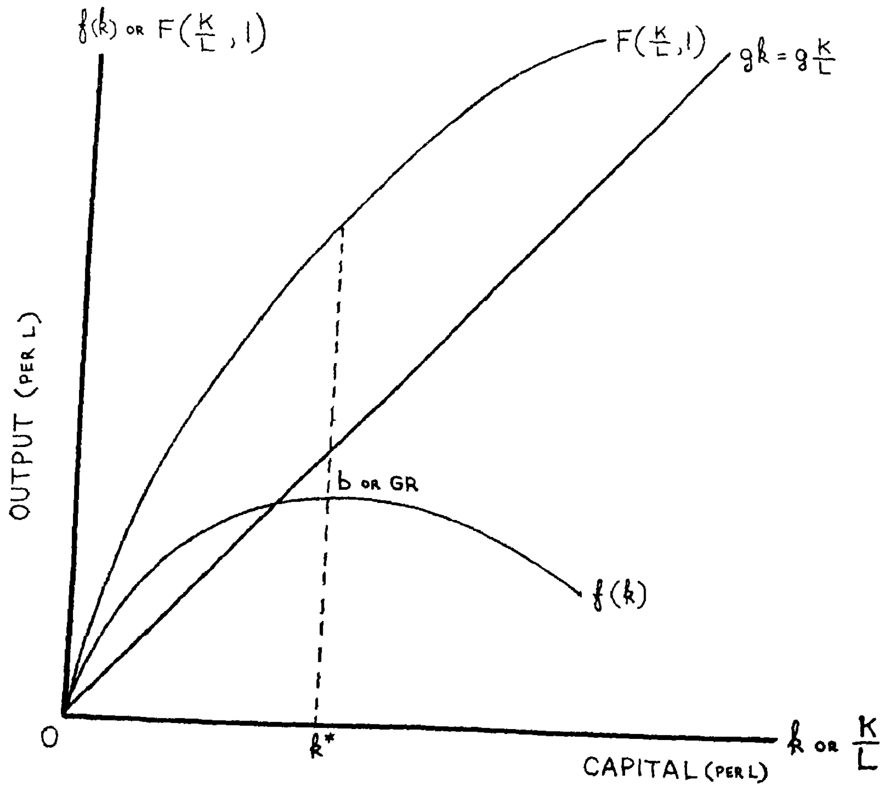


FIGURE 1

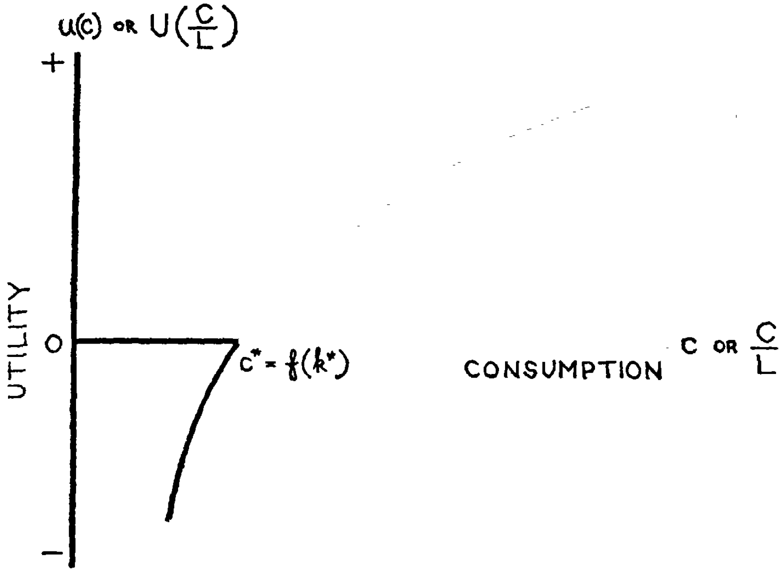


FIGURE 2

less what the *après-déluge* posterity inherits) that k^T can be allowed to be zero.

Now we have the two-point problem:

$$\text{(4)} \qquad \underset{\{k(t)\}}{\text{Max}} \int_0^T u[f(k) - \dot{k}]dt \quad \text{for} \quad k(0) \leqq k^0, \ k(T) \geqq k^T \geqq 0.$$

The standard first-variation necessary condition for a maximum is that of Euler,

$$\frac{d}{dt}\frac{\partial u}{\partial \dot{k}} - \frac{\partial u}{\partial k} \equiv 0 \quad \text{or}$$

$$\text{(5)} \qquad -\frac{d}{dt}u'[f(k) - \dot{k}] - u'[f(k) - \dot{k}]f'(k) \equiv 0$$

$$u''[f(k) - \dot{k}][\ddot{k} - f'(k)\dot{k}] - u'[f(k) - \dot{k}]f'(k) = 0.$$

Since u does not involve t explicitly, it is well known that the energy relations (3) can be directly deduced from (5).

This second-order differential equation is sufficient as well as necessary for a maximum, since u is a strictly concave function in terms of the variables $(\dot{k}, k)$. This remark rules out conjugate points and assures us of a unique solution satisfying the end condition $[k^0, k^T]$ for all T.

Figure 3 shows some optimal programs. The horizontal line bb represents the Bliss level, which is itself an optimal path if we start and end on it. Starting from limited capital at A, the path AR depicts the optimal Ram-

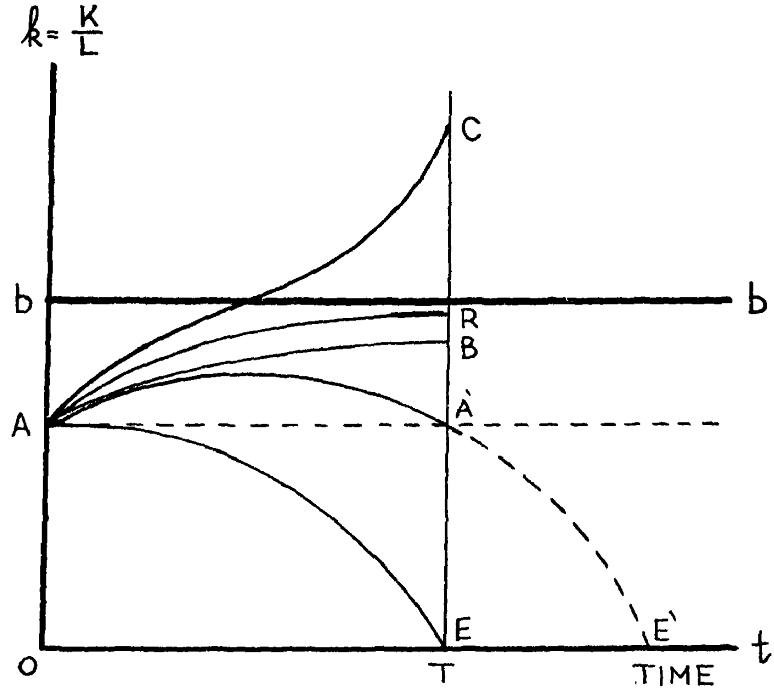


FIGURE 3

sey program, which will approach closer and closer to the *bb* turnpike as time increases. The path *AB* is optimal if we are to end up at *B*: society consumes more and saves less than in the Ramsey path, because it bequeathes less. The path *AC* proceeds on the opposite assumption, namely that the terminal k^T exceeds that of the Bliss k^*. It is no accident that *AC* has a inflection point where it crosses the turnpike, since all extremals are convex to the turnpike. The path *AA'* actually ends up at the same k^T level as k^0, where it begins. Not accidental is the fact that it *must* arch toward the turnpike with a somewhat symmetric arch. (Why? Because a positive technical interest rate faces a zero subjective rate of time preference, making it optimal to abstain now from some consumption in order to get more later and, of course, to dissave later.) Finally, *AE* gives the maximum of utility for the finite interval (*O*, *T*), ending up like a feckless annuitant with zero terminal capital. For it the world ends neither in a bang nor a whimper, but on an upbeat of consumption.

We can summarize Figure 3's contents in the aphorism:

> Just as a rope hangs toward the ground in a catenary, the optimal path arches toward the Bliss turnpike.

The Theorem Stated

Now in Figure 3, merely increase the time span *T*, holding in every case

the terminal k^T at the same A, B, C, A', and E levels as before. Economic intuition tells us that *all* the paths will then have to arch closer to the Ramsey path or, what is the same thing, to the Bliss turnpike itself. E.g. in Figure 3 the branch $A'E'$ already hinted that AE will veer toward the turnpike if T is made large enough.

The Consumption Turnpike Theorem. An extremal to

$$\underset{\{k(t)\}}{\text{Max}} \int_0^T u[f(k) - \dot{k}]dt \quad \text{for} \quad k(0) = k^0,\ k(T) = k^T$$

will, as T becomes sufficiently large, spend an arbitrarily large portion of the time arbitrarily near the turnpike. I.e., $1-\epsilon$ of the time interval will be spent within a neighborhood η of the Bliss turnpike, where ϵ and η can be made as small as we like provided we stipulate T sufficiently large.

I shall first give a proof to the theorem along the lines of my 1959 Stanford discussion of the usual Turnpike Theorem, and the Samuelson-Solow 1956 generalization of the Ramsey problem.[5]

Stability analysis of the behavior of the differential equation (5) in the neighborhood of the Bliss point where $(k, \dot{k}) = (k^*, 0)$ requires examination of the linear first-degree terms in (5)'s Taylor's expansion around that origin. As with conventional small vibration analysis, this leads to the associated linear system for $k - k^* = y$

$$L_{11}\ddot{y} - L_{22}y = 0 \tag{6}$$

where

$$L_{11} = \left.\frac{\partial^2 u}{\partial \dot{k}^2}\right|_{\substack{k=k^* \\ \dot{k}=0}} = +\, u''[f(k^*) - 0] < 0$$

$$L_{22} = \left.\frac{\partial^2 u}{\partial k^2}\right|_{\substack{k=k^* \\ \dot{k}=0}} = +\, u'[f(k^*)]f''(k^*) < 0$$

$$L_{12} - L_{21} = 0 = \text{coefficient of the } \dot{y} \text{ term.}$$

[5] P. A. Samuelson, "Efficient Paths of Capital Accumulation," in K. J. Arrow, S. Karlin, and P. Suppes, eds., *Mathematical Methods in the Social Sciences, 1959* (Stanford 1960), pp. 77–88. P. A. Samuelson and R. M. Solow, "A Complete Capital Model Involving Heterogeneous Capital Goods," *Quart. Jour. Econ.*, Nov. 1956, 537–62. In the errata slip of the Stanford book, it was pointed out that only in the one-degree-of-freedom case does the $(L_{12}-L_{21})$ coefficient of the velocity $\dot{y}$ cancel out. As Professor Morishima pointed out to me, in the n-variable case the $\dot{y}$ of p. 88 equation (34) vector is premultiplied by a nonzero matrix. Fortunately, however, it is a skew-symmetric matrix which preserves the essential catenary property—that all roots come in pairs with opposite-signs. Professor Morishima has also kindly pointed out that some errors are contained in the pp. 333–34 discussion of R. Dorfman, P. A. Samuelson, and R. M. Solow, *Linear Programming and Economic Analysis*, New York 1958; still the saddlepoint analysis there of Fig. 12-9 can be recast along the lines of Figure 4 here.

The solution to (6) is seen to be the catenary

$$(7)\quad y(t) = A_1 e^{\lambda t} + A_2 e^{-\lambda t} \quad \text{where} \quad \lambda = +\sqrt{L_{22}/L_{11}}, \text{ a real number.}$$

Figure 4 shows the contours of motion, $H(\dot{k}, k) = \overline{H}$ from (3), in the important phase space $(k, \dot{k})$. In most physics problems, as in the conservative oscillations of a trapeze star around the *bottom* of his pendulum-like swing, the trajectories are along closed energy contours (being concentric ellipses for small, sinusoidal vibrations). But here, and for the trapeze star near

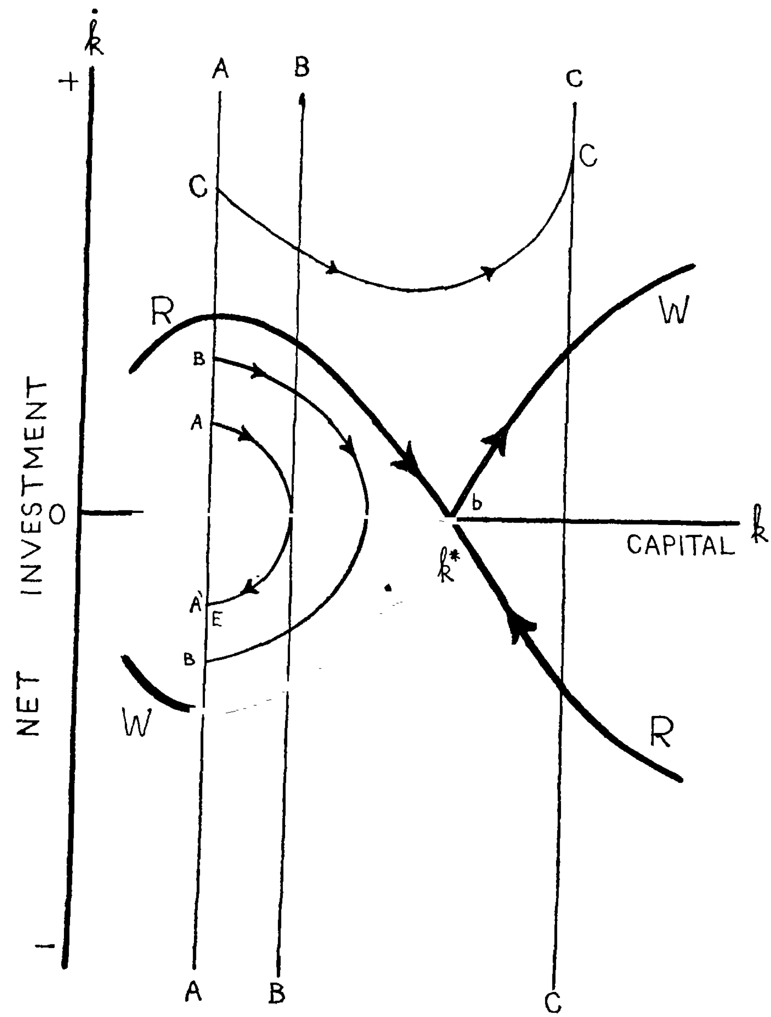


FIGURE 4

the *top* of his swings, the catenaries give rise to saddlepoint contours.

The point b represents Ramsey's equilibrium Bliss point. It is at the intersection of the heavy curve RbR, which represents the AR optimal Ramsey path of Figure 3 and WbW, which represents a Ramsey path that has been departing from Bliss since time immemorial (and which has no simple economic interpretation).

All of the paths that began at A in Figure 3 begin on vertical line AA in Figure 4. And each ends on the vertical line that corresponds to its terminal k^T. Thus the old path AB in Figure 3 appears as BB in Figure 4. And AC, AD, AE become respectively the paths CC, DD, and EE.

Time is not shown directly on Figure 3. Obviously, near the equilibrium point b the motions all slow down and it takes longer and longer to traverse the indicated paths. Indeed one never quite gets from R to b, since as b is approached speed slows down. Now the essence of the turnpike theorem is this:

If we go between any two vertical lines in Figure 4 and specify that it shall take us a longer and longer time to do so, we must begin and end nearer and nearer to the intersections of those lines with RbR or WbW. Thus, AA' must approach R and W along the vertical line AA. While the "small vibration" analysis by which I deduced catenaries here and in the 1959 Stanford paper involve only "local" considerations, the reasoning is valid globally: our strong concavity assumptions makes the saddlepoint topologically invariant and makes analysis in-the-small congruent with analysis in-the-large.

Instead of working with the variables $(\dot{k}, k)$, we could work with Hamilton's canonical variables $(p = \partial u/\partial \dot{k} = -u', k)$ and verify the same catenary, saddlepoint properties. But there is no need to do so on this occasion.

Generalization to Any Number of Variables

While I have here followed Ramsey in talking of only one consumption and one capital good, Solow and I have shown in the cited 1956 paper how the analysis generalizes to any number of variables. Now we replace $c + \dot{k} = f(k)$ by the general production-possibility frontier

(8) $$c_1 + \dot{k}_1 = f(k_1, k_2, \cdots; c_2 + \dot{k}_2, \cdots)$$

with a saturation $p-p$ frontier

$$c_1^* = f(k_1^*, k_2^*, \cdots, k_n^*; c_2^*, \cdots)$$

and

(9) $$\text{Max}\, u[c_1, c_2, \cdots] = u[c_1^*, c_2^*, \cdots] = 0 \geq u(c_1, \cdots, c_n).$$

The system will stay arbitrarily close to this c^* turnpike most of the time if we let $T \to \infty$ and hold terminal k's constant. This provides the easy generalization of the new turnpike theorem. Its proof need only be sketched here.

As in our 1956 paper define

$$\underset{\{c_1,\cdots,c_n\}}{\text{Max}}\ u[f(k_1, k_2, \cdots; c_2 + \dot{k}_2, \cdots) - \dot{k}_1, c_2, \cdots]$$
$$= U(k_1, k_2, \cdots; \dot{k}_1, \dot{k}_2, \cdots)$$

a concave function with negative-definite matrix of second partial derivatives. Note $U(k_1^*, k_2^*, \cdots; 0, 0, \cdots) \geq U(k_1, k_2, \cdots; 0, 0, \cdots)$. The optimal Euler extremals are

$$\frac{d}{dt}\frac{\partial U}{\partial \dot{k}_i} - \frac{\partial U}{\partial k_i} = 0$$

and the catenary property of the notion around the Bliss-turnpike $(k_1^*, k_2^*, \cdots)$ is assured by the fact the roots of the characteristic polynomial

$$\text{(10)} \qquad \det\left[\frac{\partial^2 U^*}{\partial \dot{k}_i \partial \dot{k}_j}\lambda^2 + \left(\frac{\partial^2 U^*}{\partial \dot{k}_i \partial k_j} - \frac{\partial^2 U^*}{\partial k_i \partial \dot{k}_j}\right)\lambda + \frac{\partial^2 U^*}{\partial k_i \partial k_j}\right] = 0$$

come in pairs of opposite sign (and which cannot be pure imaginaries because of the concavity-definiteness condition on U).[6]

Application to Golden Ages[7]

Until now I have followed Ramsey and kept population and labor constant. With $L(t) = \text{constant}$, mention of it in $f(k)$ could be suppressed. Now, following Chakravarty and others, I assume the neoclassical production function

$$\text{(11)} \qquad C + \dot{K} = F(K, L) = LF\left(\frac{K}{L}, 1\right)$$

$L(t) = L^0 e^{gt}$, an exponential growth for labor where g is the natural rate of population growth.

There are many possible integrals that might be maximized when L grows and choosing among them is not easy. I follow the crowd and posit

[6] Cf. Samuelson-Solow, *op. cit.* Section V. The small vibrations of physics there omit the middle skew-matrix, as is proper where the $\dot{S}_i$ velocities enter quadratically; but we erred in the economics case in not including a nonzero skew-symmetric matrix of the Morishima type. I am indebted to Professor Morishima for an example of a knife-edge case of semi-definite Hessian matrix of a concave U, for which (10) has pure imaginary roots, requiring us to go to nonlinear analysis to confirm the general catenary property. Also, he has produced a direct proof of this Consumption Turnpike Theorem that does not involve stability analysis.

[7] T. W. Swan, "Of Golden Ages and Production Functions," presented at the Round Table on Economic Development in East Asia (International Economic Association), Gamagori, Japan, April 1960 [revised 1962], mimeo., 18 pp.; E. S. Phelps, "The Golden Rule of Accumulation," *Am. Econ. Rev.*, Sept. 1961, *51*, 638–43; J. Robinson, "A Neoclassical Theorem," *Rev. Econ. Stud.*, June 1962, *29*, 219–26, with Comments by R. M. Solow, P. A. Samuelson, J. E. Meade, *et al.*; C. C. von Weizsäcker, *Wachstum, Zins und Optimale Investitionsquote*, (Kyklos Verlag, Basel, 1962), 96 pp.

(12) $\text{Max} \int_0^T U\left(\frac{C}{L}\right) dt$, where $U\left(\frac{C}{L}\right)$ is utility of the representative man.

With minor changes in notation, we can now throw this problem into the Ramsey form, the only difference being that we interpret k, c, and u as per capita terms. Definitionally,

$$k = \frac{K}{L}, \quad \frac{\dot{K}}{K} - \frac{L}{L} = \frac{\dot{K}}{K} - g = \frac{k}{k}$$

$$\frac{C}{L} = c, \quad U\left(\frac{C}{L}\right) = u(c)$$

Dividing (11) by L and subtracting $g(K/L) = gk$, we rearrange to get

$$c + k = f\left(\frac{K}{L}\right) - g\frac{K}{L} = f(k) - gk \equiv \psi(k) \tag{13}$$

Note that (13) looks to be exactly like our original Ramsey production relation in (1) and Figure 1, which was the special case of (13) when $g=0$ and labor is fixed.

We must now reinterpret the Ramsey Bliss convergence conditions. With labor growing exponentially at the rate g, the counterpart of Bliss is the Phelps-Swan-Robinson Golden-Rule state of maximum *per capita* consumption, defined by

$$\underset{\{k\}}{\text{Max}}\, \psi(k) = \underset{\{k\}}{\text{Max}}\, f(k) - gk = \psi(k^*) \tag{14}$$

where

(15) $f'(k^*)$ = interest rate = g, the system's natural rate of growth.

Turn back to Figure 1 to verify that the maximum vertical distance of $F(K/L)$ above $g(K/L)$ comes at the point k^*.

Now the integral of (12) will become convergent for infinite T if we measure utility as a (negative) divergence from the Golden-Rule state $c^* = f(k^*)$, at which we set $u(c^*) = 0$.

All the rest of the new turnpike theorem goes exactly as before: per capita catenaries, saddlepoint, etc. We may state it briefly as follows.

> *Per Capita Consumption Turnpike Theorem.* Let a system with exponentially growing labor begin and end with specified per capita capital stock $(k^0, k^T) = (K^0/L^0, K^T/L^T)$. If the time that elapses becomes sufficiently large, the system will spend an indefinitely large fraction of its time as near as one wishes to the Golden-Rule turnpike configuration of maximum maintainable per capita consumption. And exactly the same holds if k and c are interpreted as vectors of diverse capital goods and consumption items per capita, $(k_1, k_2, \cdots)$ and $(c_1, c_2, \cdots)$.

Loosely, one might say, even if we do not know when the world will end,

if only we can expect that date to be indefinitely far in the future, we should prudently emulate the Puritan Ethic or the Law of Moses and the Prophets: Accumulate! Accumulate! Accumulate! But *not* faster than Ramsey's Law and only if you share the philosophy of no time preference.[8] Actually, though, no loose statement can do justice to the Theorem, which says what it says, not more and not less.

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The Unseen Hand in Government

For a long time economists have given a great deal of attention to the "invisible hand" that guides resource allocation in the private sector of the economy. We have spelled out a theory of the firm, models of the private sector, conditions for achieving a limited kind of optimality, and formidable lists of limitations on the functioning of the mechanism. With the growth of government, we should probably devote more effort to understanding the mechanism that guides resource allocation in the public sector of the economy. We need a theory of government to supplement our theory of the firm, and more insights into the limitations of the invisible hand in the governmental sector. In this article I wish to take at least a few additional steps in these directions.

I. *The Private Sector*

It is usually said that the necessary conditions for Pareto optimality are met when the marginal private cost of each output (*MPC*) equals marginal private benefit (*MPB*), marginal total benefit (*MTB*), and marginal total cost (*MTC*).[1] That is, we would like to have the "welfare equation" satisfied:

$$MTC = MPC = MPB = MTB.$$

With appropriate institutional arrangements, the price mechanism guides individual decisions so that they tend to yield this result. As many persons have

[8] Suppose society has a systematic subjective rate of time preference ρ, so that the integrand of (1), $u(c)$, is replaced by $u(c)e^{-\rho t}$. Then a new "turnpike" is given by the root of $f'(k)=\rho$ rather than equal zero. Call this k^{**}. Then for T sufficiently large, for all (k^0, k^T), the optimal path $k(t)$ will remain indefinitely near to k^{**} an indefinitely large fraction of the time. The motions are not quite balanced catenaries, instead approximating to $k(t)=a_1e^{\lambda_1 t}+a_2e^{-\lambda_2 t}$, $\lambda_1>\lambda_2$. (When $g>0$, we of course find the root of $f'(k)-g=\rho$.) This constitutes an even more general Turnpike Theorem than the one elaborated here, and it reminds us that correct saving must depend on what is assumed about social time preference. Professor Karl Shell, now of M.I.T., and Dr. David Cass, now at the Cowles Foundation, have unpublished work dealing with similar problems.

[1] Although it may be a small semantic issue, I prefer to talk of "total" costs and benefits, which suggest that they are felt by individuals, instead of "social" costs and benefits, which may sound as though some mysterious entity like the state feels these effects.

emphasized, however, there are numerous limitations on the performance of this mechanism in the private sector. A degree of monopoly power leads to inequalities in the "welfare equation." Externalities lead to other inequalities.[2] Vaccinations bestow benefits on nonusers of this service, and vapors emitted by vehicles inflict costs on others that are not felt by those who take the actions. These spillover effects arise because it is sometimes uneconomical, or even impossible, to define and enforce property rights in an appropriate fashion, e.g., so that people could charge automobile drivers for damaging their rights to smog-free and noise-free air. (In the long run, one of the most important externalities may be the discrepancy between the private and total costs of having additional children. As a consequence, if we solve enough technological problems, we may ultimately have population congestion that rivals automobile congestion.) Also, it is often said that the actions of firms to increase profits leads to the concentration of power in great corporations, yielding a spillover impairment of freedom.

Another point that is sometimes raised is that firms really engage in satisficing rather than utility-maximizing, producing different outcomes from those corresponding to the welfare equation. (Actually, satisficing and utility-maximizing may turn out to be identical as soon as one recognizes that there is uncertainty, that acquiring information is costly, and that re-examining all the alternatives every five minutes would be expensive.) Another limitation frequently cited is that there is not perfect knowledge and that misinformation or lack of information leads us far from economic efficiency (as it might be conceived, in these circumstances, by some omniscient observer).

In certain kinds of advertising, firms devote resources to nullifying the efforts of other firms, much as people at parades stand on tiptoe to nullify the effects of others standing on tiptoe; and, especially if we ignore any stimuli to the introduction of new products, this type of advertising appears to yield something less than optimality. In addition, due to a variety of institutional constraints, there are restrictions on the ability of people to consider tradeoffs —there are tie-in sales, restrictions on entry into occupations, "throwaway TV" programs instead of pay-TV. Another troublesome blemish on the unseen hand is a special type of spillover—the fact that utility functions are not really independent. What Jones buys *does* affect my utility. As some see it, a final crushing blow is dealt by the theory of second-best. If all conditions for optimality are not fulfilled, we cannot always be certain that progress toward fulfilling particular conditions is truly desirable. All in all, there is a lot of palsy in the unseen hand, and it is no wonder that many persons took upon the private sector with some disapproval.

II. *The Public Sector*

Before drawing hasty conclusions about what should be done, however, we should look at the public sector from these same standpoints. In the public

[2] Doing anything about them sometimes costs more than it would gain, in which case the "departure from optimality" is much like our failure to have a world free of disease and boll weevils. See the excellent discussion of such matters by Harold Demsetz in [5].

sector too, choices are made by individuals. Politicians, executives, and agency heads are similar to managers of businesses; other personnel are laborers; and voters can perhaps be viewed as boards of directors. Inevitably each person has a separate utility function, a "parochial" viewpoint so to speak. These individuals do not arise each morning and ask, "what can I do today for Pareto optimality?" Like the rest of us, they are surely utility-maximizers.

This does not mean that they are selfish, brutish, or motivated solely by self-interest narrowly conceived. It simply means that individuals get satisfaction from a variety of things—material goods, play, helping others, performing tasks well, and so on—and that these things are to some extent substitutes for each other. As a consequence, if the cost to a person of one item increases, he will demand less of it and more of other items. If the benefit he feels from an item increases, he will demand more of it and less of other things. In government, if the cost to an official of one action increases, he will take less of it. If the gains that he feels increase, he will take more action of that sort.[8]

Also, in government as well as in the private sector, there is a mechanism that leads utility-maximizers to a pattern of decisions that is somewhat orderly and sensible. This mechanism is the bargaining process and, at least in Western nations where many interests are represented, it has some similarities to the price mechanism in the private sector of the economy. The price system makes individuals feel many of the relevant costs and gains produced by their decisions. When a business firm takes action, it has to bargain with and compensate numerous persons who supply buildings, labor services, and other inputs. That is, if the firm's action uses up or damages property, the firm ordinarily has to buy the consent of the owners. Whenever the firm's action produces beneficial effects, the management tries to charge the beneficiaries. If all compensations are made and the firm still makes a profit, some persons are made better off without making others worse off. The greater the extent to which all these compensations are made, the less the extent to which the firm's costs and gains will diverge from total costs and gains.

In government the bargaining mechanism produces some of the same effects. If a public official's action will use up someone's property or damage certain interests, he will probably find a cost associated with that action. He will feel the complaints of those damaged or the inconvenience of trying to mollify them. Or, he may suffer embarrassing or expensive enmities among his colleagues or retaliation by other officials. He has to bargain with many people who are affected and, in one way or another, encounter costs if he makes decisions that impose sacrifices on others. From those who are benefited, on the other hand, he can bargain for compensation. The reward may be support in connection with other matters, reduced enmity, increased friendship or convenience, or some other kind of *quid pro quo*. The size and completeness

[8] One of the best presentations of the utility-maximization hypothesis is by Armen A. Alchian and William R. Allen [1, Ch. 2].

of the compensations for both costs inflicted and gains bestowed depend upon bargaining strengths and circumstances (as they do in the private economy). And again the greater the extent to which these compensations are made, the less the extent to which the costs and gains felt by an official will diverge from total costs and gains.

Every decision or action, it might be noted, involves bargaining, tacit or explicit. When a senator considers voting to censure Senator McCarthy, he asks himself, "what will be the consequences?" If he decides to accept the reactions of others to a yes vote, *that* is his bargain. If he decides to refrain from voting or to vote "no," he accepts a different set of reactions, and *that* is his bargain.

As in the private sector, shifts in costs or gains (i.e., prices paid or received) cause shifts in behavior. As conditions change, the price of apples sometimes goes up and the price of oranges down. Consumers alter their behavior (though not necessarily their nature). Similarly, if a senator from Texas becomes President of the United States, the price or cost of some actions goes up and the cost of other actions goes down. For example, the cost of closing down a base in New York goes up, the cost of closing an installation in Texas goes down. The structure of rewards also alters. The rewards for a war on poverty in the entire United States go up, those for aid to constituents in a particular State go down. Thus a man who becomes President will take more nearly a national viewpoint—i.e., will acquire a greater "sense of responsibility"—though his nature need not change at all. The altered cost-reward structures are bound to affect his behavior. And, like the price system, this bargaining mechanism has many desirable effects. It might be called the "unseen hand in government."[4]

For these reasons, while the particular values, principles, or even whims of a government official occasionally play roles in his decisions, they do not usually play major roles in democratic governments. The bargaining mechanisms limit the discretion of decision-makers. Let us look at this side of the coin—the constraints on the authority of individual officials. Again the forces at work are akin to those that operate in the private sector. In a highly competitive industry, a business firm must give much attention to avoiding losses and increasing profits or it will fail. In these circumstances employees find that they must devote most of their efforts to achieving the firm's goals or be fired. In a public utility or nonprofit corporation, both managers and employees have greater leeway, but they too are limited in the extent to which altruistic, evil, or personal aims can guide their actions. In government also, while managers and other personnel have some discretion, they cannot flout the wishes of voters, superiors, and colleagues and hope to survive long. They certainly cannot do just whatever they wish. They must compromise with their principles, whether good or evil, and accept a great deal of guidance from the unseen hand.

[4] C. E. Lindblom has presented closely related arguments and has referred to "the hidden hand in government" [8]. For additional thoughts about the unseen hand and government spending, see [9].

The fact that individual decision-makers have parochial viewpoints does not mean that the pattern of decisions will inevitably be stupid or vicious. The right kind of barganing process can make special interests and parochial viewpoints, which one might think would produce chaotic decisions, lead to an orderly and sensible pattern of choices. If well designed, the invisible hand can go a long way toward turning private "vice" into public virtue, in government as well as in the private sector.

The mechanism may seem like a Rube Goldberg device in comparison with a mathematical model of optimality, since government decision-making appears to cater to majorities of large groups rather than to fine differences in individual preferences, and it appears to respond to voters' views on packages of issues rather than on each individual issue. But the mechanism is not quite as imprecise as that. As for voters, minority views on particular issues carry some weight, because there is the threat that those voters, when considering the larger package of issues at election time, may shift their votes and in effect form a new coalition that has real power. Differential intensity of feelings or different values attached to particular policies can carry weight for essentially the same reason. If a minority attaches a high value to a particular policy, it can sacrifice its feelings about other issues and join the coalition of its choice. Others react, as they see the "prices" being paid for various decisions, by accepting a little less of those items that are becoming relatively expensive and a little more of those items that are becoming relatively cheap. In legislative bodies, similarly, views on individual issues as opposed to a package, the views of minorities, and differences in the values attached to particular policies *do* help shape decisions, because some tradeoffs—giving up a vote on this issue for someone else's vote on that one, giving up some of one policy for more of another —are possible, and "shadow prices," so to speak, gradually emerge to reveal the costs and gains from various actions. With checks and balances, a multiplicity of interests represented in the bargaining process, and some possibilities for voluntary "exchange," the pattern of choices can cater to individual preferences better than may be suggested by the words "majority rule."[5]

This mechanism has *some* influence on most choices, though the extent of this influence varies greatly with circumstances.[6] In the aggregate these choices and decisions shape the allocation of resources at various levels. Consider first the allocation of resources between the private and public sectors. Suppose a larger share for government is under consideration Senators and congressmen will become aware of constituents' views on this and other issues and will respond to prospective votes almost the way a board of directors re-

[5] For some provocative contributions to the development of a theory of government to supplement the theory of the firm, see the work of Buchanan, Downs, Olson, Tullock, and Williams [4] [6] [10] [11] [13].

[6] For example, such influence is much smaller wherever decision-makers are partially sheltered from the necessity of bargaining, e.g., the Supreme Court or agencies that are relatively independent. Such independent checks and balances, however, can often play a beneficial role in the over-all bargaining network.

sponds to prospective profits. These legislators will "feel" many of the expected benefits and (probably to a lesser extent) the expected costs. The benefits are likely to be concentrated on a smaller group and be larger per-person-affected than are the costs (mainly taxes), but legislators will feel some pressure from both groups and will anticipate some reaction at election time from both groups.

Second, consider the allocation of resources within the public sector among programs and activities (comparable to the allocation of resources among industries). Officials can take home no profits, and they are spending other people's money; yet they may "feel" the major gains and costs because some tradeoffs are possible and crude "shadow prices" emerge from the bargaining process. An official finds that it *does* cost him something to expand his program—as well as bring rewards. Resources tend to be shifted toward programs in which marginal individual benefit (*MIB*) is greater than marginal individual cost (*MIC*), and bargaining helps make *MIB* equal to *MTB* and *MIC* equal to *MTC*. The hidden hand harnesses individual decisions so that they come a little closer to satisfying the welfare equation than they would if no such mechanism existed.

Finally, consider the utilization of resources within a program by lower-level officials (comparable to the allocation of resources by firms within an industry). The alternative methods of production that are considered depend upon constraints that proscribe certain of the alternatives and pressures that induce personnel to examine more or fewer alternatives. The costs and gains from each alternative, as perceived by government personnel, depend upon the criteria in terms of which personnel are judged, the importance of efficiency to survival of the agency or branch, and so on. Again the gears of the mechanism may appear to clank quite a bit; but rivalry and the bargaining process still work to internalize what would otherwise be externalities, and make gross inefficiency somewhat costly to government personnel. To some extent, flagrant inefficiency impairs an official's ability to bargain for promotion, larger budgets, freedom from investigation, and other desiderata. Compared with discretionary authority, then, the bargaining mechanism is again a valuable and unseen hand guiding resource utilization.

In the public sector too, however, we find that there are numerous limitations. We tend to blame faulty outcomes in government upon individuals, perhaps because some of them are in the limelight. We are prone to say that things go wrong because of stupidity, sloth, weak character, or some other personal inadequacy in particular government officials. Yet to a great extent, as in the private sector, there are limitations on the system that should make us expect faulty outcomes. We should understand these limitations better in trying to appraise alternative arrangements and devise improvements.

Counterpart of Monopoly

The bargaining process is extremely imperfect in government. Monopoly models do not fit precisely, of course; for one thing utility-maximization by

monopolies in the private sector implies a good deal of emphasis on obtaining higher profits, while in the public sector it implies a good deal of attention to obtaining higher budgets. Nonetheless there are somewhat analogous resource misallocations. An agency with strong bargaining power may not *restrict* its output inefficiently, but it will tend to seek approval of below-cost pricing and *expand* its output inefficiently. An agency with weak bargaining power is likely to find its activities curbed even if it would be economic to expand them. Pressure by voters and the unseen hand may tend to correct these situations, but in a slow and exceedingly imprecise fashion.

Another way in which the imperfect bargaining process in government distorts output is through the "holdout." Sometimes a project affecting several States or counties or other governmental units would be economic from the standpoint of total benefits and costs, yet uneconomic from the standpoint of any *one* of the governmental units. A deal would be mutually advantageous, and sometimes this happens. (In 1964 one city in Los Angeles County decided not to have its annual fireworks display, but an adjoining city said that it reaped benefits too, so after some bargaining, compensation was arranged, and the fireworks display was presented.) Yet often the last State or county refuses to bargain, hoping for a "free ride." Similar phenomena occur with respect to agencies within a single jurisdictional authority such as the federal government. Centralization, incidentally, may reduce the problem of the holdout, but impairs the functioning of the bargaining mechanism in other ways.

Entry of new agencies to replace obsolete ones is probably more difficult than the entry of new firms with new ideas in private industries. Reservoir-building agencies are ardent spokesmen for conventional methods of producing water. There are relatively few spokesmen for certain alternative methods of increasing water supplies, such as shifting water from lower-valued uses to higher-valued uses, reclaiming waste water, or reducing losses from seepage, evaporation, and vegetation. For new methods or products to be *seriously* considered, it often requires entry by a new branch or agency and a chance for it to build up a clientele. But entry in the public sector is difficult, and it often takes a long time to drive obsolete activities into "bankruptcy."

So much for examples of the effects of "imperfect competition" in government. The point is merely that the counterparts of oligopoly and monopoly distort prices and outputs in the public as well as in the private sectors.

Externalities

Another limitation on the unseen hand in government is the pervasiveness of externalities. Authorities who approve a new irrigation project can impose costs they do not feel on firms, individuals, and other government agencies. This can happen, for example, because irrigation introduces additional salts or pesticides into downstream or underground water supplies. Some projects even affect the porosity of soils and the underground flows of water. Sewage disposal may impose costs on others and education may bestow benefits on others that the bargaining process does not always cause local governments to

feel in reaching these decisions.[7] Federal officials, in setting tariffs or price supports, are inflicting costs that the bargaining process does not make them feel very keenly.

It is difficult to assess the size of these externalities because officials feel costs and gains attributable to their choices mostly through bargaining pressures, and these are hard to measure. Parochial outlooks as such do not reveal what costs or gains are felt. In the private sector, a businessman does not say "I want to be sure and take into account *all* the costs to the nation," and an uncritical observer might conclude that there must therefore be tremendous externalities. But the price system compels the businessman to consider (most of) the costs he causes. In the public sector, similarly, a government official does not say, "I want to be sure and count all the costs to the nation." Again a naïve observer might conclude that there must therefore be great externalities. But the bargaining process compels the official to take at least many of the costs into account.

Still, while the net externalities are hard to assess, they appear to be widespread in government. Third parties who have little or no bargaining power are often affected. Or, another way to put the matter is to point out the peculiar, sometimes indeterminate, nature of property rights within government. (Who has the right to do what with various assets in the public sector has important implications just as it does in the private sector.) The common pool problem—the resource that is treated as though it is a free good—in the private sector is still more in evidence in the governmental sector. This is not to say that there should be a massive shift of activities from the government to the private sphere. It is simply to say that in terms of the "welfare equation" and Pareto optimality, the public sector too is in plenty of trouble.

As far as the concentration of power is concerned, utility-maximization in government surely generates this spillover to a greater extent than in the private sector. The ultimate possibilities of discretionary authority in government surely loom larger than those in the world of giant corporations. In one famous Jules Feiffer cartoon, an employee of the telephone company says: If you don't like our service, why don't you try one of our competitors?[8] This may appear to be a devastating thrust at the concentration of power in the private sector, but the same sort of cartoon could apply to many services provided by government.

Other Imperfections

As for "satisficing" rather than maximizing, this is surely as applicable in government as it is in business. There are standards of performance, standards of water quality and housing, rules regarding the use of government cars, and rules of thumb regarding all manner of things. As in the private sector, rules of thumb are often better than not having them, since reviewing al-

[7] For a provocative case study and discussion of spillovers in education, see [7].

[8] From *The Explainers* by Jules Feiffer. 1960. McGraw-Hill Book Company. Used by permission.

ternatives and acquiring information are costly activities [3]. But they are there, and in that sense governments can be said to "satisfice" rather than maximize. Uncertainty, too, causes the same complications in the public sector that have been discussed so much with reference to the private sector. Even advertising is present in government, as agencies seek to hold or expand their clienteles [12, pp. 120-21].

Restrictions on tradeoffs, another point sometimes mentioned in connection with the private sector, pervade the government sphere. In order to control what is actually done in this environment, Congress must authorize appropriations for specified categories and prohibit significant transfers of resources without requests for reprogramming permission. Moreover reprogramming requests are not only an inconvenience; they are often frowned upon by Congress. Furthermore, the Departments, in order to control what is actually done, must "shred out" narrower categories and discourage transfers of resources among them. This means that substitution possibilities must often be neglected. As noted before, it may be better to have such rules of thumb or restrictions than not to have them, but their existence, like any similar phenomena in private industry, is a departure from the ideal.

There are other obstacles to the consideration of alternatives and tradeoffs in government. Small jurisdictions elect representatives at large, and there the wishes of minority groups, i.e., certain sets of alternatives, tend to be neglected. If a metropolitan government consisted of councilmen elected at large, or a state legislature consisted of assemblymen elected at large, the majority might well ignore the "needs" of minorities or local areas and the costs thrust upon them. The actual outcomes would depend upon the kind of behind-the-scenes logrolling mechanism that developed.

Finally, when we turn to the public sector, we do not escape the interdependence of utility functions or the troublesome implications of the theory of second-best. The success of Administrator Jones affects my utility function, and either deliberate or by-product favors to some voters affect the satisfaction of others. As for second-best, it is omnipresent. The full conditions for optimality in the public sector are definitely not fulfilled. We are destined to live in a world of *N*th best and to wonder at times if apparent improvements really make us better off.

III. *Conclusions*

What significance does the unseen hand in government, and its imperfection, have for us? First of all, just as background in thinking about government activities, we ought to keep in mind the existence of this invisible hand and the way it works. It tends to harness individual interests within government to carry out broader objectives. It keeps parochial viewpoints from yielding exclusively parochial policies. But we must keep in mind the formidable imperfections of the mechanism—the fact that there are major spillovers affecting parties who have inadequate voices in the bargaining process. Moreover, we should recognize that many questionable policies or choices are

inherent in the institutional framework and should not be blamed on "bad" officials or bad luck. Where the bargaining process does not eliminate or offset serious spillovers, the cost-reward structures confronting officials tend to pull them toward wrong decisions.

As far as comparing the two sectors is concerned, the only thing that is obvious is that both are imperfect.[9] In fact the whole discussion underscores the limitations of Pareto optimality as a guide to policy. Actions that can clearly make some persons better off without making others worse off constitute a small set, and for a number of reasons we cannot possibly expect many governmental actions to be in that set. It may seem unfair and irrelevant, therefore, to appraise government choices in terms of the conditions for Pareto optimality. If this seems inappropriate, however, it is surely inappropriate also to call for government action wherever the conditions for economic efficiency are not fulfilled in the private sector. One ought not argue that the government should control an activity *because* the private sector does not bring about Pareto optimality and *then* regard it as irrelevant that government control does not bring about such optimality either.

We should not favor having an activity in one sector or under one particular regulatory arrangement *merely because the alternative is less than perfect.* Our choice must rest on the view reflected in a remark sometimes attributed to Maurice Chevalier: when asked how he felt about growing older, he is said to have replied, "It's not exactly ideal, but it's better than the alternative."

A more important point, perhaps, is the following: we should learn more about the unseen hand in government and its shortcomings in order to improve institutional arrangements affecting both sectors. We should study the anatomy of market failure, the anatomy of government failure, and the possibilities of improving the functioning of both. In the government sphere in particular, a better understanding of the unseen hand and its limitations may help us improve the bargaining framework that shapes public decisions.

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Substitution and Values of Elasticities

It is commonplace in intermediate price-theory texts, as well as elementary texts, to explain variations in the coefficient of price elasticity in terms of the number and closeness of substitute products. That is, a relatively large number of close substitutes results in a large (in absolute value) coefficient of price elasticity, and a relatively small number of substitutes results in a small coefficient of price elasticity.

To take a recent example, (and other examples can be found in nearly any intermediate theory text) consider Professor Watson's approach to the concept. He says:

> If a commodity has many close substitutes, its demand is almost certain to be elastic; perhaps highly so. If price goes up, consumers buy less of the commodity and buy more of its substitutes. If its price goes down, consumers desert the substitutes and buy the commodity in (relatively) much larger quantities [2, p. 40].

Later, in describing the causes of variations in the coefficient of cross elasticity, Professor Watson makes the following statement:

> The closer two commodities are as substitutes for each other, the greater is the size of the cross-elasticity coefficient. Close substitutes have high cross elasticities of demand. Two commodities are poor substitutes for each other, if the cross elasticities are low [2, p. 100].

After encountering practically the same explanation for the two different

concepts, a reader might question the usefulness of the coefficient of cross elasticity. If the degree of substitutability is expressed in the coefficient of price elasticity, why introduce another elasticity coefficient to represent the same phenomenon?

Watson, as well as other authors of intermediate theory texts, face the problem of incorporating the basic concepts of substitutability and complementarity into the theory of demand. They have tried to accomplish this integration using the Marshallian approach based upon the constant marginal utility of money. Hicks demonstrated that this approach neglects the income effect; and hence substitutability and complementarity can only be treated in a satisfactory method if the income effect is included.

Using Hicks's results, we will show why substitutability must be associated with both the (direct and cross) price elasticities and corresponding components of the income elasticity. It is the only way in which one can accurately discuss substitutability of two goods in the consumer's market basket whenever the income effect is significant.

Our argument will be based upon the Fundamental Equation of Value [1, p. 309]. Using Hicks's notation, the substitution term, X_{ij}, can be expressed as follows:

$$(1) \qquad X_{ij} = \frac{\partial x_i}{\partial p_j} + x_j \frac{\partial x_i}{\partial M} \quad \text{where} \quad i, j = 1, 2, \cdots, n.$$

It can be shown that the substitution term has the following properties:

$$(2) \qquad X_{ii} < 0 \qquad \text{for } i = 1, 2, \cdots, n$$

$$(3) \qquad X_{ij} = X_{ji} \quad \text{for } i, j = 1, 2, \cdots, n \text{ and } i \neq j$$

$$(4) \qquad \sum_{j=1}^{n} p_j X_{ij} = 0, \qquad i = 1, 2, \cdots, n.$$

Stating the first set of equations in matrix terms, we have:

$$(1') \qquad (X_{ij}) = \begin{bmatrix} X_{11} & X_{12} & \cdots & X_{1n} \\ X_{21} & X_{22} & \cdots & X_{2n} \\ \cdot & \cdot & \cdot\ \cdot\ \cdot & \cdot \\ X_{n1} & X_{n2} & \cdots & X_{nn} \end{bmatrix} = X$$

The matrix X, commonly referred to as the Slutsky matrix, has all negative terms on the diagonal. Hence for each good, the direct substitution effect is always negative.

The matrix X is symmetric; that is, the substitution effect of the ith good for the jth good is equal to the substitution effect of the jth good for the ith good.

Substituting the respective Slutsky term in (4), we obtain:

$$
(5)\quad
\begin{aligned}
&p_1\left[\frac{\partial x_1}{\partial p_1}+x_1\frac{\partial x_1}{\partial M}\right]+p_2\left[\frac{\partial x_1}{\partial p_2}+x_2\frac{\partial x_1}{\partial M}\right]+\cdots+p_n\left[\frac{\partial x_1}{\partial p_n}+x_n\frac{\partial x_1}{\partial M}\right]=0\\
&p_1\left[\frac{\partial x_2}{\partial p_1}+x_1\frac{\partial x_2}{\partial M}\right]+p_2\left[\frac{\partial x_2}{\partial p_2}+x_2\frac{\partial x_2}{\partial M}\right]+\cdots+p_n\left[\frac{\partial x_2}{\partial p_n}+x_n\frac{\partial x_2}{\partial M}\right]=0\\
&\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\\
&p_1\left[\frac{\partial x_n}{\partial p_1}+x_1\frac{\partial x_n}{\partial M}\right]+p_2\left[\frac{\partial x_n}{\partial p_2}+x_2\frac{\partial x_n}{\partial M}\right]+\cdots+p_n\left[\frac{\partial x_n}{\partial p_n}+x_n\frac{\partial x_n}{\partial M}\right]=0
\end{aligned}
$$

If we assume that a positive quantity of each good is demanded (i.e., $x_i>0$ for all i), we can divide the first equation in (5) by x_1, the second by x_2 and so on. Doing this division and completing the multiplication we obtain:

$$
(6)\quad
\begin{aligned}
&\left[\frac{p_1}{x_1}\frac{\partial x_1}{\partial p_1}+\frac{p_1x_1}{x_1}\frac{\partial x_1}{\partial M}\right]+\left[\frac{p_2}{x_1}\frac{\partial x_1}{\partial p_2}+\frac{p_2x_2}{x_1}\frac{\partial x_1}{\partial M}\right]+\cdots\\
&\qquad+\left[\frac{p_n}{x_1}\frac{\partial x_1}{\partial p_n}+\frac{p_nx_n}{x_1}\frac{\partial x_1}{\partial M}\right]=0\\
&\left[\frac{p_1}{x_2}\frac{\partial x_2}{\partial p_1}+\frac{p_1x_1}{x_2}\frac{\partial x_2}{\partial M}\right]+\left[\frac{p_2}{x_2}\frac{\partial x_2}{\partial p_2}+\frac{p_2x_2}{x_2}\frac{\partial x_2}{\partial M}\right]+\cdots\\
&\qquad+\left[\frac{p_n}{x_2}\frac{\partial x_2}{\partial p_n}+\frac{p_nx_n}{x_2}\frac{\partial x_2}{\partial M}\right]=0\\
&\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\\
&\left[\frac{p_1}{x_n}\frac{\partial x_n}{\partial p_1}+\frac{p_1x_1}{x_n}\frac{\partial x_n}{\partial M}\right]+\left[\frac{p_2}{x_n}\frac{\partial x_n}{\partial p_2}+\frac{p_2x_2}{x_n}\frac{\partial x_n}{\partial M}\right]+\cdots\\
&\qquad+\left[\frac{p_n}{x_n}\frac{\partial x_n}{\partial p_n}+\frac{p_nx_n}{x_n}\frac{\partial x_n}{\partial M}\right]=0
\end{aligned}
$$

The first term in each bracket in (6) is a direct or cross price elasticity of demand. The other terms become economically recognizable if we consider the components of the income elasticity of demand for the ith good:

$$
(7)\quad \frac{p_1x_1}{x_i}\frac{\partial x_i}{\partial M}+\frac{p_2x_2}{x_i}\frac{\partial x_i}{\partial M}+\cdots+\frac{p_nx_n}{x_i}\frac{\partial x_i}{\partial M}=\frac{M}{x_i}\frac{\partial x_i}{\partial M},\qquad i=1,2,\cdots,n.
$$

The sum of all of the fractional components of the income elasticity for the ith good equals the income elasticity for the ith good. So letting

$$\epsilon_{ii} = \frac{p_i}{x_i}\frac{\partial x_i}{\partial p_i},\ \epsilon_{ij} = \frac{p_j}{x_i}\frac{\partial x_i}{\partial p_j},\ \epsilon_{iM} = \frac{M}{x_i}\frac{\partial x_i}{\partial M} \text{ and } \eta_{ij} = \frac{p_j x_j}{x_i}\frac{\partial x_i}{\partial M},$$

we can write (6) as follows:[1]

$$(8)\qquad \begin{aligned} (\epsilon_{11} + \eta_{11}) + (\epsilon_{12} + \eta_{12}) + \cdots + (\epsilon_{1n} + \eta_{1n}) &= 0 \\ (\epsilon_{21} + \eta_{21}) + (\epsilon_{22} + \eta_{22}) + \cdots + (\epsilon_{2n} + \eta_{2n}) &= 0 \\ \cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots \\ (\epsilon_{n1} + \eta_{n1}) + (\epsilon_{n2} + \eta_{n2}) + \cdots + (\epsilon_{nn} + \eta_{nn}) &= 0 \end{aligned}$$

where

$$\sum_{j=1}^{n} \eta_{ij} = \epsilon_{iM} \quad \text{for all } i = 1, 2, \cdots, n.$$

The η_{ij} represents the contribution of the jth good to the income elasticity of the ith good, ϵ_{iM}. (It might be looked upon as the percentage change in demand for the ith good relative to the percentage change that income is of the expenditures upon the jth good.) Hence, the terms in each parenthesis in (8) are price (direct and cross) elasticities and the fractional components of income elasticity.

Hicks [1, p. 311] defines two goods x_1 and x_2 to be substitutes if the substitution term $X_{ij}>0$ and to be complements if $X_{ij}<0$. Using these definitions, the above relationships imply for all i, j:

$$(9)\qquad (\epsilon_{ij} + \eta_{ij}) < 0 \quad \text{if } i = j.$$

$$(10)\qquad (\epsilon_{ij} + \eta_{ij}) > 0 \quad \text{if } i \text{ and } j \text{ are substitutes and } i \neq j.$$

$$(11)\qquad (\epsilon_{ij} + \eta_{ij}) < 0 \quad \text{if } i \text{ and } j \text{ are complements and } i \neq j.$$

Hence, commodity i can be a substitute for commodity j in each of the following cases ($i \neq j$).

$$(12)\qquad \epsilon_{ij} < 0,\ \eta_{ij} > 0 \quad \text{and} \quad |\epsilon_{ij}| < |\eta_{ij}|$$

$$(13)\qquad \epsilon_{ij} > 0,\ \eta_{ij} < 0 \quad \text{and} \quad |\epsilon_{ij}| > |\eta_{ij}|$$

$$(14)\qquad \epsilon_{ij} > 0, \quad \text{and} \quad \eta_{ij} > 0$$

Moreover, they also imply that commodity i can be a complement of commodity j if $|\epsilon_{ij}| > |\eta_{ij}|$ in (12), $|\epsilon_{ij}| < |\eta_{ij}|$ in (13), and the inequalities are reversed in (14).

Using Hicks's definitions, the above argument demonstrates whenever two goods are compared that they can be substitutes even though the cross

[1]The reader should recall that

$$\sum_{j=1}^{n} p_j X_{ij} = 0; \quad \text{so} \quad \sum_{j=1}^{n} \frac{p_j}{x_i} X_{ij} = \sum_{j=1}^{n} (\epsilon_{ij} + \eta_{ij}) = 0, \qquad i = 1, 2, \cdots, n.$$

elasticities are negative. Furthermore, it shows that two goods can be complements when the cross elasticities are positive.

Considering a definition such as the first one cited above (referring to price elasticity and many substitutes) and the relationships in (8), another inconsistency may be noted. We can illustrate this if we rewrite those relationships as follows:

$$\sum_{\substack{j=1 \\ \& \, i \neq j}}^{n} (\epsilon_{ij} + \eta_{ij}) = -(\epsilon_{ii} + \eta_{ii}), \qquad i = 1, 2, \cdots, n. \tag{15}$$

Suppose now that the commodity i has many substitutes as well as many complements and that the sum on the left in (15) is 0.1. Further suppose that the contribution of commodity i, η_{ii}, to its income elasticity, ϵ_{iM}, is 0.1. Then it follows that the coefficient of price elasticity, ϵ_{ii}, must equal $-.2$, an extremely inelastic (price) demand. Hence, it is possible for a commodity to have many substitutes and many complements and still have an inelastic price demand.

Use of elasticities to classify substitutes and complements can result in classifications contrary to common sense and everyday observation. For example, a price decline in hamburger could enable a poor person to eat more beefsteak. The utilization of the cross-elasticity definition would classify these obvious substitutes as complements, while Hicks's definition would not. The explanation, of course, is the income effect. Hicks formulated his definitions in terms of the substitution effect to overcome pitfalls such as this.

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The Monetary Effect of Long-Term Debt Finance

In his contribution to the series of research studies prepared for the Commission on Money and Credit [1], James Tobin has advanced an interesting new approach to the problem of debt management. We would like to point out, however, that while we feel that the entire article makes an excellent contribution to economic theory, there is one major conclusion which requires qualification.

In his essay [1., pp. 158-67] Tobin contends that an increase in total government debt may well have expansionary monetary effects, regardless of the maturity of the new issue. His argument is that all government debt, even long-term debt, is a better substitute for money than for equity capital.

Hence, an increase in government debt will increase the demand for capital. This, argues Tobin, can happen even if the rate of interest on long-term government bonds must rise in order to insure absorption of the new issue. Indeed, this rise in the rate on long-term governments is the very result which had traditionally been relied upon in attempts to demonstrate that the monetary effect of long-term debt finance must be contractionary. This, as Tobin correctly points out, reflected the incorrect implicit identification of the supply price of capital with the rate on long-term governments.

While it must be admitted that the increase in the quantity of government debt may reduce the supply price of capital by increasing the demand for capital, Tobin's model also includes private debts. These are presumably good substitutes, in lender portfolios, both for government debt and for equity capital. Therefore, will not an increase in government debt raise interest rates on private debts? Given the importance of private debt in corporate finance, this will discourage demand for capital and offset in part, in whole, or more than fully the favorable effects of increased liquidity in the form of government debt.

The resulting monetary effect of an increase in long-term government debt hinges, therefore, upon the relative substitutability of private debts for government debt as opposed to equity capital. If private debts are better substitutes for government debt, then the rates on these two assets will move together, both rising. If, on the other hand, private debts are better substitutes for equities, then *these* two rates will move together, both falling. In the appendix to his essay, Tobin implicitly assumes the latter to the case.[1] He thus concludes that increases in long-term government debt will unambiguously increase investment.

We believe, however, that the analytical approach used by Tobin to fix long-term government debt as a better substitute for other government debt than for corporate equities [1., pp. 162-67], leads one to conclude, as well, that private debts are better substitutes for government debt than for equities. They possess the purchasing-power risk and the same market risks as government bonds. They also do not share the capital-gains possibilities of equities. While they do share the default risk with equities, this does not seem to offset the above factors which operate in the opposite direction, especially since private debts possess prior claim to earnings and assets.

In terms of Tobin's model, our contention is that, in equation (P6) of the Appendix, i.e., the public demand for capital, $C = c_p\ (r_2, r_3, r_4, r_5)\ W$, it is more likely that r_4 (Tobin's rate on private debts) will rise as M_3 (long-term government debt) is increased. The higher is r_4, the lower will be the demand for capital, and r_5 (the supply price of capital) may even have to rise to restore equilibrium. In this case, the sign of $\partial r_5/\partial M_3$ is positive, and long-term debt finance is contractionary. The result can go either way, and depends upon the magnitude of $\partial r_4/\partial M_3$, or how closely private debts and

[1] This explanation for the difference between his results and ours was given to us by Tobin as one of his comments addressed to an earlier draft of this note. We would also like to point out that nowhere in the text of his essay does Tobin discuss the role of private debts in his model.

government long-term debt are related. A weak relationship may simply temper the fall in the supply price of capital, while a strong relationship will turn this into a rise. Since the result, in the latter case, would be contractionary, it is not correct to say that an increase in long-term government debt is unambiguously expansionary.

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Engineering Data and the Production Function

The idea that a production function can be synthesized from engineering data is an appealing one; and the recent work of Mordecai Kurz and A. S. Manne [5], drawing on technical information from the metal-machining industry, provides a welcome contribution to the literature. However, while this new study directs thinking into some interesting channels, it also raises a number of fundamental questions concerning the nature and definition of a production function—questions which must be considered if empirical models are to be linked meaningfully to the standard formulations of production found in pure theory.

I. *The Censoring Rule*

Kurz and Manne are concerned with the general problem of capital-labor substitution, but base their analysis on some rather detailed engineering estimates revealing the possibilities for direct substitution between specific types of machine tools. The data used represent the findings of an earlier study by Markowitz and Rowe [6] and indicate the capabilities of 115 different machine tools to perform 129 alternative metal-working tasks. Various productivity coefficients are established, and these supply information on the number of pieces per day that can be produced by one worker employing a particular type of machine tool to accomplish a given "task" (as defined by size of piece worked, geometrical shape, etc.).

A central objective of the K-M paper is to organize the available productivity data within a comprehensive production function. However, the thing that immediately attracts attention is the unusual conception of a production function which seems to be held. The interpretation made is apparent from the following passage:

> The original M-R data were intended to represent a set of feasible technological alternatives and included many possibilities that would be ir-

relevant except under short-term emergency conditions. For example, in the performance of task 2, M-R allowed altogether for 22 alternative machine tools. . . . Apparently, M-R utilized so much imagination in specifying machines to deal with short-run emergencies that their raw data are virtually useless for analyzing the long-run substitution possibilities between capital investment and direct labor.

Accordingly, we decided to delete many of the inefficient machines. The following censoring rule was applied separately for each task: If, in the performance of that task, one machine tool had a higher invesment cost and not a higher output than a second machine tool, the first was said to be "inefficient" and was deleted from our analysis [5, pp. 664-66].

Now, from the viewpoint of orthodox theory, the K-M position can be criticized on two major points: (1) The production function proposed is not established independently of input prices and, thus, if any price changes, a new array of efficient processes will tend to appear.[1] (2) The "prices" used in the censoring operation are investment outlays rather than charges per unit of productive service, but the analysis is conducted as though no distinction need be made between capital and current costs.

II. *Capital Goods and the Theory of Production*

It is possible to formulate a production function on the assumption that one or more of the inputs is a capital stock rather than a service flow [7]. However, when this is done, allowance must be made for the changed dimensions of the inputs. Failure to recognize the difference between current inputs and capital inputs can cause difficulties both for the understanding of productive relationships and the interpretation of rational economic behavior. It is precisely this problem which seems to plague much of the K-M analysis.

From the explanation given [5, pp. 665-66], the K-M censoring rule was established for the specific purpose of giving order and meaning to certain engineering data which appeared anomalous. That is, a plot of (i) output per machine per day on (ii) investment cost per machine was made for various types of machine tools, and the points obtained fell into a random pattern. This outcome was regarded as extraordinary and untenable—the K-M presumption was that a positive correlation should be observed between variables (i) and (ii). And it was to secure an "appropriate" pattern that the censoring rule was invoked. Quite simply, all points inconsistent with the anticipated correlation had to be deleted.

But is the K-M hypothesis valid? When one capital good has a greater supply price than another, is it necessary that the former show greater physical productivity per day than the latter in order to be "economic"? Clearly, the answer here is no. Information on one-period productivity does not afford a sufficient basis to judge the desirability of an investment which yields

[1] Whether or not a new array of processes appears after a shift in the price structure depends on the nature and magnitude of the individual price changes and on the pattern of physical productivity differences between the capital inputs.

effects over many periods;[2] considerations other than initial expenditure and current physical productivity must enter into the economic calculation. Two capital goods with widely different supply prices (e.g., $Z_i > Z_j$) may give the same productivity results in the immediate period, but, beyond this, the economic consequences for the firm of possessing one factor or the other may be quite dissimilar, so that the more costly good (Z_i) can be, on balance, the better choice. For example, expensive capital equipment which can be adapted readily to changing production requirements need be no more productive physically, in any given period, than narrowly specialized equipment; but, nevertheless, the adaptable factor may promise a greater rate of return over its lifetime than the specialized one. Or, again, the cheapest and least durable[3] capital good that is able to perform a particular function is not necessarily the ideal one from the standpoint of optimization over time [1].

In general, therefore, we find that no reliable inferences concerning the relative economic potentials of capital goods can be drawn from a simple consideration of capital costs and one-period productivity data. The K-M rule must be judged defective on this ground if no other.

III. *Input Prices and Technical Alternatives*

Insofar as the supply prices of machine tools (Z_i, $i = 1, 2, \ldots n$) are to be used to develop a set of relatively promising investment options, the obvious strategy is to compare the prices in question with corresponding present values (V_i, $i = 1, 2, \ldots n$) which can be estimated for the different tools. In other words, granting that present value is the logical productivity index to compare with investment cost,[4] the K-M rule might be reformulated as follows: If, in the performance of a given task, one type of capital good has a higher supply price per unit and not a higher present value than a second type of capital good, the first good is "inefficient" and must be rejected as a valid production alternative.

Now, following this test of efficiency, a number of possible operating combinations can, presumably, be obtained and a "production function" of sorts constructed. Yet a basic difficulty exists. Prices, whether they are prices of productive services or of capital goods, have no role in the determination of a true production function [8, p. 2]. In the case at hand, the censoring rule implies that the set of capital inputs designated as "efficient" can change with each shift in the price structure: $Z_1, Z_2, \ldots Z_n$. And this is not all, for it is apparent that the composition of the "efficient" set must also be influenced by the valuations: $V_1, V_2, \ldots V_n$. But, the latter are, in essence, *subjective estimates* made by entrepreneurs and, thus, there can be, at any moment, as

[2] Even the effective life span of a capital good cannot be defined without reference to the general economic matrix [2].

[3] Since the initial cost, or supply price, of a capital good tends to change in some systematic way with longevity [1, pp. 314-16], the firm may often be in a position to choose from among an array of capital inputs serving the same function, supplying the same type of productive service, and yielding the same current product, but costing different prices.

[4] A plot of V_i on Z_i will almost certainly reveal a different, and more meaningful, pattern than that of Figure 1 [5, p. 666].

many efficient sets (or production functions) as there are individuals considering investment.

Any introduction of input prices into the analysis suggests that the objective is not to find a production function, but the minimum cost operating combination for some output.[5] However, even if it is the latter which is sought, the K-M rule is not very helpful. To obtain the solution to this *economic* problem it is necessary to scan the array of all existing Z_i and V_i with a view to discovering the one particular type of capital equipment which promises to satisfy the production requirements and to provide the greatest rate of profit over the planning interval [3, pp. 33-36].

It follows from the discussion that a decision must always be made on the kind of solution desired. If an *economic* solution is wanted, both price and technological data have to be employed; and, given a specific set of prices, it should be possible to determine a corresponding (optimal) operating point for the firm. On the other hand, if a *production function* is to be established, information on prices is not needed. The efficient technical alternatives can be separated from the total array of processes on the basis of objective physical criteria[6] [4, p. 44]; the problem here is technical rather than economic. Needless confusion is created whenever the distinction between these two levels of choice is blurred.

IV. *Conclusion*

Theoretical considerations lead to the conclusion that the censoring rule is invalid as stated by Kurz and Manne, and incapable of useful revision. Since a true production function cannot be defined, it is obvious that any application of the rule to empirical cases will tend to be misleading. The rule plays a crucial role in econometric research because it decides the data employed in statistical analysis. Thus, in the K-M study, the original 1,143 productivity estimates are reduced to a total of 290 after the rule is applied [5, p. 666]; and the latter values constitute the statistical observations treated by least-squares regression. But since these data are necessarily biased, the quantitative results obtained are open to question, and the whole work is undermined.

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[6] This generalization must be qualified if it is assumed that a process of production, as such, can convey utility or disutlity. For, in such case, the process has to be judged not only by its capacity to yield output of the prime commodity, but also by the value of the secondary products it generates.

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The Cost of Capital, Corporation Finance, and the Theory of Investment: Comment

In the June 1958 issue of this *Review*, Franco Modigliani and Merton H. Miller (hereafter MM) presented a model of corporate leverage which challenged the traditional view that an optimum capital structure exists. They demonstrated quite convincingly that, within the context of the model's assumptions, arbitrage operations will make the market value of the firm independent of its capital structure. While considerable attention has been given to the validity or "realism" of these assumptions,[1] little has been given to the feasibility of the tests MM proposed to distinguish between these two views of the effects of leverage. These tests are based on what MM believed to be the implication of each view for the following two relationships: (1) that between the expected yield on a firm's shares and its debt-equity ratio; and (2) that between the weighted average of the expected yields on a firm's shares and bonds and its debt-equity ratio. The purpose of this communication is to show that these implications differ in important respects from what MM supposed them to be, and that even when the appropriate corrections are made, serious doubt remains as to whether either view can be contradicted by the data.

I. *Implications of the Alternative Views in the Absence of Corporate Income Taxes*

MM based their entire analysis on Proposition I, which asserts that "*the average cost of capital to any firm is completely independent of its capital structure and is equal to the capitalization rate of a pure equity stream of its class*"

[1] See, for example, D. Durand [1] and J. Lintner [2].

[3, pp. 268–69; their italics]. Proposition I may be written as follows:[2]

$$\rho_k = i\frac{S}{V} + r\frac{D}{V}, \tag{1}$$

where i and r are, respectively, the expected yields on the firm's shares and bonds, S is the value of its shares, D is the value of its bonds and $V=(S+D)$.[3] We may solve for the relationship between the expected yields on the firm's securities and its debt-equity ratio by rearranging (1) as follows:

$$i = \rho_k + (\rho_k - r)D/S, \text{ and} \tag{2}$$

$$r = \rho_k - (i - \rho_k)S/D. \tag{3}$$

The stock-yield function (2) is identical to their Proposition II [3, p. 271], which they similarly derived from Proposition I. However, MM failed to recognize that Proposition I also implies the bond-yield function (3). As a result, they posited a form for (2) which is inconsistent with their assumption about investors' attitudes toward risk. To determine the permissible forms which (2) can take, it will be necessary to first make clear the constraints which attitudes toward risk place upon both (2) and (3).

While the validity of Proposition I does not depend on the attitudes of investors toward risk as MM correctly argued, these attitudes nevertheless play an important role in their analysis. Thus, by making ρ_k greater than the expected yield on riskless bonds, they implicitly assumed risk aversion. From this it follows that, since shares become progressively more risky as leverage increases, the expected yield on shares must be an increasing function of the debt-equity ratio. MM neglected this corollary when they attempted to include a rising supply curve of borrowed funds in their analysis. To accommodate risky bonds, they assumed "as a first approximation that this yield curve, $r=r(D/S)$, whatever its precise form, is the same for all borrowers" [3, p. 273]. MM then supposed the marginal cost of borrowed funds could rise so rapidly as to cause the yield on shares to fall. This possibility is represented by the downward-sloping segment of the

[2] With some exceptions, which will be noted when they occur, we shall preserve the notation used by MM. For convenience, subscripts denoting individual firms have been omitted.

[3] This expression follows directly from their equation (4) [3, p. 268]:

$$\frac{\bar{X}}{(S+D)} = \frac{\bar{X}}{V} = \rho_k,$$

where $\bar{X}$ is the expected return on the firm's assets. Let $(\bar{X}-\bar{R})$ be the expected return on the firm's shares and $\bar{R}$ the expected return on its bonds so that $\bar{X}=(\bar{X}-\bar{R})+\bar{R}$. Letting $iS=(\bar{X}-\bar{R})$ and $rD=\bar{R}$, we have

$$\frac{(\bar{X}-\bar{R})}{V} + \frac{\bar{R}}{V} = \rho_k,$$

which is equivalent to (1) above. See note 5 for a more detailed discussion of these expected returns.

MD curve in their Figure 2 [3, p. 275] which is inconsistent with the assumed risk aversion.

Since these yield curves are interdependent, they need not be considered separately. Focusing on the properties of the share-yield curve, $i=i(D/S)$, risk aversion requires $i'>0$. The implications of this constraint can be seen by differentiating (2) with respect to D/S, which yields:

$$i' = (\rho_k - r) - r' \frac{D}{S}. \tag{4}$$

From (3) we know that $r=\rho_k$ when the firm is completely financed by debt, at which point the bonds are exactly equivalent to unlevered shares.[4] Because the distribution of returns to assets, $\phi(X)$, has some dispersion, bonds must become risky prior to this point so that r' must exceed zero beyond some finite level of leverage.[5] As a consequence the bond-yield curve is subject to certain general constraints independent of the exact form of $\phi(X)$.

Inspection of (4) reveals that $i'=0$ when $\rho_k - r = r'D/S$. Unless r' approaches zero as $\rho_k - r$ approaches zero, the share-yield curve will pass through a maximum as does MM's *MD* curve. To avoid this, r must approach ρ_k asymptotically as D/S approaches infinity which entails $r''<0$ over this same range. (In the limit, both r' and r'' tend to be zero.)

[4] The promised yield on these bonds could be infinite since the coupon must reflect the most favorable return on assets. However, the expected value of the coupon will be less than the promised value so that the expected yield on bonds will never exceed ρ_k.

[5] In their footnote 6 [3, p. 265] the return on the assets X is defined as a random variable subject to the probability distribution $\phi(X)$ which differs among firms by almost a scale factor. The expected return is

$$\bar{X} = E(X) = \int_a^b X\phi(X)dX,$$

where a and b represent, respectively, the lower and upper limits of $\phi(X)$ and $\int_a^b \phi(X)dX = 1.0$. The expected return on bonds is

$$\bar{R} = \int_a^R X\phi(X)dX + R\int_R^b \phi(X)dX,$$

where R is the promised coupon.

The expected return on shares is

$$\begin{aligned}\bar{\pi} &= \int_R^b (X - R)\phi(X)dX \\ &= \int_R^b X\phi(X)dX - R\int_R^b \phi(X)dX.\end{aligned}$$

Substituting from the expression for $\bar{R}$ yields

$$\begin{aligned}\bar{\pi} &= \int_a^b X\phi(X)dX - \bar{R} \\ &= \bar{X} - \bar{R}.\end{aligned}$$

Differentiating (4) with respect to D/S yields

$$(5) \qquad i'' = -2r' - r''D/S,$$

which is equal to zero when both r' and r'' are zero and when $2r' = -r''D/S$. If riskless bonds can be issued, i will be a linear function of D/S over this range. As debt becomes risky, we may suppose r' and r'' to both be positive so that i'' becomes negative. Since $r''<0$ for the upper range of D/S, i'' may well become positive as complete debt financing is approached. Share- and bond-yield curves with these characteristics are depicted in Figure 1.

The axes in Figure 1 are the same as those in MM's Figure 2 [3, p. 275], facilitating the comparison of their curves with ours. MA is the share-yield curve and BB' the bond-yield curve implied by our analysis. MD is the share-yield curve MM believed might result from a rising bond-yield curve. (Consistent with the ambiguity in their discussion of the bond-yield curve, they did not include one in their Figure 2.) $ML'G$ represents MM's interpretation of the traditional view that up to some "reasonable" amount of leverage, L_k, share yields remain constant but rise sharply beyond it.[6]

The shapes of our share- and bond-yield curves are to some extent arbitrary, since they depend on the exact forms of the density function $\phi(X)$ and the utility functions describing investors' attitudes toward $\phi(X)$, neither of which can be specified a priori. Even so, this particular pair of curves serves to illustrate, as could any one of a large number of others, the constraints imposed by risk aversion. The magnitude of L_b, the debt-equity ratio beyond which bonds become risky, could range from zero to some large finite value, its magnitude here depending on the particular form of $\phi(X)$ we assumed.

To distinguish between the traditional view and the MM model, the relationship between the yield on shares and the debt-equity ratio must be observed over a range where negative curvature could be detected if it exists. Clearly, a finding of positive curvature could contradict neither hypothesis.[7] However, this difficulty can be avoided by observing instead the relationship between the weighted average cost of capital and leverage. In the absence of taxes, the MM model implies a linear horizontal relationship whereas the traditional view implies a U-shaped one.[8] Observable behavior will reflect the impact of the corporate income tax so that its effect on the weighted average cost of capital must be taken into account. Let us turn, then, to MM's analysis of the effect of taxes.

[6] See [3, p. 277].

[7] If L_k can be determined a priori so that firms with $D/S>L_k$ can be excluded from the analysis, a finding of a positive slope for the share-yield curve would contradict the traditional view.

[8] See Figure 1 [3, p. 275] for a graph of these alternatives.

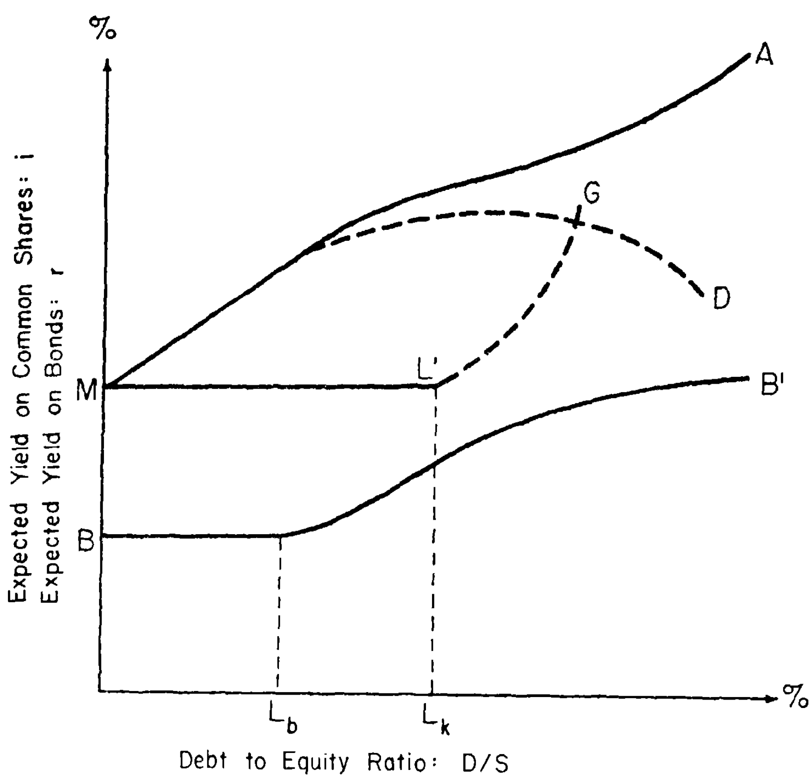


FIGURE 1

II. *Implications of the Alternative Views in the Presence of the Corporate Income Tax*

The special treatment accorded interest payments under the corporate income tax is likely to render regression analyses of the kind proposed by MM ineffective as a means of distinguishing between these alternative views for two reasons. First, as MM recognized in their recent correction [4] of their earlier discussion of the effects of the tax, the forms of the relationship between the weighted average cost of capital and debt-equity ratios are similar for both views under the tax. Second, and perhaps more important, the tax provides an incentive for rational managers to use debt. As we shall show, maximizing behavior is likely to lead to similar debt-equity ratios for all firms in the same risk class; consequently, the relationship between debt-equity ratios and the average cost of capital (and between the debt-equity ratio and the expected yield on shares as well) may be extremely difficult to estimate from the behavior of firms in a given risk class.

To illustrate these points, let us consider MM's equation (3) [4, p. 436], the counterpart to Proposition I corrected for the impact of taxes. It asserts that "the value of a levered firm of size $\overline{X}$, with a permanent level of debt

D_L in its capital structure" is as follows:

$$V_L = \frac{(1-\tau)\overline{X}}{\rho^\tau} + \frac{\tau R}{\dot{r}} = V_u + \tau \dot{D}_L,$$

where τ is the tax rate, ρ^τ is the cost of capital for an unlevered firm under the tax (or equivalently, for a levered firm under the tax if interest payments did not receive special treatment), and $\dot{r}$ is the riskless rate of interest. We have altered their notation, denoting the present value of the tax saving as $\tau \dot{D}_L$, to emphasize the fact that the tax saving is a function of the promised rather than the expected coupon.

The value of tax saving depends on the promised coupon, R, because the full amount of the promise is deductible from earnings in determining tax liabilities and can always be realized, if necessary, in the market for tax losses.[9] Because τR is thus certain, it must be discounted by the riskless rate of interest.[10] The promised and expected coupons are identical when bonds are riskless, so that no distinction between them need be made in this special case. However, this distinction becomes important when bonds are risky. The value of risky bonds is $D_L = \overline{R}^\tau / r^\tau$, the expected returns on the bonds including the possible returns from the sale of tax losses discounted by the expected yield.[11] Since $\overline{R}^\tau < R$ and $\dot{r} < r^\tau$, $\tau(\overline{R}^\tau / r^\tau) = (\tau D_L < \tau R/\dot{r})$.

The implications of this can be seen by rewriting their equation (3) as

$$V_L = V_u + \frac{\tau R}{\dot{r}} = V_u + \frac{\tau r^* D_L}{\dot{r}}, \tag{6}$$

where $r^* = R/D_L$, the promised yield. R can be quite large since the highest possible return before interest and taxes, which determines the minimum coupon for complete debt financing, can be quite large. Neglect of the dif-

[9] See footnote 5 [4, p. 435].

[10] This can be seen as follows: the expected return on a firm's bonds under the tax is

$$\overline{R}^\tau = \overline{R} + \tau \int_a^R (R - X)\phi(X)dX,$$

where $\overline{R}$ is as defined in note 5, and the second term is the increment due to the market for tax losses. The expected return on shares is simply

$$\bar{\pi}^\tau = (1-\tau)\int_R^b (X - R)\phi(X)dX.$$

The expected return on bonds and shares, $\overline{R}^\tau + \bar{\pi}^\tau$, is

$$\begin{aligned} \overline{X}^\tau &= (1-\tau)\int_a^b X\phi(X)dX + \tau R\int_a^b \phi(X)dX \\ &= (1-\tau)\overline{X} + \tau R(1.0). \end{aligned}$$

Thus, if a market for tax losses exists, τR is certain.

[11] Because the tax saving provides additional protection for the holder of risky bonds, the expected yield on them under the tax is lower than that given by the rising portion of BB' in Figure 1.

ference between promised and riskless yields understates the value of the levered firm by

$$\tau D_L \frac{(r^* - \dot{r})}{\dot{r}} \cdot$$

Equation (6) poses a problem which we shall mention only in passing; namely, if V_L varies directly with $\tau R/\dot{r}$, it appears that all firms would be completely financed by debt. However, this would destroy the market for tax losses, since firms would no longer have tax liabilities, with the result that τR would no longer be certain. Rather than considering here the implications of this, we shall assume that institutional constraints, such as those imposed by the Internal Revenue Service, suffice to prevent complete debt financing and so insure the existence of a market for tax losses.[12]

It follows from (6) that the weighted average after-tax cost of capital is

$$(7) \qquad \dot{\rho}^\tau = \frac{\overline{X}^\tau}{V_L} = \rho^\tau - \tau \frac{r^*}{\dot{r}}(\rho^\tau - \dot{r}) \frac{D_L}{V_L} \cdot$$

This differs from MM's expression for ρ^τ (in their notation, $\overline{X}/V_L$) as given in their equation (11.c) [4, p. 439] in two ways. First, the fact that the tax saving depends on the promised yield is made explicit by the factor $r^*/\dot{r}$: by omitting this factor MM implicitly assumed it to be unity, which is correct only for riskless bonds. And second, the fact that the tax saving is certain is made explicit by the identification of bond yield in the tax adjustment term as the riskless yield, $\dot{r}$. Since this tax adjustment term increases with leverage, ρ^τ is clearly a declining function of leverage. The rate of decline is greater over range of leverage for which bonds are risky than MM's equation (11.c) suggests.

To see how (7) differs from the weighted average cost of capital under the tax implied by the traditional view, consider MM's interpretation of this relationship as given by their equation (17), [3, p. 277], which we write as

$$(8) \qquad \dot{\rho}^\tau = i^\tau - (i^\tau - r^\tau)D/V$$

to make it clear that the expected yield on bonds is the relevant one in this function.[13] Differentiating (8) with respect to D/V reveals that

[12] There is nothing in the MM model which precludes complete debt financing by all firms. Indeed, they state that strategic considerations "not fully comprehended within the framework of static equilibrium models, either our own or those of the traditional variety [4, p. 442]," prevent such behavior.

[13] In [4, p. 439] MM restate their equation (17) [3, p. 277] as $\overline{X}^\tau/V = \rho^\tau - (\rho^\tau - r)D/V$ to facilitate its comparison with their equation (11.c). As our equations (7) and (8) make clear, this comparison is valid only when promised, expected, and riskless yields are identical; when bonds are risky, divergences between these yields make the two expressions more nearly alike for at least some range of leverage. Moreover, this version of the traditional view obscures the fact that i^τ increases for leverage beyond L_k. The substitution of ρ^τ for i^τ is appropriate only when i^τ is constant.

(9) $$\dot{\rho}^{\tau\prime} = i^{\tau\prime} - (i^{\tau} - r^{\tau}) - (i^{\tau\prime} - r^{\tau\prime})D/V.$$

For leverage less than or equal to L_k, (9) reduces to

(10) $$\dot{\rho}^{\tau\prime} = -(\rho^{\tau} - r^{\tau}) + r^{\tau\prime}D/V$$

because i^{τ} is then equal to ρ^{τ}, a constant. As long as bonds are riskless, $\dot{\rho}^{\tau\prime} = -(\rho^{\tau} - \dot{r})$, which is more steeply inclined than $-\tau(\rho^{\tau} - \dot{r})$, the slope of (7). But when the bonds become risky, and for leverage beyond L_k, the slope (8) becomes less steep—and hence more closely approximates that of (7) over some range of debt-equity ratios.

Because the traditional hypothesis does not place constraints on the share- and bond-yield curves like those imposed by the MM hypothesis, it is not possible to specify the exact shape of the weighted average cost of capital curve on a priori grounds. As a result, even though the slopes of (7) and (8) diverge over the range of leverage for which (8) turns up—presumably (8) is U-shaped under the tax as well as in its absence—it is not possible to determine the range of debt-equity ratios over which the slopes are similar. To distinguish between (7) and (8), then, requires observation of firms over nearly the entire range of possible leverage so that, if the traditional hypothesis were correct, the upward-sloping portion of the U-shaped curve could be observed. As we shall show, it is unlikely that firms with debt-equity ratios in the relevant range can be found.

Recourse to estimation of the share-yield curve is unlikely to circumvent this difficulty since the basic shape of MM's share-yield curve is unaltered by the tax. The following expression for the weighted average cost of capital under the tax, paralleling (1), will help to illustrate this:

(11) $$\dot{\rho}^{\tau} = i^{\tau}\frac{S}{V_L} + r^{\tau}\frac{D}{V_L}\cdot$$

From (7) and (11) it follows that

(12) $$i^{\tau} = \frac{\bar{\pi}^{\tau}}{S} = \rho^{\tau} + (\rho^{\tau} - r^{\tau})D/S - \tau\frac{r^*}{\dot{r}}(\rho^{\tau} - \dot{r})D/S,$$

which differs from MM's expression for $\bar{\pi}^{\tau}/S$ as given in equation (12.c) [4, p. 439] in the same way that our equation (7) differs from their equation (11.c); namely, by the presence of $r^*/\dot{r}$ and $\dot{r}$ in the tax-adjustment term. The share-yield curve described by (12) is similar in shape to the MA curve in Figure 1 but is displaced to the right.[14] Although (12) differs from the traditional view represented by $ML'G$ in Figure 1, a finding of positive curvature is consistent with both hypotheses. As in the absence of taxes, only a finding of negative curvature can serve to distinguish between them.[15]

[14] We abstract here from the possible implications of the tax for the yield on the shares of an unlevered firm; ρ^{τ} need not be equal to ρ_k.

[15] See note 7.

Turning now to the second and more intractable difficulty noted above, it would appear that firms in a given risk class would have utilized debt up to the limits of the institutional constraints facing them or up to the optimum amount, if such exists, to maximize V_L. Under both hypotheses, then, firms in a given class will tend to have the same debt-equity ratios. Consequently, a scatter diagram of either expected yields on shares or weighted averages of expected yields on bonds and shares plotted against debt-equity ratios should form a tight cluster (ideally a point).[16] The interpretation to be given the slope of a line fitted to such a scatter, such as the regression coefficients reported by MM [3, p. 281–87] for the oil and utility industries, is far from clear. Apart from measurement error, wide dispersion probably indicates the inclusion in the sample of firms from different risk classes. It may, therefore, be necessary to expand both hypotheses to include the effects of variation of institutional constraints and of optimal debt-equity ratios among risk classes to determine which of them describes the effects of leverage on market value in the real world. In the absence of such an undertaking it appears likely that tests quite different from those discussed here will be necessary to distinguish between the two hypotheses.

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The Cost of Capital, Corporation Finance, and the Theory of Investment: Reply

Although we can appreciate the ingenuity with which Messrs. Brewer and Michaelsen have developed parts of their argument, we fear that they have underestimated the limitations of their theoretical analysis and overestimated the significance of even their valid results for empirical applications.

[16] We abstract from the unsettled questions of how expected yields are to be measured or whether they exist in the sense used by MM. In much of the traditional literature expected and promised yields on bonds are not carefully distinguished. This might be the source of some of the current controversy, since the weighted average of expected yields on shares and promised yields on bonds will be a U-shaped curve under both hypotheses.

Taking up the various points in their paper in order, consider first their treatment of the much-mooted falling zone in the expected yield curve for shares. We referred to such a zone only very casually in our original paper and only because the possibility of its existence was a somewhat surprising implication of our Proposition I (namely, that with perfect capital markets, no taxes, and identical borrowing functions for firms and individuals, the value of any firm must be independent of its financial structure). We did not push the matter further at that time partly because we considered the point to be of no practical consequence and partly because there seemed to be no more that could validly be said about such a zone within the confines of our basic assumptions. Nothing in the Brewer-Michaelsen paper leads us to believe we were wrong in these judgments. To reason, as they do, that a ρ_k larger than the riskless rate of interest implies "risk aversion," which in turn implies ρ_k greater than r everywhere, and hence a monotonically increasing yield curve is merely to play with words. No precise definition of "risk aversion" in this context is provided, let alone a proof that risk aversion (everywhere?) is implied by ρ_k greater than riskless r. Nor is this very surprising. The concept of risk aversion may perhaps have some heuristic value for rationalizing the gross behavior of the yield curves in commonsense terms; but we doubt that the term can ever be defined with sufficient precision and generality to derive conclusions of the kind Brewer and Michaelsen assert.[1]

Brewer and Michaelsen are on sounder ground in their derivation of the shape of the share-yield curve, *given* a bond-yield curve (or vice versa). That, as they show, is a straightforward matter of curve tracing, and we have no particular quarrel with it. We must admit, however, to being puzzled as to why they think their discussion of the curvature properties has any significant bearing on problems of empirical testing. As they themselves acknowledge, there would always be other and more direct implications by which to distinguish the two models (in particular, by reference to the behavior of total market value in response to differences in capital structure). Even in terms of the yield curves, there would be no very serious difficulties, in principle, in distinguishing between their curves *MA* and *ML'G* (particularly since the slope and curvature of *MA* can be directly predicted, as they show, from *ML'G* and an estimate of ρ_k). Whether, in practice, the two curves could be distinguished by simple regressions of yield on leverage, is, of course, another matter. But the uncertainties surrounding the usefulness of this particular type of test have nothing much to do with the sorts of questions raised by Brewer and Michaelsen.

Turning next to the issue of the proper measure of the value of the tax saving on debt, we fear that their proposed new expressions are based on a mis-

[1] The difficulties that arise with respect to defining risk aversion are merely one symptom of what is the real obstacle to specifying the relations between ρ_k and r, namely that these relations can be adequately treated only in the framework of a general equilibrium model of valuation under uncertainty. Hopefully, recent advances in this direction by Arrow (in "Le Rôle des Valeurs Boursières pour la Répartition la Meilleure des Risques," *International Colloquium on Econometrics*, 1952) and developed further by Hirshleifer ("Investment Decision Under Uncertainty," forthcoming), may open up some new lines of attack.

understanding (for which we must take some of the blame) of what we meant by the "certainty" of the tax deduction for interest payments. What we perhaps should have said more clearly is that our formulas would be valid when the tax deduction was exactly as certain or uncertain as the interest payment itself (or, equivalently, that the government's liability to the creditors is essentially the same as that of an ordinary stockholder).[2] Operationally, this means that the amount of the interest deduction for tax purposes is conditional on the amount of the interest actually paid to the bondholders either by the issuing corporation, or, in the event of the sale of the issuing firm, by the corporation acquiring the issuing firm and its accumulated tax losses. If so, the present value of the tax saving on interest should be computed by discounting the "expected value of the tax saving"—the tax saving itself being in principle a random variable—at the very same rate the market applies to the stream of expected interest payments in arriving at the market value of the debt. And this, in turn, will lead to precisely our τD_L as the required present value. In practice, of course, the government's liability is not exactly the same as that of the stockholders. The complexity of our tax laws is such that cases can arise in which the government's liability to the creditors may be somewhat greater or may well be smaller. On the whole, however, we feel that our assumption represents a good first approximation; and certainly a far better one than that implied by Brewer and Michaelsen's equations (6), (7), and (12). These formulas, since they assume that the tax saving is certain in the literal sense, whether or not the interest is paid, amount to saying that the government assumes an absolutely unlimited liability to the bondholders and in perpetuity to boot!

Even if their formulas were acceptable descriptions of valuation under existing tax laws, we would find it hard to take seriously their claim to have disclosed new and "intractable" difficulties for empirical testing. For one thing, such discrepancies as would exist between their valuations and ours would be substantial only at levels of leverage far higher than any we normally observe. Nor would their higher estimate of the tax subsidy change the picture materially in the matter of choice of capital structure. We have noted

[2] In fact, under this "stockholder" interpretation for the government's share, it is possible to derive our tax formulas directly from the no-tax case. If we let the superscripts G, P, and T stand, respectively, for the government's "ownership" interest, the private sector's ownership interest, and the combined holdings of both groups; and if we assume that the government "owns" the fraction τ of the total common stock, then from Proposition I, we will have for an unlevered firm $V_U{}^T = S_U{}^P + S_U{}^G = \overline{X}/\rho_k$. The value of the purely private interest in that firm will then be

$$V_U^P = S_U^P = (1 - \tau)V_U^T = \overline{X}(1 - \tau)/\rho_k.$$

For a levered firm we will have

$$V_L^T = S_L^P + S_L^G + D_L^P = \overline{X}/\rho_k,$$

so that the private interest is

$$V_L^P = S_L^P + D_L^P = D_L^P + (1 - \tau)[\overline{X}/\rho_k - D_L^P] = V_U^P + \tau D_L^P$$

exactly as in our equation (3).

many times that if one looks only at the tax subsidy to debt in our model (or at the "gains" from leverage under the simple traditional model), then one might expect every firm in the class to have the same debt ratio and that it would be as large as the tax laws or creditor restrictions permit. Similarly, under the more sophisticated traditional model, all firms would presumably always be at the unique "optimum" debt ratio for the class. In the real world, however, such tight clustering does not seem to occur; and the differences in capital structure in most industries we have looked at are larger than can be convincingly accounted for by measurement errors or mixing of risk classes. We have always acknowledged that we have no completely specified model to account for these observed differences, though we think we can see some of the important elements out of which such a more general theory will someday be built.

In the meantime, however, empirical research need not grind to a halt. Differences in capital structure, for whatever reasons, do exist and they can be exploited to shed much light on the controversy over the effects of financial policy on market valuation. This is not to say, of course, that the empirical problems are easy or straightforward; on the contrary, they present a most severe challenge to the econometrician. Until this challenge has been accepted by finance specialists, may we propose a moratorium on all further speculations about what might or might not be true about valuation?

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Structural Unemployment: Comment

In a recent paper [1], Lowell Gallaway has entered the controversy over the reasons for the rising trend in the unemployment rate in the United States in the 'fifties and early 'sixties. He applies a novel test which purports to distinguish between two alternative explanations for the rising trend and concludes "that the structural unemployment hypothesis is not a valid explanation of the increase in unemployment that has marked recent developments in the U.S. economy" [1, p. 712]. The purpose of this note is to show that Gallaway's test cannot distinguish between the alternative hypotheses and to suggest a test that can.

Briefly, the two alternative explanations are: (1) the level of aggregate demand has increasingly become inadequate, and (2) structural changes in the economy, due mainly to rapid technological change, have led to higher levels of frictional unemployment [2]. If the latter explanation is the correct one, Gallaway argues, the distribution of unemployment among the various sectors of the economy would be altered. "Therefore, if the structural argument is valid, the distribution of unemployment during the 1957-60 cycle should differ from that during the previous cycle" [1, pp. 710-12].

Gallaway refers to a straightforward method of measuring the degree of

change in relative unemployment in a sector that was developed by a joint Economic Committee Study [2]. The method consists of regressing sectoral or group unemployment against total unemployment and time for each sector or group and testing whether the regression coefficient of the time variable is significant. He finds that this method "requires some judgment as to what constitutes a meaningful number of sectors in which the time factor is statistically significant" [1, p. 711], and he suggests an alternative method which avoids this kind of judgment. The alternative consists in computing an R statistic for the period before 1957, and one for the period after 1957.[1]

> If the structural unemployment hypothesis is borne out, the R value for the 1957-60 period will be lower than that for the pre-1957 period, as unemployment will not be as strongly correlated between various sectors in the post-1957 period [1, p. 712].

The calculations made for the three different sectoral classifications (industrial, occupational, and regional) of unemployment all lead to an R statistic for the pre-1957 period smaller than for the 1957-60 period. On the basis of these results, Gallaway concludes that the structural unemployment hypothesis is not valid.

Unfortunately, the conclusion remains unsupported because R does not measure the distribution of unemployment among a number of sectors at a point of time, or over a period of time, and therefore the comparison of two such statistics cannot be interpreted to measure the change in such a distribution. The R statistic is simply a measure of the extent to which the sectoral unemployment rates are correlated with one another. It is quite possible to have the sectoral unemployment rates perfectly correlated with one another within two separate periods of time, and yet have a change occur in the distribution of unemployment among the sectors from the first period to the second period. The following hypothetical example will illustrate the contention.

Assume there are three occupational sectors of equal weight with unemployment rates over six years as given by Table 1. The six years are divided into two periods, Period I consisting of the first three years and Period II consisting of the remaining three years. The unemployment rates were chosen so that no reasonable doubt would arise about change in the structure of unemployment rates from Period I to Period II. For any given level of total unemployment, the sectoral unemployment rate of the sector with the lowest un-

[1] R is defined by Gallaway [1, p. 703] as:

$$R = \frac{\sigma^2 - \hat{\sigma}_0^2}{\hat{\sigma}_1^2 - \hat{\sigma}_0^2}$$

where σ^2 is the actual variance of the aggregate unemployment series, $\hat{\sigma}_0^2$ is the predicted variance assuming the correlation coefficient for any pair of series equal to zero, and $\hat{\sigma}_1^2$ is the predicted variance assuming the correlation coefficient for any pair of series equal to one. In terms of the variances of the sectoral unemployment series (σ_i^2's and σ_j^2's) and the weights of the individual series in the aggregate series (α_i's and α_j's) $\hat{\sigma}_0^2$ and $\hat{\sigma}_1^2$ can be written as:

$$\hat{\sigma}_0^2 = \sum_{i=1}^{n} \alpha_i^2 \sigma_i^2 \qquad \hat{\sigma}_1^2 = \sum_{i=1}^{n} \sum_{j=1}^{n} \alpha_i \alpha_j \sigma_i \sigma_j$$

employment rate has been decreased by one percentage point, and the sectoral unemployment rate of the sector with the highest unemployment rate has been increased by one percentage point as one moves from Period I to Period II. If Gallaway's procedure were correct, the R statistic for the second period ought to be significantly lower than the R statistic for the first period. That is not the case. Both R values are equal to each other and equal to one.[2] Clearly, Gallaway's conclusion is incorrect.

TABLE 1—UNEMPLOYMENT RATES BY OCCUPATION: A HYPOTHETICAL EXAMPLE

Occupation	Weight	Unemployment Rate (per cent)							
		1	2	3	Average First 3 Years	4	5	6	Average Last 3 Years
X	1/3	1	2	3	2	0	1	2	1
Y	1/3	1.5	3	4.5	3	1.5	3	4.5	3
Z	1/3	2	4	6	4	3	5	7	5
Total	1.0	1.5	3	4.5	3	1.5	3	4.5	3

The hypothesis as framed by Gallaway implies a sudden increase in structural unemployment from the period before 1957 to the period after. This does appear a limiting hypothesis because of the nature of the presumed causes: technological change, population change, and changes in consumer preferences. It would be more reasonable to expect that, if structural unemployment were increasing, the increase would be a gradual rather than a sudden one. In this case too, it is possible to construct clear examples in which structural unemployment would be continually increasing and for which Gallaway's R test is completely inadequate.

One simple measure of the distribution of unemployment rates at a point in time is the standard deviation[3] of the sectoral unemployment rates for a given year. Standard deviation for different years can be related then to the general unemployment rate. If the relationship between the standard deviation of sectoral unemployment rates and the total unemployment rate remains the same for two different periods of time, then it can be claimed that the distribution of the sectoral unemployment rates has not changed. If, on the other

[2] Substituting the values of the example into the definition of R (as given in footnote 1), one obtains the same values of σ^2, $\hat{\sigma}_0^2$, and $\hat{\sigma}_1^2$ for both periods of time, and thus the same values of R. The same result is obtained whether the absolute values of Table 1 or the first differences are used for the calculations of R values, as long as in the latter calculations no attempt is made to use the first difference linking the two periods. Gallaway uses first differences "to minimize the impact of serial correlation on the R value" [1, p. 705].

[3] In a first draft of this note the variance of the sectoral unemployment rates rather than the standard deviation was suggested. From the purely empirical point of view of regressing the two measures against the total unemployment rate both seem equally satisfactory. Gallaway has kindly pointed out that the standard deviation makes better theoretical sense on certain assumptions.

hand, the relationship changes so that the standard deviation for any given level of general unemployment increases, then it can be claimed that unemployment due to structural changes in the economy has increased.

Table 2 summarizes the suggested calculations for the hypothetical example just discussed. It is clear that the relationship between the standard deviation and the total unemployment rate has changed from Period I to Period II. The standard deviation at any given unemployment rate has increased, reflecting the increase in structural unemployment built into the example.

The standard deviation at any point of time is defined by the following expression:

$$S.D._t = \sqrt{\sum_{i=1}^{k} \alpha_i (u_i - \bar{u})^2},$$

where u_i is the sectoral unemployment rate, $\bar{u}$ the total unemployment rate, α_i the weight of the sector in terms of the size of the labor force, and k the number of sectors.

Table 3 summarizes the results of the suggested method using the same data which were employed by Gallaway to compute the R statistic for the occupational classification [1, p. 707].

Least-square lines were fitted to the pre-1957 period and the 1957-60 period with the following result:

$$\text{pre-1957} \qquad S.D._t = -.182 + .503\bar{u}$$

$$\text{1957–60} \qquad S.D._t = -.052 + .506\bar{u}$$

It is clear that there has occurred a small upward shift in the relationship, suggesting an increase in structural unemployment—a result that contradicts Gallaway's findings based on his computation of the R statistic. Unfortunate-

TABLE 2—STANDARD DEVIATION IN SECTORAL UNEMPLOYMENT RATES

	Period I			Period II		
	1	2	3	1	2	3
Standard Deviation	.408	.817	1.225	1.225	1.633	2.041
Total Unemployment Rate	1.5	3	4.5	1.5	3	4.5

TABLE 3—STANDARD DEVIATION IN SECTORAL UNEMPLOYMENT RATES FOR OCCUPATIONAL CLASSIFICATION, 1948–60

	1948	1949	1950	1951	1952	1953	1954
$S.D._t$	1.48	2.83	2.45	1.22	1.25	1.27	2.47
Total Unemployment Rate	3.5	5.9	5.6	3.2	2.9	2.7	5.3
	1955	1956	1957	1958	1959	1960	
$S.D._t$	2.32	1.88	2.04	3.45	2.56	2.57	
Total Unemployment Rate	4.4	3.9	4.0	6.8	5.3	5.3	

ly, the difference between the two least-square fits is not statistically significant, as could be expected in a case where one of the lines is fitted on the basis of four observations.[4] Nevertheless, Gallaway's strong conclusion that the "structural unemployment explanation for the recent increase in unemployment is not valid" [1, p. 714] does not appear warranted.

The search for a measure which avoids making "some judgments as to what constitutes a meaningful number of sectors in which the time factor is statistically significant" [1, p. 711] is certainly commendable, but in the process one can lose track of some detail that is relevant and that is highlighted in studying the individual sectors separately, as done by the Knowles and Kalacheck study mentioned earlier [2].

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Structural Unemployment: Reply

It should be obvious to the reader of Vladimir Stoikov's comment that there has been an extensive interchange between him and myself through the medium of the editor's office. Nevertheless, differences remain, and so we must now "have at it" with literary broad swords. First, Stoikov challenges the effectiveness of my test for structural unemployment, arguing by counterexample. His argument is valid except for the fact that the two periods in my analysis are linked in that first differences are used throughout (the first observation for the 1957-60 period is the change in unemployment between 1956 and 1957). Thus, any shift in the structure of unemployment after 1956 would be detected by my test. In all fairness though, a counterexample can be constructed which is insensitive to my test and sensitive to Stoikov's, even though it involves a slightly different concept of structural change than that for which I tested. Consider the following example:

	Period I					Period II			
Sector	1	2	3	4	5	6	7	8	9
X	1.0	2.0	3.0	2.0	1.0	2.0	3.0	2.0	1.0
Y	1.5	3.0	4.5	3.0	1.5	3.0	4.5	3.0	1.5
Z	2.0	5.0	8.0	6.0	4.0	7.0	10.0	8.0	6.0
Total	1.5	3.33	5.17	3.67	2.17	4.0	5.83	4.33	2.83

[4] The standard error of estimate for the pre-1957 period fit is .67 and for the 1957-60 period fit .73.

This example shows an upward drift in the aggregate unemployment rate as the result of an accumulation of unemployment in Sector Z. Despite this, the *R* values for the two periods are identical (even with the use of first differences). The increase in sturctural unemployment shown in this example is not something unique to Period II but exists in both Periods I and II, i.e., the structural unemployment has been accumulating throughout both periods. Now, this is a possible interpretation of the structural change hypothesis. Consequently, Stoikov's test merits consideration.

As to the results of Stoikov's test, I must confess that I am puzzled and bewildered by his interpretation of his statistical data. From his regressions, he concludes that there has been a small upward shift in the 1957-60 period in the relationship between the standard deviation of the distribution of sector unemployment rates and the over-all unemployment rate. I trust he refers to the change in the intercept term of his regressions (from —.182 to —.052), for the change in the parameter of his independent variable is almost miniscule. What is puzzling is that the intercept terms in his regressions do not and should not differ significantly from zero (if they did, negative sectoral unemployment rates would be implied).[1] Thus, his final conclusion that my generalization is not warranted is based on an insignificant change in a term which does not and should not differ significantly from zero. If this term is disregarded (as it should be), Stoikov's results lend striking support to my conclusion with respect to the structural change hypothesis, rather than weakening it. Enough said.

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Diminishing Returns and Linear Homogeneity: Comment

Professor G. Warren Nutter [5, p. 753] prefers Dr. Dieter Schneider's functions (1) and (2) [8, p. 747] to Professor Ryuzo Sato's (1) [7, p. 744] and my (3) [6, p. 746]. He bases his preference on the Knightian qualities of Schneider's functions as opposed to the textbook-like qualities [4, p. 124], [9, p. 115] of Sato's and mine. While I still prefer Sato's (1) and my (3) because they yield positive output for all positive input combinations, the purpose of this comment is not to discuss what sort of production function best approximates reality. The first purpose of this comment is to examine Schneider's functions to see under what circumstances they are good representatives of the Knightian production function [3, p. 100]. The second is to develop equations representing the semi-Knightian production functions of Sune Carlson [1, pp. 18, 22] and John M. Cassels [2, p. 109].

Schneider specifies the properties of a Knightian production function as:
1. The law of diminishing returns is represented by a three-dimensional surface that is symmetrical with respect to two inputs [8, p. 747].

[1] Presumably a zero aggregate unemployment rate implies zero rates for all sectors and a zero standard deviation.

2. If one input, b, is held constant, and the other input, a, varied, marginal returns to a at first increase and eventually diminish [8, p. 747].
3. Output, x, is negative for $a=0$, and becomes positive for $a>q$, where q is some positive value of a [8, p. 748].

Apparently Schneider means his functions to be symmetrical in the nonrigorous sense. If a is held constant, the marginal product curve for b is *similar* to the marginal product curve derived from holding b constant and varying a. However, a special case of symmetry in the nonrigorous sense is symmetry in the rigorous, or mirror-image, sense. A function which is symmetrical in the general sense should be able to accomplish its objectives if parameters are picked to make it symmetrical in the rigorous sense. If Schneider's equations (1) and (2) [8, p. 747] are symmetrical in the rigorous sense, $A=D$, $B=C$, and $E=F$. These equations are then:

$$(1)\qquad x = \{(a+b)[-Aa^2+(A+B)ab-Ab^2]\}^{1/3}$$

$$(2)\qquad x = \frac{(a+b)[-Aa^2+(A+B)ab-Ab^2]}{E(a^2+b^2)}\,.$$

The first and second partial derivatives of x with respect to a for (1) and (2) are:

$$(1)'\qquad \partial x/\partial a = 1/3\{(a+b)[-Aa^2+(A+B)ab-Ab^2]\}^{-2/3}(-3Aa^2+2Bab+Bb^2)$$

$$(1)''\qquad \partial^2 x/\partial a^2 = 2/9\{(a+b)[-Aa^2+(A+B)ab-Ab^2]\}^{-5/3}(B+3A)\cdot[-Ba^2b^2+(3A-B)ab^3-Bb^4]$$

$$(2)'\qquad \partial x/\partial a = \frac{-Aa^4-(B+3A)a^2b^2+2(A+B)ab^3+Bb^4}{E(a^2+b^2)^2}$$

$$(2)''\qquad \partial^2 x/\partial a^2 = \frac{2(A+B)b^2(a+b)(a^2-4ab+b^2)}{E(a^2+b^2)^3}\,.$$

Setting $b=1$, we see that:

1. For equations (1) and (2) $x>0$ if:

$$1/2\{1+B/A-\sqrt{(B/A+3)(B/A-1)}\} < a < 1/2\{1+B/A+\sqrt{(B/A+3)(B/A-1)}\}.$$

2. For equation (1)″ $\partial^2 x/\partial a^2>0$ if:

$$1/2\{3A/B-1-A/B\sqrt{3(B/A+3)(1-B/A)}\} < a < 1/2\{3A/B-1+A/B\sqrt{3(B/A+3)(1-B/A)}\}.$$

3. For equation (2)″ $\partial^2 x/\partial a^2>0$ if:

$$a < .28 \quad \text{and} \quad a > 3.73.$$

For equation (1) and its derivatives we see that the B/A ratio must be greater than one if output is to be positive at all, and that if the marginal

product curve of a is to have an upward-sloping range for increases in a, the B/A ratio must be less than one. Since B/A cannot be both greater than and less than one, Schneider's (symmetrical) (1) can not generate a rising marginal product curve within the positive output range. It must, therefore, be rejected as an example of the Knightian law of diminishing returns.

For equation (2) and its derivatives we see that the B/A ratio must also be greater than one if output is to be positive at all, and that the values of a at the highest and lowest points of the marginal product curve are independent of the B/A ratio. For the maximum and minimum points of the marginal product curve to fall within the range of positive output it is necessary that B/A be greater than three. Otherwise the marginal product curve falls throughout the positive output range. Only if $B/A>3$ is Schneider's (2) a good example of the Knightian law of diminishing returns.

Schneider's Knightian function (with $B/A>3$) and Sato's and my textbook function both have regions within which the marginal product of a increases with increases in a, but they differ in two respects. Output is zero (presumably this is what Schneider means when he says output is negative) for $a \leq q$ in the Knightian function; whereas output is zero for either input equal to zero and is positive for all positive combinations of inputs in the textbook function. The marginal product of a is positive at the boundary of the positive output range ($a=q$) in the Knightian function; whereas the marginal product of a is zero at the boundary of the positive output range ($a=0$) in the textbook function.

There are two intermediate cases between the Knightian and textbook functions. The first of these has zero output if either input is equal to zero, positive output for all positive combinations of inputs, and the marginal product of a is positive for $a=0$. The second has a region of zero-output for positive inputs, and the marginal product of a is zero for $a=q$. The first of these intermediate functions is the production function constructed graphically by Sune Carlson [1, pp. 18, 22], and the second is that constructed by John M. Cassels [2, p. 109].

To obtain an equation representing the (linear homogeneous) production function of Sune Carlson, set $A=0$ in Schneider's (symmetrical) (2). Then:

$$(3) \qquad x = \frac{Bab(a+b)}{E(a^2+b^2)}.$$

The first and second partial derivatives of x with respect to a are:

$$(3)' \qquad \partial x/\partial a = \frac{Bb^2(-a^2+2ab+b^2)}{E(a^2+b^2)^2}$$

$$(3)'' \qquad \partial^2 x/\partial a^2 = \frac{2Bb^2(a+b)(a^2+4ab+b^2)}{E(a^2+b^2)^3}.$$

Setting $b=1$, we see that:

1. $x>0$ for $a>0$.

. $\partial x/\partial a > 0$ for $a < 2.42$.
. $\partial^2 x/\partial a^2 > 0$ for $a < .28$ and $a > 3.73$.
. $\partial x/\partial a = B/E$ for $a = 0$.

So (3) yields the desired Carlsonian results. Output is zero if either input quals zero, output is positive for all positive input combinations, the marinal product curve of a has a rising range for increases in a, and the margiıal product of a is positive for $a=0$.

To obtain an equation representing the (linear homogeneous) production unction of John M. Cassels, my (3) [6, p. 746] may be slightly altered to ead:

$$x = \frac{(a - gb)^{3/2}(b - ga)^{3/2}}{.5(a - gb)^2 + .5(b - ga)^2} \tag{4}$$

where g is the a/b ratio (and the b/a ratio) below which output is zero, $(1 > g > 0)$. The first partial derivative of x with respect to a is:

$$\partial x/\partial a = 1/4(a-gb)^{1/2}(b-ga)^{1/2} \cdot \left\{ \frac{-3g(a-gb)^3 - (a-gb)^2(b-ga) + g(a-gb)(b-ga)^2 + 3(b-ga)^3}{[.5(a-gb)^2 + .5(b-ga)^2]^2} \right\}. \tag{4'}$$

Setting $b=1$, we see that $x=0$ (assuming negative or undefined output means zero output) for $a \leq g$ and $a \geq 1/g$, the marginal product of a is zero for $a=g$ and $a=1/g$, and (setting $g=.2$) the marginal product of a is increasing with increases in a for $a < .47$ and $a > 2.1$. Thus, equation (4) illustrates the Casselsian production function.

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* The author, assistant professor of economics at the University of Oklahoma, wishes to thank Professor James Hibdon, Department of Economics, University of Oklahoma, for assistance in preparing this comment.

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Diminishing Returns and Linear Homogeneity: Comment

In this note we attempt to resolve the controversy on the existence of diminishing marginal products in the case of a linear homogeneous production function.

Let a production function $f(x_1, \cdots, x_n)$, where the x's denote inputs, be linear homogeneous. If one keeps all x's but x_1 constant and graphs f in terms of the variable input x_1 (total product curve), then an inflection point in the graph indicates that $\partial^2 f/\partial x_1^2$ passes through zero, and hence that $\partial^2 f/\partial x_1^2$ has both a positive and a negative range, i.e., f allows both increasing and decreasing marginal products $\partial f/\partial x_1$.

Professors Stonier and Hague [12, p. 229] state that if f is homogeneous, $\partial f/\partial x_1$ is always decreasing (no inflection point); and Professor Liebhafsky [7, p. 126n.] adds that a search of the literature has failed to reveal a specific linear homogeneous production function that justifies such an inflection point. Professor Nutter [11] thereupon did specify such a function and thereby provoked a controversy in this journal (September 1964, pp. 739–53) which, in my opinion, has failed to clarify the issue.

The controversy can be resolved by the simple lemma below, which states that, for a homogeneous production function, *marginal products are nonincreasing or nondecreasing according as the function is concave or convex.* The graph (with x_1 in abscissa) of a function $f(x_1)$ that is concave (convex) in x_1 is concave from below (above).

For simplicity we denote by f_i the first partial $\partial f/\partial x_i$; by f_{ij} the second partial $\partial^2 f/\partial x_i \partial x_j$; by f_x the gradient vector $[\partial f/\partial x_i, \cdots, \partial f/\partial x_n]$ evaluated at x; and by $f_x x$ the inner product

$$\sum_{i=1}^{n} (\partial f/\partial x_i) x_i,$$

so that Euler's theorem for positively homogeneous functions becomes $f(x) = f_x x$ (x is the n-tuple $[x_1, \cdots, x_n]$). R^n denotes the space of real n-tuples; R, the real line. Consider the n-tuples x, y ($x \neq y$) and let $\Delta x \equiv y - x$, $\Delta f_x \equiv f_y - f_x$.

Lemma: Let $f\colon R^n \to R$ denote a single-valued function that is positively homogeneous of degree one. If f is concave (convex) and f_x, f_y exist, then necessarily

$$\Delta f_x \Delta x \leq 0 \quad (\geq 0). \tag{1}$$

Proof: We refer to a concave function (as the convex case is entirely parallel). It is well known that, for f concave,

$$f(y) - f(x) \leq f_x(y - x) \tag{2}$$

and, alternatively,

$$f(x) - f(y) \leq f_y(x - y). \tag{3}$$

By Euler's theorem these formulas may be written

(2′) $$f_y y \leq f_x y$$

(3′) $$-f_y x \leq -f_x x.$$

Adding: $f_y(y-x) \leq f_x(y-x)$, i.e., $(f_y - f_x)(y-x) \leq 0$, which is equivalent to (1). QED.

Homogeneous functions are a subclass of the positively homogeneous functions. Hence, in the case of a linear homogeneous production function, increasing only the ith input ($\Delta x_i > 0$; $\Delta x_j = 0$ for all $j \neq i$) yields, by (1),

(4) $$\Delta f_i \Delta x_i \leq 0 \quad (\geq 0)$$

for f concave (convex). It follows that the marginal product f_i of the ith input is nonincreasing or nondecreasing according as f is concave or convex.[1]

The aforementioned Stonier and Hague statement is thus incorrect. The Liebhafsky statement is justified because the maximands economic literature has been concerned with are almost exclusively of the concave type as they are nearly always required (a) to represent an increasing rate of technical substitution (along an isoquant the expression $-dx_2/dx_1$ increases with x_1); (b) to satisfy the second-order conditions for a maximum:

$$d^2 f = \sum_{i,j} f_{ij} dx_i dx_j < 0,$$

a condition implying concavity. Concave maximands further relate to (c) convex preference preorderings (cf. Koopmans and Bausch [5 p. 90]); (d) nonincreasing average returns under monopoly (cf. Dorfman, Samuelson, and Solow [3, pp. 186ff.]); (e) risk aversion (cf. Arrow [1, pp. 41–48], Debreu [2, p. 101], and Moeseke [9]); and (f) the body of literature on constrained maximization known as convex programming (concave maximands or convex minimands defined on convex constraint sets, cf. Kuhn and Tucker [6]).

Nevertheless, convex linear homogeneous production functions can occur in economics. For instance, to deal with situations of risk, where the maximand $F(x)$ is random, I have introduced in [10] the *truncated minimax-maximax* decision rule $\max \phi(x) \equiv EF(x) + m\,\sigma F(x)$ that attaches the relative weights 1 and m to the expectation EF and the standard deviation $\sigma F(x)$ of $F(x)$. It is shown in [9] that, for F linear, ϕ is linear homogeneous and that it is concave or convex according as $m \leq 0$ (risk-averting attitude) or $m \geq 0$ (risk-seeking attitude). By the lemma the marginal product of the variable input will be nondecreasing in the eyes of a gambler. (The reader is also referred to the construction by Friedman and Savage [4], and by Markowitz [8], of a utility function for money whose graph contains both concave and convex segments.)

The lemma indicates that, in order to exhibit a homogeneous function with an inflection point in its graph, it will be necessary to specify a function whose graph has *both* a concave and a convex segment. This is precisely

[1] *Strict* concavity would allow the strict inequality in (2) and hence in (4). However, a homogeneous function is *never* strictly concave because it is linear on the rays. I am indebted to Professor Koopmans for pointing this out to me.

Nutter's idea: he adduces a numerical example where one Cobb-Douglas function is subtracted from another.

The simplest representative of this class of function is

$$(5) \qquad f(x_1, x_2) = x_1^{\alpha} x_2^{1-\alpha} - x_1^{\beta} x_2^{1-\beta}, \qquad (0 < \alpha, \beta < 1; x_1, x_2 \geq 0).$$

The graph of $f(x_1)$ for $x_2 = $ constant, say $x_2 = 1$, is concave or convex (concave from below, respectively from above) according as

$$(6) \qquad f_{11} = \alpha(\alpha - 1)x_1^{\alpha-2} - \beta(\beta - 1)x_1^{\beta-2}$$

is negative or positive; i.e., according as the first or the second term on the right in (5) dominates in f_{11}. The graph exhibits an inflection point for $f_{11} = 0$. By (6)

$$(6) \qquad f_{11} \gtreqless 0 \quad \text{if} \quad \frac{\alpha}{\beta}\,\frac{1-\alpha}{1-\beta}\,x_1^{\alpha-\beta} \lesseqgtr 1.$$

Let, for instance, $\alpha = \frac{1}{2}$, $\beta = \frac{1}{4}$. By (6), for $x_2 = 1$, the graph of $f = f(x_1)$ is concave, convex, or inflected according as $x_1 \lesseqgtr 3^4/4^4 = .316\ 40625$. Indeed, one computes readily from (5) that, for $x_1 = 1$, say, $f_{11} = -1/16 < 0$; for $x_1 = 3^4/4^4$, $f_{11} = 0$; for $x_1 = \frac{1}{4}$, say, $f_{11} = (3/\sqrt{2}) - 2 > 0$.

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Diminishing Returns and Linear Homogeneity: Reply

Professor Rowe has shown that my equation (5) and Schneider's equation (1) do not reproduce the Knightian properties. This is easily seen if we suppose that a cubic function of the variable input yields the sequence 1, 8, 27 for the sequence 1, 2, 3 for the variable input. The cube root of the function would then simply yield the sequence 1, 2, 3, so that what were increasing marginal returns in the cubic function would become constant marginal returns.

At the same time, Schneider's equation (2) and my own equation (6) remain valid, with additional restrictions such as those outlined by Rowe.

As to Professor Moeseke's note, I find nothing new but the form of logic. His justification of Liebhafsky's statement is, however, odd. The only reason Liebhafsky asks for a specific algebraic function is that a convex region (as defined by Moeseke) has always been included in the normal production function, at least since Knight put it there.

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Decreasing Opportunity Cost: An Amendment

I wish to make an amendment to my communication "Factor Marginal Products and Decreasing Opportunity Cost," which appeared in the March number of this journal.[1] This will, I hope, anticipate some objections, and at the same time strengthen the principal claim made there.

I asserted that, with at least three factors of production, the production-possibility curve could have parts that were concave in the upward direction. This was true despite the assumption of constant returns to scale. The point was illustrated with a pair of production functions that differed in their complementarity relations among the three factors.

What I failed to see then was that the production-possibility curve not only

[1] This note was prompted by a discussion with Professor J. R. Hicks, who read the earlier paper. He argued that the Bishop-Samuelson reasoning ought to apply to any pair of points on the transformation curve. I have not been able to show him a draft of this note, and he may well still disagree with its contents. I also wish to acknowledge helpful comments on the earlier paper by J. G. Witte, R. Carson, E. W. Martin, and H. J. Heinemann. They, too, are not responsible for any shortcomings, either there or here.

could have dips, but could pass below the diagonal connecting the end points of the curve. Indeed it could be concave over the whole stretch. The Bishop-Samuelson argument, while applicable in the two-factor case, is inoperative when there are at least three factors.

This bold assertion depends on the distinction between the firm and the economy, and on the important assumption that the firms in this two-sector economy each produce only one product. They are engaged either in manufacturing or in agriculture but not in both. Thus the production-possibility curve for the economy is in no sense a replica or average of those for firms. If, indeed, the economy were operated by one big firm, or were steered by a price-control official who could distcriminate perfectly in setting factor prices it could always move in a convex way. The Bishop-Samuelson argument would always apply, no matter how many factors or what technical relations existed among them. But given that our atomistic, single-product firms do all face the same factor prices, the market mechanism can perfectly well steer them through a concave path of production.

The foregoing will recall to mind the importance of fulfilling *all* the marginal conditions in order to reach a Pareto optimum. One of them, that the marginal rate of transformation between any two products be the same for all firms, is inoperative when firms don't produce both manufactures and agricultural products.

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BOOK REVIEWS

General Economics: Methodology

Economics of the Kennedy Years—And a Look Ahead. By SEYMOUR E. HARRIS. New York: Harper & Rowe, 1964. Pp. xii, 273. $5.95.

The economics of the Kennedy years will probably be discussed by economists and political scientists for a long time to come. There are those whose views are characterized by a statement by Oscar Gass that President Kennedy's economic policies "projected little and accomplished nothing" (quoted by Seymour Harris, p. 218). There are others who believe that the economics of the Kennedy years marked a breakthrough for the acceptance of fiscal policy, not only as an anticyclical weapon but as a tool to support sustained economic growth and growing welfare. In their view fiscal policy will never be the same again. Harris' book is certainly not the last word on this controversy, but on such a topic a timely—even if not definitive—study is highly welcome if it is informative. And informative Harris' book certainly is.

The author deals with the topic on three different planes. First is the story of "the conversion" of John F. Kennedy from the Massachusetts congressman with somewhat provincial views on economics to the President, who, as Harris says, surpassed any public official, and certainly any President, "in his vigorous support and dissemination of modern economics" (page 56). The personal observations of Harris (an occasional adviser to John Kennedy for more than ten years) contribute to an understanding of the manner in which Kennedy not merely adopted some of the jargon but also absorbed the philosophy of the "new economics."

On a second plane, Harris recounts and evaluates the economic and fiscal policies of the Kennedy Administration, both domestic and international. He gives full credit to Walter Heller as chief architect of the fiscal policy recommendations. Interesting are his observations about the respective positions in regard to monetary expansion of the Council of Economic Advisers, the Treasury, and the Federal Reserve System. He summarizes his impression by the following statement: "The Treasury generally was not as expansionist as the Council but more disposed to have additional credit than the Federal Reserve" (p. 110). Harris does underemphasize, I believe, the importance of the increase in spending, largely for defense, space, and social insurance. The rate of federal spending (consolidated cash account) actually increased by $26 billion from fiscal year 1960 to 1964.

He believes the tax-cut proposal is the most significant aspect of the fiscal policy of the Kennedy Administration. It is significant because it marked a conflict between President Kennedy's political stance and his conversion to the "new economics." Furthermore, it was an experiment with fiscal policy on a large scale (even considering the slowdown in expenditure increases ultimately associated with the proposal) which permits as much of a laboratory test as we can have in economics. Finally, the tax cut was not only "new" but "newer" economics. Fiscal policy was used not as an anticyclical device de-

signed to counteract an actual or threatening recession, but to bolster and prolong a recovery. The tax-cut recommendation was based on the theoretical concepts of "fiscal drag" and the "full employment budget surplus."

Harris presents the theoretical argument, its translation into policy recommendation, and the concessions which were made in order to make the recommendation acceptable to key congressmen and groups believed needed for political support. Harris indicates that President Johnson has gone further than Kennedy in concessions to expenditure restraint and possibly further than was required or desirable (pp. 235ff.).

Harris does not claim that the Kennedy Administration had achieved its economic objectives. He expresses dissatisfaction particularly with the consistently high level of unemployment. He is cautious in evaluating the success of the tax cut. I suspect that Harris today, ten months after the completion of his book, would be more certain about his tentative evaluation of its results. But even today, it is still too early for a definitive evaluation.

On a third plane, Harris spiritedly defends the economic and fiscal policies of the Kennedy Administration against critics and detractors from conservative and liberal camps. He is obviously pained by the bitter criticism from liberals like Leon Keyserling and Oscar Gass.

I find this a useful and informative, even if not a profound, book. In a few places speed has resulted in minor errors. For example, Harris states that the ministerial agreement of the OECD countries set a collective target of 4.4 per cent for the annual rate of growth. Actually, the computation amounts to 4.1 per cent (see *Economic Report of the President*, January 1962, p. 114). There is some inconsistency between a very eloquent statement about the benefits of an adequate rate of growth in the United States (p. 177) and the statement that the President in 1960 (*sic*!) had been "oversold" on growth (p. 192).

In general, one can only admire Seymour Harris for being able to write and publish such an informative book in the short time since the end of the period which he covers.

I find little justification in the book for the last part of the title.

GERHARD COLM

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Systematic Glossary of Selected Economic and Social Terms—English/French/Spanish/Russian. Vol. 1. Compiled by ISAAC PAENSON. New York: The Macmillan Co., 1963. Pp. xxxv, 414. $30.

This volume, the first of a series on four related subjects, is a valuable addition to the tool kit of the researcher with active interest in the international economy. With the increasing pressures for effective dissemination of the growing pool of socioeconomic knowledge, the professionals should welcome this systematic four-language effort. The compiler, drawing on 12 years of practical experience as a translator in the United Nations, has methodically included over 1,500 significant economic and sociological terms in the context in which they are usually encountered by the social scientist. He correctly notes that "only in considering the ideas with which a given term is usually

associated can we hope to discover the full range of its meanings . . ." (p. xi).

Rather than being ordered alphabetically, the terms are logically presented in eight chapters, which are broken down according to subject matter—i.e., demand, production, business economics, labor and social security questions, financial questions, economic theories, and international trade. Interestingly, this rather unique ordering of material by topic has, in Paenson's words, "facilitated the parallel presentation of Marxist and non-Marxist terms in those numerous and important cases where deep political, economic and social differences . . . are also reflected in their respective terminologies" (p. xi).

Any glossarist is plagued with the near-overwhelming problem of selection, and the end product necessarily is abandoned, not completed (as, of course, is true in all research activity). Especially in this instance, owing to the ambitious nature of a broad social science approach involved, there will obviously be those who detect what are felt to be serious omissions in the text. Nonetheless, reference material of this type is badly needed, and a good beginning has been made here in Paenson's endeavors to contribute constructively to improving international communications. It is gratifying to note that the text is securely bound in a solid loose leaf binder expressly for the purpose of making periodic page insertions available in the future.

In sum, the volume will be quite useful as a general reference guide designed to insure maximum precision in the difficult area of transferring certain technical ideas. Although the reviewer has been involved in working only with the English and Spanish languages, one could reasonably expect that comparable workmanship is present in the French and Russian entries. It would seem that this Glossary (though expensive) should be readily available for all internationally-oriented researchers in the social science disciplines.

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Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

A Contribution to Demand Analysis. By I. F. Pearce. New York and Oxford: Oxford University Press, 1964. Pp. 258. 42s; $6.75.

To my knowledge no book giving a reasonably complete statement of modern demand theory has been published since 1953. In the ensuing 12 years a great deal of work has been done on the subject. Professor Pearce's book does not summarize all this work, nor does it provide a *complete* statement of modern demand theory. What it does do is bring to hand and unify several earlier papers by Pearce, all of which deal with problems currently receiving attention in the literature. Thus the book is timely and serves well as a basis for advanced courses in demand theory, one of its stated purposes. The primary theme of the work is the development and application of the concept of neutral want association. Neutral want association is closely akin to the various notions of separable utility developed by

by Sono, Leontief, Strotz, Gorman, Frisch, and Houthakker [1]. It is roughly that there exist "marked gaps" such that the consumer's preferences may be represented by a function of several functions in the following manner:

$$u = u(\Phi_1, \Phi_2, \cdots, \Phi_n)$$

where

$$\Phi_1 = \Phi_1(x_1, \cdots, x_r),$$
$$\Phi_2 = \Phi_2(x_{r+1}, \cdots, x_{r+s}),$$
$$\Phi_3 = \Phi_3(x_{r+s+1}, \cdots, x_{r+t}), \text{ etc.}$$

The relationships between the various concepts of separability have recently been discussed by Goldman and Uzawa [1], and accordingly we will not discuss them here. In my opinion the idea of separable utility (neutral want association) is the most promising development in modern demand theory, and Pearce, by making this the unifying theme of his work, has made a significant contribution.

This is not to say that the book is without fault. It contains many stylistic and methodological idiosyncrasies which are, at best, disconcerting. Pearce drives his mathematics as far as it will go, and when it will go no further, he gets out and walks. This has many consequences, including such statements as "most of the more obvious conclusions of economic theory are based upon nothing more subtle than the belief that demand curves slope downward to the right" (p. 87), and "the sign of the income effect $-x_i(\partial x_j/\partial y)$ is always negative, so that $\partial x_j/\partial p_i$ could be negative even if σ_{ji}/Δ were positive" (p. 138). Unless Pearce is aware of some extremely important theorems which he neither states nor cites, such comments (which are not rare in the book) would seem to have no justification in a precise work on demand theory. The reader is also treated at several points to rather lengthy rhetorical arguments in support of what are, in fact, assumptions. Now the entire point of Pearce's demand theory is to provide a set of empirical results, the testing of which will provide a sufficient examination of the effective realism of his assumptions. Since the results of such tests will presumably be independent of our feelings regarding the assumptions, any rhetoric on their behalf would seem misplaced. In spite of this sort of thing, there is no doubt that Pearce is aware of what he is doing, but he is not as careful as he might be in imparting this awareness to the reader. The result is that the reader must be careful for himself. In order to illustrate this, it is necessary to go into some part of the book fairly extensively. Accordingly we will give disproportionate attention to Pearce's theory of saving.

Pearce is either unaware of a great deal of existent work on the subject of consumer's saving and the demand for assets or he is aware of criticism of this work which he does not choose to disclose. He writes regarding saving and the demand for assets, "a precise theory is necessary to an understanding of our aims, and . . . no such theory is possible except on

the lines outlined above" (p. 43). Since both the Patinkin approach to consumer saving and the Bushaw and Clower approach to stock-flow analysis appear to be significantly different from the approach he outlines, this looks to be a strong statement. Pearce assumes that the consumer maximizes

$$u = u(x_1, \cdots, x_n, q_1, \cdots, q_n, r, s, w)$$

subject to

$$y = s + \sum p_i x_i,$$

where x_i is the quantity of commodity i, y is the current income, q_i is the expected future price of commodity i, r is the rate of interest, s is current saving, and w is accumulated wealth. Notice that neither wealth nor possible interest payments accruing to it enter the consumer's budget restraint, nor do they appear to enter the analysis anywhere, except as two undisturbed letters in the consumer's utility function. It is further assumed that future consumption is neutrally want associated with current consumption so that,

$$u_{iq_i} = \alpha u_i = \beta p_i$$
$$u_{jq_i} = \alpha u_j = \beta p_j$$
$$u_{is} = \alpha' u_i = \beta' p_i$$
$$u_{js} = \alpha' u_j = \beta' p_j,$$

where $u_{iq_i} = \partial u_i/\partial q_i$ and $u_{is} = \partial u_i/\partial s$.

In his "Readers Guide" (appropriately called Chapter 0) Pearce claims that "the theory of demand for consumer durables fits naturally into the whole" (p. 14) of his theory of savings. It seems doubtful that his results would in fact follow if assets other than money were explicitly considered. Under these conditions wealth holdings would include such assets, and $\partial w/\partial p_i = 0$ would not be true. This would introduce n additional terms into Pearce's counterpart of the Slutsky equation, and it is not at all clear that the assumption of neutral want association would enable one to derive testable results. The results which Pearce does derive are summarized in two equations.

$$\frac{\partial x_j}{\partial p_i} = -\left(x_i + \frac{1}{1-h}\frac{\partial s}{\partial q_i}\right)\frac{\partial x_j}{\partial y} + [\sigma_{ji}]_s, \quad \text{and}$$

$$\frac{\partial x_j}{\partial p_i} = -\frac{1}{1-h}\left(x_i + \frac{\partial s}{\partial p_i}\right)\frac{\partial x_i}{\partial y} + \sigma_{ji},$$

where both $[\sigma_{ji}]_s$ and σ_{ji} satisfy all the symmetry and sign conditions established for the traditional substitution term, and $h = \partial s/\partial y$. Pearce claims that he is able to attach a sign to the coefficient of $\partial x_i/\partial y$ in the first of these equations. This depends upon the following argument, "all u_{iq_i} must have the same sign, which will be positive; for increasing q_i reduces real well-being which increases marginal utilities." Notice that by this argument one could show that in the traditional Hicksian analysis *all* u_{ij} *must*

be negative, which result is absurd. The sign of the coefficient of $\partial x_i/\partial y$ is precisely as well established as the sign of u_{ij} in Hicks's analysis, which is to say, not at all. Notice, however, that the second equation above may be rewritten

$$\frac{\partial x_j}{\partial p_i} + \frac{1}{1-h}\left(x_i + \frac{\partial s}{\partial p_i}\right)\frac{\partial x_i}{\partial y} = \sigma_{ji},$$

which has all the essential properties of the Slutsky equation. Demand theory implies numerous restrictions on the term on the right-hand side, and *every term on the left-hand side is observable.* Thus the theoretical properties of σ_{ji} may be subjected to empirical test.

The discussion of the notion of neutral want association is lengthy and not terribly clear to me. Pearce has tried to render this topic more intuitively appealing—he has succeeded in rendering it more opaque. There are many areas of economics which have developed beyond our oral tradition, and consumer demand theory is surely one of them. The more unashamedly mathematical developments of the notion of separability seem to me clearer than that offered by Pearce. He shows that under neutral want association the price effect may be written as a function of income effects only. He also develops a straightforward empirical test for the existence of neutral want association between commodities. Finally he shows conditions under which we may deal with composite commodities (e.g., food, amusement, etc.) as if they were individual commodities having relationships analogous to neutral want association between them. Separable utility theory is, I think, an extremely important area for further work, and Pearce's contribution to it is valuable.

There are many other topics discussed in the book. Pearce attempts to establish weak conditions under which we can apply the results of individual consumer demand theory to community demand data. He also develops a mathematical definition of complementarity which better conforms to the notion of "being used together" than does Hicks's definition. Using the arguments of his section on community demand theory, Pearce closes the book with three attempts to test empirically the notion of neutral want association on aggregate data. These attempts are not very successful, and Pearce does not claim much for them. The point really is this: Pearce has here collected and unified several of his earlier papers relating to demand theory. Neither his empirical work nor his theoretical work should be considered the final word, nor is it so intended. The book is only what it claims to be, a contribution to demand analysis. In my opinion, it will prove a valuable contribution to all those demand theorists possessed of patience, pencil, and paper.

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REFERENCES

1. S. M. GOLDMAN AND H. UZAWA, "A Note on Separability in Demand Analysis," *Econometrica*, July 1964, *32*, 387–98. The other articles on separable utility mentioned in the review are cited here on page 398.

The Theory of Inflation: A Review. By A. J. HAGGER. Victoria: Melbourne University Press; London and New York: Cambridge University Press, 1964. Pp. viii, 238. $9.50.

The purpose of this compendious book is to show the kinds of conceptual and theoretical gaps that need to be filled in order to meet the common complaint that inflation theory "is not moving in any definite direction" (p. vii). The book is not intended "to break new ground." The major portion of the study is devoted to the exposition, and necessarily rather brief though pointed criticisms, of a very large number of inflation models constructed during the past twenty years. The final chapter, likewise aimed primarily at filling gaps and remedying specific defects, is entitled "Some Suggestions for Further Work." Thus the author is calling for further investigation of problems encountered in models already built, and is searching out specific theoretical issues which he believes have not yet received sufficient attention. He is not undertaking a fundamental revision of the approach to the theory of inflation, nor is he attempting to evaluate the gains or losses to economic theory that may have occurred because certain writers selected this particular set of models rather than alternative ones to illustrate the mechanism of inflation.

The interested reader who is initially unacquainted with the details of the many models discussed in this book cannot expect to be spared the necessity of working through the originals for himself. But one who is already familiar with them will be struck by the author's aptitude for capsular exposition. The volume covers (1) excess demand inflation (with Bent Hansen's work occupying the center of the stage); (2) creeping inflation, latitudinously defined (p. 65) as "a rise in the general price level which is continuous but not sufficiently rapid to destroy confidence in the currency" (Chile's perennial inflation and that of the United States from 1955 to 1965 both qualify under this definition); (3) the cost-push demand-pull inflation controversy and its unhelpfulness when concerned with the "distinction between inflationary mechanisms or as a distinction between inflationary shocks" (p. 134); (4) the contribution of the monetary approach in the analysis of hyperinflation; and (5) models which stress the phenomenon of continuously rising wage rates, for example, the wage-drift theory of Hansen and Rehn as well as the work of Phillips and his followers.

Confronted with such a spate of diverse inflation models, even within the compass of particular types of inflation, the reader must feel the lack of even a rough consensus on the economic variables whose interdependence is to be tested. Much of the complexity arises out of the multitude of situations contained in the generally accepted definition of inflation as "a rise in a general price index." The wide variety of economic experience subsumed under this extraordinarily flexible definition must raise the question of the appropriateness of speaking of "the" theory of inflation. Another source of complexity is the freedom asserted by the model-builder to select a particular functional relationship out of alternatives. The consequence, when this practice is generalized to the population of model-builders, is an inordinate proliferation of alternative theories, with relatively little progress toward realistic and testable models.

A. J. Hagger's book abounds with illustrations, of which a few may be taken at random, to indicate why the construction of a new model does not necessarily mean a nearer approach to an actual description of the inflationary process. Thus, variously, planned production has been taken to be an ever-increasing function of the price-wage ratio, that is, of profits; expenditure on consumer durables has been taken to be an inverse function of the interest rate; commodity prices in general have been treated alternatively as flexible (demand-determined) or inflexible (fixed by the seller on the basis of some rule); agricultural prices have been taken as flexible and industrial prices as inflexible; money investment has been taken as entirely exogenous; the consumption function has been treated alternatively as aggregative or as applying separately to wage earners and profit receivers; excess demand has been treated as exogenous, thus ignoring indirect effects on the size of the inflationary gap when prices and wage rates rise ostensibly to close the gap; and so on. Clearly there is no necessary reason why successive models would come closer to reality with respect to these assumptions.

In view of these characteristics of the models, coupled with the fact that they are at best only slightly disaggregated, it is not surprising to learn, for example, that Bhatia is cited as finding (p. 100) that none of the hypotheses explain very satisfactorily the rate of change of average wage-earnings during the decade of the 'fifties. Hagger is likewise critical of creeping-inflation models because of the limited range of the types of shocks to which the models are subjected, and of the comparative neglect of speeds of reactions (p. 106). The author's point with respect to shocks may be illustrated with reference to the impact on the Consumer Price Index of the pattern of rises in State and local indirect tax rates since World War II. Some economists have maintained that a large proportion of the minor rise in the CPI experienced during the past few years is accounted for by this phenomenon. But it would take a considerably more detailed inflation model than any yet presented to give us an answer to this, since it is not enough simply to have a tax term in the price equation. The whole subject of the distribution of the tax increases through time and their impact on the components of the CPI would have to be examined.

There is, however, a simpler approach. The author considers three possible interpretations of the cost-demand inflation distinction (pp. 116 ff.): (1) that based on the inflationary mechanism (demand or cost determination of prices); (2) the distinction based on the type of initial shock; and (3) that based on the type of inflationary situation. Hagger regards the third distinction as the only one of the three that is useful. He seeks a test of creeping inflation that avoids taking a stand on the extent to which prices are demand- or cost-determined, and he finds the test in terms of a "situation" rather than of a shock or of a mechanism. An inflationary situation is said to be a demand inflation if, through (politically possible) demand restriction measures, the rate of increase in prices can be reduced to a desired level, say 2 per cent a year, within a prescribed period, say two years (p. 214). Otherwise it is cost inflation. Finally, it is suggested that a full econometric model be employed to predict the rate of price increase under the above assumptions. If

this predicted rate exceeds the "acceptable limit," we have a cost inflationary situation. The author acknowledges that this test has the limitations that are usual to econometric procedures. It also seems to be true that in the light of market imperfections and the complexities of cost and demand inflationary shocks, along with demand shifts, the selection of the boundary rate of price increase must be rather a difficult matter.

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The Price System. By ROBERT DORFMAN. Englewood Cliffs, N.J.: Prentice Hall, Inc., 1964. Pp. xiii, 152. $4.50; paper, $1.95.

The number of pages of this elementary price theory text points to both its virtue and vice. The virtue is that it is a concise, extremely well-written text which permits a rapid introduction to price theory for the appropriate (sophisticated) audience. The vice is its limitation solely to this audience without the flexibility possessed by a larger book containing sections directed toward students of varying maturity and capability. A somewhat detailed discussion of the contents and order of presentation should permit an evaluation of its acceptability for any particular student group.

The three chapters presenting the theories of the firm, the consumer, and markets (a total of 93 pages) present the basic tools of price theory in a style which should be clear to a beginner yet assumes a considerable understanding of the methodology of economic models. The treatment of the theory of the firm begins with the production decision, relating revenue and cost considerations and resulting in the $MR = MC$ condition. Next a discussion of technological considerations rapidly introduces isoquants, isocost curves, and marginal productivity conditions. Consumer theory is presented solely in terms of preferences and marginal rates of substitution, with utility functions relegated to an appendix. Again all standard tools are presented—complements and substitutes, income effects, substitution effects, Engel curves, and the derivation of demand curves. Both long- and short-run equilibria are presented for monopoly and for atomistic competition with both standardized and differentiated products. There is, as well, a discussion of oligopolistic behavior.

The introductory and closing chapters discuss the rôle of prices in the determination of equilibrium. In addition to the standard discussion of the efficiency of competitive equilibria, there is a contrast of partial and general equilibrium analyses, with a discussion of the latter for input-output and linear programming models.

As can be seen from this expanded table of contents, the middle three chapters concentrate on presenting the basic tools of price theory as economically as possible, omitting complications and writing for students amenable to and capable of grasping these tools quickly. As such it is a very good introduction for future theorists. The general equilibrium chapters are perhaps too difficult for an introductory course for even these students. However, the mid-

dle chapters would admirably arm a student to go on to the micro parts of the remaining books in this series.

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Economic Choice of Human and Physical Factors in Production—An Attempt to Measure the Micro-economic and Macro-economic Possibilities of Variation in Factor Proportions of Production. By G. K. BOON. (Contributions to Economic Analysis XXXV.) Amsterdam: North-Holland Publishing Co., 1964. Pp. xviii, 332. $9.80.

The traditional theory of production is written at a relatively high level of abstraction and, because of this, there are difficulties in making a smooth application of the analysis to actual industrial cases. A fundamental objective of Dr. Boon's study is to develop an alternate, and superior, approach to production problems. What is sought is a method which is well adapted to empirical investigations and which can be used to determine meaningfully the optimum, or least-cost, technique for producing any commodity output. Emphasis on the question of optimum technique is explained reasonably on the ground that detailed factual information concerning technical alternatives is essential if underdeveloped countries are to be given sound advice in development planning—particularly with respect to the choice between production processes of different capital intensities.

Since Boon wishes to focus attention on the comparative costs of rival productive techniques, he finds it useful to introduce a special form of "break-even" analysis. The point of departure is the assumption that a given commodity can be obtained by employing any one of a number of alternative production processes—where each process is characterized by its association with a particular type of fixed equipment having an annual cost F. Then, for any process, annual output of the commodity (U) can be plotted against the annual total cost of production (T). A linear figure appears because of the further assumption that variable costs per unit of output (v) are constant up to the annual capacity point; in short, each process can be represented by a linear cost function ($T = \mathrm{v}\mathrm{U} + F$) over its effective output range.

Within the system constructed, differences between processes are reflected by differences in slope (v) and intercept (F). In turn, the magnitudes of these variables depend on technology and the factor price structure in existence. If technical conditions are given, the prices of the relevant variable and fixed inputs decide the positions of the separate total cost functions (via v and F). The cost functions are considered important, of course, because they are supposed to indicate how costs vary from process to process and from one level of output to another; ultimately, it is knowledge of the pattern of intersection of the cost functions that allows a firm to select the "optimal" process to produce any stipulated output.

The break-even analysis just described represents the basic tool used by Boon to study microeconomic relationships. Much of the book is devoted to the application of this method to data drawn from particular industries in the

manufacturing and agricultural sectors. These selective investigations have interest in themselves and also because they give rise to definite conclusions concerning the choice of optimal techniques in underdeveloped countries. For example, at the factor prices assumed to hold in such countries, the cost configurations indicate that labor-intensive methods are cheapest in agricultural lines, no matter what output level is produced; but that labor-intensive techniques can be optimal in the typical manufacturing operation only for rather small outputs.

Supporting and amplifying the studies of individual industrial processes is a body of material which attempts to explain some of the underlying technical conditions influencing the total cost functions. Here, the discussion touches on a variety of topics: the definition of plant capacity, the significance of lot size for efficient production, the impact of working capital requirements, the role of new machine design, etc. A number of interesting ideas are presented and, wherever possible, the theoretical argument is buttressed with empirical evidence. Boon is able to escape, in some degree, from the sterility of the conventional abstract theory; he does convey an idea of the dimensions and complexity of actual productive operations. However, he is much less successful in explaining the interconnections between the break-even scheme and the mechanism through which capital is substituted for labor. The trouble lies with the framework of analysis. We are provided with certain information on the physical amounts of factors used and the expenditures made on them, but there is no explicit indication of the way physical inputs are related to output; the production function is simply ignored here.

Although there is talk of investigating the general possibilities for varying capital-labor proportions in production, this objective has not really been achieved. The problem has been reduced in scope and difficulty by assuming, at the outset, a limited number of productive processes based on fixed technical coefficients. In effect, the choice considered exists between input sets which are composed of mutually exclusive elements (e.g., x_i^0, x_j^0 *vs.* x_u^0, x_v^0), not between sets showing different proportions of the *same* types of capital equipment and labor (e.g., $x_i^{(1)}$, $x_j^{(2)}$ *vs.* $x_i^{(2)}$, $x_j^{(1)}$).

By failing to make real use of the production-function concept, Boon renders his own analytic position difficult to interpret, and also looses the chance to link his study with the growing volume of literature on engineering production functions. This last matter has some importance because existing work suggests that the number of operating combinations which can be employed to produce a commodity is often very great. Thus, the essential problem in a large-scale empirical study is to establish the bewildering array of efficient technical alternatives; once the true options are known and factor prices given, there is no particular difficulty in defining optimal techniques according to some cost criterion. On the whole, the book seems to spend too much time on the second task and not enough on the first. Yet, even so, there is some ambiguity attached to the notion of what constitutes an optimal technique. The impression is given at various points in the text that the lowest cost technique for producing a stipulated output is truly an optimal technique —just as one might say, by analogy, that every point on a U-shaped long-run

average cost curve represents an optimal-sized plant. Of course, there may be situations where a limited market makes necessary the use of a relatively inefficient small-scale unit; but, whatever the reasons for the choice, the solution should be recognized as second-best.

In Part II of the book, Boon draws on some of the production data previously assembled and sets up a static input-output model. Three sets of production coefficients, representing three different levels of capital intensity, are defined. Then, using each set in turn, the objective is to determine the total (direct and indirect) factor requirements associated with the production of a given final output mix. Comparison of results permits inferences to be made concerning the possibilities for input substitution within the economy as a whole. This approach is ingenious and may point the way to further work, but, as things stand, it leaves many questions unanswered—e.g., why should the *mechanization level be uniform for all sectors of the economy?* Here, as in the earlier sections, Boon has set for himself a very large and complex project. To make progress, he is forced to employ numerous simplifying assumptions; depending on one's acceptance of these assumptions, there is room for greater or lesser doubt about the value of the results obtained.

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Essays in the History of Economics. By GEORGE J. STIGLER. Chicago: University of Chicago Press, 1965. Pp. viii, 391. $6.95.

As journal articles many of these essays have become such well-known landmarks that no scholar in this field should be unfamiliar with them. From a teaching point of view, however, their collection in book form presents a new and welcome opportunity to introduce students to an analytical (though largely nonmathematical) approach to the history of economic thought. The lucid prose, penetrating logic, and wry humor which have become the author's trademarks make this volume nearly ideal for this purpose. Stigler's essays are no substitute for reading the original classics—nothing could be—and they are obviously not intended as innocuous textbook summaries of old economic chestnuts. There is bite in each essay and sometimes devastating attacks on myths which have grown up out of carelessness or inertia. A student cannot help coming away from this book with a healthy distrust of secondhand accounts and pat generalizations. The view that Ricardo had a "labor theory of value," the supposed unoriginality of John Stuart Mill, the assumed influence of events on ideas and other popular notions receive vigorous, if not savage, rebuttals.

Even a work of this general excellence is not without flaws, or at least points on which a reviewer would take issue. In an essay on the Fabians, Professor Stigler claims that Marx had "denied" that utility was "a universal attribute of commodities." On the contrary, Marx affirmed this on the first page of the first chapter of the first volume of *Capital.* One shrinks from imagining what Stigler himself would have said of a scholar who had committed an error of this sort. In a discussion of the controversy between Malthus and

Ricardo on "Say's Law" and "general gluts," Stigler claims that Malthus "fends" a Ricardian criticism by "introducing rigidity of wages." Unfortunately for this claim, Malthus "fended" this criticism before it was made, by "introducing" wage rigidity in the first edition of his *Principles*. Stigler's whole interpretation of the controversy is open to serious question, particularly his assertion that Malthus' theory "does not contain any germs of a theory of underemployment equilibrium." However, a detailed discussion of this controversy would be too long to be feasible here, and is available elsewhere in any case (*Oxford Economic Papers*, Nov. 1963).

Perhaps the most dubious argument advanced in these essays is the notion that training in economics tends to make one politically conservative ("The Politics of Political Economists"). The evidence advanced is that competent economists of whatever philosophical leanings do not accept certain analytically erroneous economic beliefs of the left. The obvious answer is that they equally reject various erroneous beliefs of the right. Perhaps it might be said that training in any discipline tends to reduce the range of ideas which one will take seriously in the field of one's competence. Scholarly journals in most fields pay little attention to discredited popular ideas.

But despite the inevitable questionable points, this is a book of value to both scholars and sophomores. It treats the history of economic thought as something to *think* about, not as a field for leisurely cataloguing or a source of decorative allusions.

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Economic History; Economic Development; National Economies

Capital Formation and Economic Development. Edited by P. N. ROSENSTEIN-RODAN. Cambridge: The M.I.T. Press, 1964. Pp. 164. $5.00.

A brief note on the dust jacket informs the prospective reader that the ten papers assembled in this volume came from the work of the India team of the Center for International Studies of the Massachusetts Institute of Technology. The authors include the editor, S. Chakravarty, R. S. Eckaus, and Louis Lefeber, sometimes singly and sometimes in pairs.

Neither capital formation nor economic development describes well what these papers are about. *Planning Methodology, with Applications to the Indian Third Plan,* as a title, would convey better the nature and scope of the subjects discussed. Beginning with the national-income-projection type of planning framework commonly used in development plans, the discussion proceeds by successive approximation to multisectoral, intertemporal planning models both of the "fully optimizing" and the merely "consistent" variety.

There is some overlap in the process of generalizing the planning models and to this reviewer the volume seemed to reproduce the evolution of thought in the India Project at MIT rather than to distill and synthesize its results. This may have real advantages, of course, for the reader who is being introduced to the Project's planning methodology. Moreover, it means that each paper could be read alone with profit, though the collection constitutes an in-

tegrated presentation of its subject. I suspect, however, that many readers would have preferred the allocation of less space to where the thinking of the India Project has been, and more space to where it is now. Most of these papers are several years old.

The models are essentially applications of dynamic Leontief systems and linear programming. What special interest they have must derive from their practical applicability to development planning, especially in India, and from the choice of policy variables, which gives them substantive content. In the three instances in which there is an attempt to relate theory to Indian practice, the results are disappointing on both counts.

One instance involves suggestions about "approximate procedures" for estimating shadow prices for capital and foreign exchange in lieu of a general solution of a programming model. One would like to have a shadow price for foreign exchange that measures the marginal opportunity cost of saving or earning foreign exchange in both import substitution and export expansion. Chakravarty's method of calculation permits import substitution only with respect to consumer goods, not raw materials or capital goods. Moreover, exports are taken as exogenously determined, a procedure defended by an assumption of relatively low price elasticities of demand. A shadow price for foreign exchange calculated on the assumption of limited possibilities for import substitution and no opportunities for export expansion is bound to exaggerate the disequilibrium and, at the same time, to discourage the use of a shadow price where it might be most important in influencing resource allocation.

The shadow rate of interest calculation is also biased against choice in that it abstracts from the possibility of changes in factor proportions. It is, in fact, nothing more than the calculation of the rate of profit on capital needed to provide the saving to finance a prescribed rate of growth in a modified von Neumann model. This makes sense only if factor proportions really can't be changed much, either within industries or in the aggregate by altering the weights of industries, so that the only function of the rate of interest is to influence income distribution.

Another instance is the attempt in Chapter 5 to apply to the Indian case the basic planning model that appears in one form or another throughout the book. In its simplest aggregative form, the model can be described by the differential equation

$$b K(t) = \bar{C} e^{rt} + K(t)$$

where b is the output-capital ratio (a datum), $K(t)$ is the capital stock at time t, $\bar{C}$ is the initial level of consumption, and r is the prescribed rate of growth of consumption. If the length of the planning period and the initial and terminal levels of the capital stock are specified, the solution yields the required initial level of consumption. Estimates of the value of $\bar{C}$ for India on the eve of the Third Plan are calculated on the assumption of a planning period of 15 years and various alternatives for r and $K(15)$. These are then compared with actual Indian consumption in 1960-61 to "demonstrate that what is done right now does make a great deal of difference for the future."

This conclusion is correct in the case of selecting alternative rates of growth of consumption, which do indeed require sharply different initial levels. Changing the length of the planning period or the value of the terminal stock of capital does not have this effect, however. In fact, the reader is startled to learn from the tables presented that doubling the capital stock requires scarcely more sacrifice of consumption than does maintaining the capital stock constant. There appears to be no real conflict between capital formation and consumption.

A view of the actual time paths implied by this model demonstrates that it may not be so much "what is done right now," but what is done later that makes all the difference. This is obvious in the case of a model in which consumption grows steadily while the terminal value of the capital stock must be equal to its initial value. The stock in this case must first grow and then decline. If consumption grows at 6 per cent per annum, the absolute value of disinvestment in the final year (15) exceeds the value of the national income. But then no one would expect such an assumption about the capital stock to yield a feasible time path of investment.

Even with the apparently plausible assumption of a doubling of the capital stock in 15 years, however, the time paths exhibit some awkward features. If the consumption growth rate is 2 per cent, investment in the last year must exceed one-third of national income. If r is 6 per cent, the capital stock overshoots and disinvestment is required in the final year. It is not that these paths are impossible to achieve. It is rather that they are unlikely and undesirable paths of growth. What is needed is a side condition relating to the feasibility of the time path of growth in the light of the institutional framework and the constellations of policies that can be politically implemented. Moreover, the terminal conditions must specify more than the capital stock and its composition. The institutions and policies that determine the terminal saving rate—both average and marginal —are equally important. Finally, a lesson from the model seems to be that the rates of growth of consumption and capital stock cannot stray too far from each other if the above-mentioned side condition of feasibility is to be met. This means combinations of values for r and $K(15)$ that are not too far from those which make $\overline{C}/b\text{-}r$ equal to $K(O)$, which is the condition for "Golden Age" growth.

There is no explanation as to why the initial level of consumption is the residual variable to be determined by specifying the other choice elements; yet this is the procedure most often followed throughout the volume. One suspects that it is because this is a means of estimating the foreign aid requirement without saying so. In any case, Rosenstein-Rodan, in Chapter 2, does treat foreign aid explicitly as a substitute for domestic saving in his analysis of the Indian Third Plan. Moreover, he not only avoids the error of neglecting the question of feasibility of the time path of saving and investment, he seems, in fact, to be overwhelmed by it.

Rosenstein-Rodan begins with the so-called "Pant-Little" model based on pre-Third Plan memoranda. An investment program of Rs. 10,000 Crores, foreign assistance of Rs. 1,000 Crores, and an output-capital ratio of .45 are the basic assumptions of this model. Substituting what, in his view, is a more

realistic estimate of .33 for the output-capital ratio, Rosenstein-Rodan finds that this implies an unrealistically high requirement for the marginal saving rate. The way out he suggests is to raise foreign aid to 2,500 Crores, keeping investment, however, at the same level.

It is interesting that the actual Third Plan includes this higher level of foreign aid, but retains the more favorable output-capital ratio of the Pant-Little model! And, as in the Rosenstein-Rodan solution, investment is kept at 1,000 Crores, permitting an even lower marginal saving rate.

Approximate values for these alternative Third Plan models are reproduced in the first three columns of Table 1. In columns (4) and (5), I have suggested alternatives to Rosenstein-Rodan's solution (2) and the actual Plan (3) wherein the only change in each case is to allocate the increase in foreign aid to investment instead of to consumption.

Note that the result of allocating the rise in foreign aid to investment rather than to consumption is to raise consumption as much in (4) as in Rosenstein-Rodan's solution and by more in (5) than in the actual Third Plan projection. It seems that real consumption requirements are not a constraint on the investment program within this range of alternatives. Yet Rosenstein-Rodan's argument that the increase in foreign aid should substitute for domestic saving rather than add to investment is based on seemingly plausible assumptions about sectoral income shares and saving propensities. The conclusion that emerges from this picture is, I suggest, that the real constraint is absorptive capacity —in the very broad sense. Either the output-capital ratio would be depressed by the increase in the investment program or the government would be unable to implement the kinds of fiscal and other policies that would ensure meeting the saving target. In any case, the essential problem is not, I think, what Rosenstein-Rodan seems to suggest—the capacity to endure (or enforce) austerity.

TABLE 1—ALTERNATIVE THIRD PLAN MODELS
(Crores of Rupees)

	(1) Pant-Little	(2) Rosenstein-Rodan	(3) Third Plan	(4) Alternative to (2)	(5) Alternative to (3)
Output/Capital	.450	.333	.450	.333	.450
Investment	10,000	10,000	10,000	11,500	11,500
Saving	9,000	7,500	7,500	9,000	9,000
Foreign Aid	1,000	2,500	2,500	2,500	2,500
Δ Income	4,500	3,330	4,500	3,830	5,175
Δ Consumption	3,250	2,580	3,750	2,580	3,925

I must regretfully add, in closing this review, the comment that this volume would have benefited from further editing. Mistakes abound. There is neither a preface nor an introduction. And when the book goes on the library shelves even that cryptic bit of information on the dust jacket will disappear.

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Planning Economic Development. Edited by EVERETT E. HAGEN. A Study from the Center for International Studies, M.I.T. Homewood, Ill.: Richard D. Irwin, Inc., 1963. Pp. x, 380. $7.50.

Professor Hagen introduces and summarizes the lessons from nine accounts of planning in various parts of the world. Three of the nine studies relate to Southeast Asia, but what they have in common is not merely the geographic location, but that they present, in the words of the heading of Part II of the book, "A Decade of Experience with Formal Planning Machinery." Three further cases relate to "Varying Planning Procedures in Three Countries with a Decade of Rapid Growth," and they are as diverse as Japan, Mexico, and Yugoslavia. Finally Iran, Nigeria, and England are presented as special cases whose common denominator is simply that they are interesting.

A collection of essays by different authors is always difficult to summarize and to review, even when they have a common topic. All of the essays are highly competent. Yet it is inherent in the topic that they must be diverse. Planning proceeds in an historic context and is, I believe, essentially a method of harnessing economic reasoning to the purpose of mobilizing and allocating resources for growth. The nature of planning will therefore differ not only with the degree with which it is taken seriously in different countries—and some of them manifestly pay lip service to it for reasons of their own which have little or nothing to do with economic development; it will not only vary with the competence of the personnel undertaking it, but ideally it ought to be adjusted to the problems which a particular economy has to overcome at the moment of planning, and it ought to be adjusted to the realistic alternatives open to a particular economy at a particular time.

This does not mean that planning is so heterogeneous an activity that nothing general can be said about it. It accounts, however, for the fact that it is very difficult for Hagen to summarize "The Nature of a Good Plan" (Ch. 11) in a manner which would really be applicable everywhere.

The chapters on Southeast Asia essentially recount failures of planning, or rather the immense difficulties with which planners are faced in India (J. P. Lewis), Burma (L. Walinsky), and Pakistan (Clair Wilcox). Each author speaks with authority, each is candid about the failures, which include overcentralization and underestimation of the considerable importance which good economic policies have to play in planning. Each author stresses what Lewis calls the "shoddiness of the whole operation" (p. 108). Each brings out the fact that to a large extent planning hung in the air, not because it was formally separated from execution, but because it had little influence on policy and assumed an unrealistic degree of executive ability.

The rapidly growing countries represent entirely different problems. Yugoslavia is, of course, the remarkable phenomenon of a decentralized planned economy. What the Yugoslavian story tells is the crucial fact that the heart of economic planning is the development of good economic policies and of institutions capable of carrying them out.

Needless to say, the chapters on Japan by Shigeto Tsuru, and on Mexico by Miguel S. Wionczek, tell interesting stories in an interesting manner. Both

authors are critical of what has been done. This is evident already by the titles of their contributions: "Formal Planning Divorced from Action: Japan"; and "Incomplete Formal Planning: Mexico." Hagen, in his summary discussion, takes up the critical view: "The adoption by Japan of a competently staffed planning agency and *its use as a facade* not closely related to government-decision-making processes and thus not interfering with the traditional practices of Japanese government operations is an instance of the frequently observed tendency of the Japanese to absorb external forms from other cultures but to incorporate them into Japanese practice only in ways consistent with underlying Japanese values" (p. 361; my italics). Specifically, the Japanese are criticized for failure to provide for basic transport facilities.

I know nothing about Mexico and only little about Japan. I had extensive talks with Japanese planners, and they all did indeed complain of not being used properly. Yet I continue to be (a) unmoved by their complaint; and (b) deeply impressed by both their competence *and* their achievements. The Japanese ability of adapting other people's ideas without losing their own identity is of course the secret of their success, and the highest compliment one can possibly pay them.

Now, surely, it is relevant to point out that the countries criticized for not having certain aspects of planning are the ones that have in fact achieved spectacular growth. Certainly Japan, and probably also Mexico, were already at a fairly high standard of sophistication in their economies when they started to plan, unlike Burma, Iran, Nigeria, or even India and Pakistan. Surely, a planning operation must and should look different in an economy and society which, like Japan, literally has tens and hundreds of thousands of highly competent people of all kinds, from one in which this is manifestly and painfully not so. Surely it is not very relevant to criticize Japanese planners for constantly underestimating the achieved rate of growth. My advice to underdeveloped countries looking for a model would be to look to success: Japan made it; it made it at least partially by good policies and drawing lots of people into the actual policy-making process. India, the example usually recommended, has certainly not been successful as yet. No doubt this has been at least partially due to reasons beyond its control. Just the same, we have little evidence that Indian planning methods are actually effective, even though they seem to be neat and complete.

Skipping, for reasons of space, over the chapters on Iran (P. Bjorn Olson and P. Nørregaard Rasmussen) which might be described as a rhapsody in frustration; Nigeria (Peter B. Clark) with which I am too closely associated to discuss, and England (G. D. N. Worswick, I would like to end with some comments on Hagen's summing up. Like everything of Hagen's, this summary is stimulating, and it would not be as excellent as it in fact is if it were not also controversial. I have already indicated my belief that the heart of planning is policy, and I find that in many respects the ordinary budget is the most important planning document. Yet I would hesitate to say that this view differs much from Hagen's. When confronted with actual problems, different planners with equal competence will come up with similar prescriptions, regardless of their ideological background or predilections.

Yet there remain some questions. The advice is often given to plan ambitiously and beyond the capabilities of a country. Hagen is more cautious and advises only that "Any country's reach should exceed its present grasp. It should always be stretching its program to the limit of its capabilities and, moreover, be considering how these capabilities can be increased" (p. 330). Maybe. But engineers who give such advice would not be taken seriously. *They* build in safety factors against defects of materials and so forth. What is so sound about neglecting safety factors in economic planning? When the "thinnest dam in the world" broke a few years ago, it cost hundreds of lives. When a plan that is stretched to the limit is taken seriously and then fails, all priorities get distorted, and a mess is created which causes unnecessary misery to millions who already have more than their share of worldly troubles. And what happens politically when ambitions that have set the imagination of people on fire are not fulfilled and frustration sets in? Particularly when the charismatic leader, when a Nehru, dies? I find the advice dangerous in the extreme and unacceptable. The present mess into which the Nigerian Plan has fallen is largely due to such advice.

A second fundamental disagreement refers to the use of aggregative planning. The latter is, of course, necessary, but primarily as a consistency check. Hagen's proposition that "On principle, budgeting for current expenditures, like planning for development, should rest on aggregative economic analysis" (p. 343) is unacceptable to me. "On principle" it should rest on detailed microeconomic considerations and analyses, since aggregates are logically built up from them. In fact, *some* aggregative procedures remain necessary as an unavoidable, though highly undesirable, short cut. But a plan that does rest only on aggregative analysis without links to underlying *real* things is not worth the paper it is printed on.

A review is hardly the place to discuss the relationship of micro- and macro-concepts. But surely, one reason why the macro-concepts work satisfactorily in advanced economies is that well-working market mechanisms make it possible to assume legitimately that the underlying microeconomic relations will work themselves out satisfactorily. The less this is the case, the more dangerous the use of macro-models becomes.

Thus, Hagen's proposition that in the course of evaluating the prospective fiscal and economic situations, the planning agency and the budget must jointly estimate, at least provisionally, the "level of aggregate government expenditures which will achieve full employment of the nation's resources . . ." is very heady wine indeed. I wonder how one would do this in India or Nigeria, with presumably large unemployment of unskilled labor and extreme scarcities of every conceivable complementary factor from foreign exchange to managerial talent. Is this really the way to go about tackling this basic problem?

It is evident from this review that Hagen has edited a stimulating book which as yet has no rival. Though I find much to dispute, I nevertheless am uncertain just how big our disagreement really is, since Hagen's discourse of planning and its difficulties is very extensive. For example, he does point out that Japan has, after all, good development planning, even without formal

machinery (p. 360), and while the failure to provide basic transport facilities is a black mark against the Japanese planners, it is also true that they tackled this problem when it became evident. Can formal planners always say that much for themselves?

The difficulties of writing a brief yet comprehensive review bring out the point of the book: development is a complicated and heterogeneous process, and planning is a highly pragmatic activity. You do what works in a particular situation. Economic theory has some lasting things to say, such as that resources are scarce; that you can't do everything at once; that everything takes time; and so on. But what you prescribe in a situation depends on this situation, and comes close to defying generalizations.

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Essay in Southern Economic Development. Edited by MELVIN L. GREENHUT and W. TATE WHITMAN. Chapel Hill: University of North Carolina Press, 1964. Pp. xix, 498. $7.50.

This volume is a collection of papers on recent economic developments and economic issues in the South. There are 14 papers ranging in scope from economic history to econometrics. Their content supports the reviewer's belief that regional economics consists of the reformulation and application of economic analysis to specific problems and bodies of data, rather than a set of techniques formed from the union of many disciplines.

The reader opening this book should expect to learn two things: why the South is different, that is, why it is a specific region with its own problems; second, how economic analysis may be applied to the understanding of regional problems and the prediction of change. I think that the first goal is met better than the second.

Clarence Danhof's excellent paper, "Four Decades of Thought on the South's Economic Problems," sets the stage for the first section. Danhof cites the earlier explanations of the low income position of the South and notes the gradual disappearance of the conspiracy thesis. The latter was posed in terms of tariffs, railroad rates, monopoly, and absentee ownership. In part, this change of view is due to the pioneering work of Hoover and Ratchford, and to the investigations of Hanna, Spengler, Nicholls, and others. With his eye on the slow progress of the past, Danhof is very likely too pessimistic about the future of income levels in the South. He argues, "In the South, the economic progress that has been accomplished does not appear to provide a broad base for accelerated growth." I think that time will prove this view too limited.

In a noteworthy essay, "Negro Entrepreneurship in Southern Economic Development," H. B. Young and J. M. Hund report on a large-scale interview study. This area of research should certainly be pursued further for the light it sheds on the region's most important social and economic problem. Young and Hund indicate the distance which must be traveled before the Negro entrepreneur becomes an important element in our economy. At the

moment, the Negroes in the South do not participate fully in business enterprise, and those who do might conceivably be injured by the elimination of segregated patterns of economic relationships.

The book's second section contains a number of statistical studies of income, employment, population, migration, and industrial composition. These studies are by J. M. Henderson, J. L. Fulmer, and F. A. Hanna. Fulmer's analysis of migration contains the seeds of a fruitful theory of growth. In Fulmer's thinking, migration plays two roles. On the one hand, it is a factor improving per capita incomes. This is true in a purely definitional sense if the migrants have lower per capita income than the remaining population. On the other hand, the growth of Southern metropolitan areas has depended strongly on intra-regional migration from farm to city. In many cases the migrants to Southern cities have had the same characteristics as those who left the area entirely. Economic growth in the city proceeds by raising the productivity of migrants and involving them in a superior mix of economic activities.

The third section of the book contains essays which are regional in orientation, but not restricted to the South. These are more speculative and theoretical than the earlier papers.

Three authors are concerned with aspects of efficiency in government programs. M. R. Colberg has written a critique of Area Development Programs in terms of the conflict between legal-administrative restraints on federal agencies and the requirements of correct resource allocation. Robert Haveman has analyzed investment criteria used by the U.S. Army Corps of Engineers in their construction programs in the South. As one would expect, he concludes that resources have very likely been wasted, while the South has benefited both from the transfer of income and from the indirect stimuli to agricultural out-migration. James Rinehart has computed Rates of Return on Municipal Subsidies to Industry. The rate of return is computed by evaluating the "perpetual annual value of the income benefits over the capitalized cost of the subsidy." The income benefits consist of the incremental income flow arising from the plant payroll itself and from the income multiplier effect. The rate of return so computed is undoubtedly very high and leads Rinehart to conclude that such subsidy activities are profitable to the localities practicing them. These rates of return cannot be used to formulate national policy, since each municipality is treated in this context as an independent country. From the point of view of the national economy, such subsidies make sense only when there is a discrepancy between the private and social cost of hiring certain locally immobile resources. This was pointed out by Buchanan and Moes in another context.

The last, and perhaps most ambitious, essay is a paper by C. E. Ferguson which estimates the amount of capital in U.S. and regional manufacturing industries (two-digit S.I.C.). Ferguson's method is an extension of one originally employed by Gallaway, in which a Cobb-Douglas function was fitted to data on labor input and value-added output. With the use of a homogeneity assumption, it is possible to estimate the quantity of capital employed in each regional industry. Ferguson carries the procedure one step further by using a CES production function. He also computes implicit values of rent per unit of

capital, and uses them in a moderately successful attempt to predict rates of growth of capital. Ferguson does not attempt to compare his allocations with those made by Gallaway, nor with estimates of capital in regional manufacturing industries prepared by the Census for 1957. Nevertheless, his work is very suggestive and will undoubtedly be followed by other investigations.

These essays do not provide a wholly satisfactory forecast of future work in regional economics as it applies to the South. Over time, the Southern economy is coming to resemble the rest of the country. Incomes of white urban Southern families are very close to incomes of white urban Northern families. The problem of the South should be seen partly in terms of resource allocation in agriculture and mining, and partly in terms of the low productivity and income of Negroes. These are the areas where future research will pay off, in giving us greater understanding of how the economy behaves and help in formulating intelligent policies.

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Inflation and Growth in Latin America. Edited by Werner Baer and Issac Kerstenetzky. Homewood, Ill.: Richard D. Irwin, Inc., for Economic Growth Center, Yale University, 1964. Pp. xix, 542. $9.50.

Early in January of 1963, a large and distinguished group of economists gathered in Rio de Janeiro to discuss inflation and growth in Latin America. The meeting was a major event in the controversy between structuralists and monetarists. The former believe that inflation is due to sectoral rigidities or bottlenecks, the latter to credit creation and printing of money. This book is the final outcome of the conference and, quite likely, marks a low ebb in the not-so-good relations between two U.N. agencies, the IMF and ECLA. It has three parts: I, Summary Papers; II, Papers and Comments—the main body of papers presented at the conference which come to focus with brief criticisms by the participants—and III, an edited version of the panel sessions.

Richard Ruggles' summary is a useful opening piece. It describes the rest of the book, and the reader uninformed about the controversy will learn of it before getting lost in the specific points each writer is trying to make. W. Arthur Lewis' closing remarks are a balanced effort to show the compatibility of the opponents' ideas, to suggest the explanatory value of each, and to give each one its due. In doing this I think he thoroughly succeeds; his is a paper which should be read again before closing the book, especially because he leaves one with the impression that there is nothing really peculiar about Latin American economic phenomena. He finds only two characteristics in this region different from others: namely, collective tolerance to persistent budgetary deficits, and the balance of political power lying in the hands of those opposing change. Neither of these, even if true, can be explained solely by economists.

Part II is comprised of papers on the theoretical framework, statistical and organizational problems, comparative analysis of experiences of Latin American and other countries, and an evaluation of policy instruments. Dudley

Seers and Graeme Dorrance present the structuralist and monetarist points of view in a part that is perhaps the weakest of the book. Seers, having expressed his views extensively elsewhere, I presume to avoid being repetitious, presented a paper that on its own says little about structuralism. It says not much more than that the assumptions of nineteenth-century liberalism are not applicable to Latin America. Dorrance came up with an interesting paper which, unfortunately, dodges the issue. His is a paper intended to show how inflation causes structural maladjustments: it decreases liquidity and domestic saving; thwarts capital markets; dissuades foreign investment, export diversification, and long-term investment; stimulates inventory accumulation and capital exports.

The sections on statistical and organizational problems discuss information and policy-planning requirements for growth. Within them a short comment by Kalman H. Silvert is a golden piece which shows how much insight is to be gained in development economics with the collaboration of political scientists. The experience of countries located in other parts of the world follows. Sidney Dell's paper on the industrial countries is informative but diminished by his pursuing a feud all his own. John M. Montias' paper on Eastern Europe shines with a description of instruments and objectives of monetary policy in socialist countries. However, his description of selected countries' experiences is marred by excessive details. Otherwise, the paper is especially illuminating, since the main objective of financial policy in the socialist countries is to assure economic growth, unlike that in other countries where it is mainly used for short-run stabilization. Tun Thin's paper on Asian countries received a quick dismissal in an objective comment by Gustav Ranis. The section comparing the experience of Latin American countries and of policy instruments just sketches both. However, among these, some notes of Arnold Harberger about the effects of devaluation on the price level are quite original and solid. He shows, for instance, a positive effect of devaluation on the price level not only when money wages are allowed to increase, but even when they are held constant.

Part III, Panel sessions, shows an array of opinions of distinguished economists on changes in economic structure, investment, international, fiscal and financial policies, and income distribution. It is quite amusing to see, even after editing, how economists discuss a point, without listening to each other —each trying to impose his own thinking on the rest.

One gets the impression that some agreement was gained that moderate inflation can, but rapid inflation cannot, be explained on structural grounds alone. The reader should be aware by now that this is a rather important book in coverage and, although uneven, in depth too. The book will be a standard selection in reading lists on development economics, especially as a complement to Hirschman's Essays. The specialist, too, will gain much by reading it, and members of the profession engaged in other fields will get an insight in this field by reading at least the summary papers and Harberger's. However, it is a disappointing book in many respects. Its gaps are considerable. For example, nowhere does there appear an explanation of the assumptions, objectives, and set of instruments underlying the stabilization programs

of the International Monetary Fund. We must resort to David Felix's description to get some feeling of what credit ceilings and the other monetary devices used by the Fund are supposed to mean and accomplish. There is no analysis of Mexico, Peru, and other countries which have turned from growth with inflation to grow with reasonable price stability. Such an analysis would complement the descriptions of the countries plagued by rampant inflation and would help to provide understanding of growth and inflation. The statistical handling of variables is generally unsatisfactory, showing that in this field much is to be gained by more quantitative testing of those theories that have been repeated scores of times. In Part III, reference is made several times to a paper by Kaldor that was not published, and the reader is left wondering what it was all about. Finally, a gloomy note. The book shows once again, the fact repeated in many a conference, that most of the main papers were written by outsiders. It is sad to admit that economic backwardness in Latin America is accompanied, with few exceptions, by lack of economic research by, and professional advancement of, Latin American economists.

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Economic Survey of Latin America, 1962. Prepared in the Department of Economic Affairs of the Pan American Union. Baltimore: Johns Hopkins University Press for Organization of American States, 1964. Pp. xv, 444. $8.50.

This Survey has had a checkered career with changing sponsors over the past few years. The 1962 volume has been subdivided as follows: Part I deals with Latin America (excluding Central America) and the Alliance for Progress; Part II, with Central America. In general, a good balance is maintained between over-all Latin American developments and specific country and sector analyses. However, the following negative criticisms are made: (1) despite the plethora of statistics, data for fitting the study into a theoretical framework for growth are missing, and expressing many of them in percentage rather than value or volume terms compromises the utility of some tables; (2) theoretical postulates which guide the OAS's assessment of countries' development performance are not stated. Less than half of nineteen countries attained in either 1961 or 1962 increases in per capita domestic products which conformed with Punta del Este goals.

Part I consists of five chapters which deal with the following topics: (1) exports and markets for basic products; (2) import capacity and balance of payments; (3) total production, capital formation, and monetary developments; (4) the manufacturing sector; and (5) the Alliance for Progress.

Some progress has been made towards establishing an international mechanism for compensating against losses in export earnings arising from price fluctuations. The international agenda includes proposals from the International Monetary Fund (February, 1963), the United Nations (January, 1961), and the Organization of American States (April, 1962). These preserve the desiderata of free-market price determination; none envisions a

maintained price-parity relationship between manufactured and primary products such as Latin Americans have clamored for since the 1940's. EEC discrimination against coffee and bananas remains of special concern; IA-ECOSOC action groups study world markets of particular primary products. The International Coffee Agreement is still stymied in the U.S. Senate because of charges of cartelization, supply control, and price manipulation to the detriment of U.S. consumers.

The Survey recognizes the need for banking reform to carry out Latin American monetary policies. Money markets are "thin." There is almost a complete divorce between countries' external payments' positions and the evolution of liquidity; government deficit financing has been the major factor in the expansion of the money supply. Overvalued currencies result in important losses of foreign exchange resources. More specifically, the Argentine peso was depreciated by more than 60 per cent in 1962; Brazil's cruziero, 50 per cent between December, 1961 and May, 1963; Chile established a dual exchange rate; Colombia's import rate was altered by 50 per cent; etc. High amortization payments of as much as 30 per cent of import capacity in Argentina can interfere with development policies. A high proportion of foreign aid goes into servicing foreign debt. A judicious debt policy can contribute strongly to development, but public debt ranges from three to thirteen times gold and foreign exchange reserves of some countries. In 1965, external aid for Latin America is estimated at $3.1 billion; $1.9 billion will go for debt amortization.

Some Latin Americans continue to regard the *Alianza* with suspicion as a means of perpetuating U.S. influence, and rarely as a humanitarian effort or experiment in social change. Many Latin Americans strive to extricate themselves from a crass, materialistic U.S. exposure which, they feel, subverts their national identities and their more senstive and spiritual outlook. However, the *Alianza* has clearly provided a salutary impetus to Latin America and in fairness cannot bear the total onus for the impediments to profound structural changes which are the product of historical, social, and economic conditions.

Latin American countries have plans of considerable detail, completed or in process. The extensive plans defy the reader; the miniscule description is remarkable; their inherent logic and consistency require careful analysis. They may be examples of "learning by doing." Planning is restricted mainly to the public sector, which accounts for 50 per cent of total investment in Chile to less than 20 per cent in Haiti and Panama. The Survey does not discuss techniques of programming recommended or feasible in Latin America.

Private investment inflow slackened to $23 million in 1962-63 compared with a modest target of $300 million. More funds are required from Japan, West Europe, and the United States; ADELA may help. The Punta Charter was mute on this important point. A decline in private foreign investment probably implies a substantial deficit of local private investment as well, since similar factors operate here. The average rate of return of U.S. manufacturing industries in Latin America after depreciation was 3.6 per cent for 1962 compared with 8.8 per cent in the United States and 11.3 per cent in Western Eu-

rope. Over 1957-62, these average rates were 6.6 per cent, 8.7 per cent and 14.9 per cent, respectively.

Part II deals with Central America and contains an introduction and three chapters on (1) export structure, developments, and prospects, (2) structure of production, and (3) intra-area trade. Although economic integration prospects are encouraging, Central America, which will remain small by most criteria, can derive encouragement from other areas which have attained high per capita incomes with highly skilled populations.[1]

"Import substitution" has been raised to the level of a credo in underdeveloped countries. This reviewer becomes concerned when he reads that the range for import substitution has been completed for consumer goods in some Latin American countries, is greater for intermediate goods, and still greater for capital goods. Such statements are implicitly predicated on static analyses and overlook that lines among classes of goods are clear-cut, not fuzzy; that increases in incomes have little or no effect on demand patterns and elasticities; that development leaves factor endowments, their relative prices and productivities, and technological levels almost unaffected. In his Wicksell Lectures, Nurkse noted that import substitution occurs not only when home-produced goods displace imported goods, but also when capital-good imports are substituted for consumer-good imports so domestic capacity increases. Moreover, in Graham's terms, import substitution can be affected by the density of the productive structure in response to small changes in the terms of trade as well as by factor mobility. Finally, to assess import substitution by reference to the trend in the import coefficient is a highly dubious procedure.

The Survey contains projections for Central American exports which are expected to increase approximately 30 per cent in real terms between 1960 and 1967. On the other hand, from information in the Survey, I have estimated that in the period 1955-62 export proceeds foregone for the area amounted to a *staggering* $700 million, or about one-fourth of total export values: $600 million for coffee; $60 million for bananas.

Significant sectoral developments occurred in Central America between 1940 and 1962: the primary sector declined in relative importance; the tertiary sector showed the greatest increase; manufacturing increased slightly. The Central American profile closely approaches that described by Chenery for a typical low-income area.

This Survey should be required reading for Latin American specialists. This reviewer remains concerned that the OAS fails to outline the type of economic growth theory it uses to evaluate Latin American change. Developments in Latin America and elsewhere should provide economists with an unparalleled opportunity to enrich our theoretical insights into very complex phenomena. This enrichment is desirable not only for its own sake, but also because in the process the "strategy of development" can be more rational and effective.

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[1] *Economic Consequences of Size of Nations*, A. Robinson (ed.), International Economic Association (New York 1953).

Economic Development for Africa South of the Sahara. Edited by E. A. G. ROBINSON. New York: St. Martin's Press, 1964. Pp. xvi, 733. $21.00.

Students of economic development and African economies should welcome the publication of this volume, which offers a broad view of African development problems as well as suggested approaches to solutions. This book, recording the proceedings of the third regional conference of the International Economic Association, provides an analysis of sub-Saharan development by both "outside experts" and academic economists from Africa. The volume is divided into two parts, the first containing an interpretive background paper prepared by G. J. Ligthart and B. Abbai of the U.N. Economic Commission for Africa, a succinct outline of African development problems presented by E. A. G. Robinson, followed by careful studies of the various geographical and political entities, such as the French-language area, East Africa and the Congo. The second section covers the conceptual aspects of development as they pertain to Africa. Papers given by J. J. Spengler, D. H. Houghton, J. Vaizey, and S. Enke are concerned with problems of population and human capital. S. H. Frankel and L. B. Rist center their attention upon capital supply in relation to the developmental needs. Trade and commodity problems and price formation are dealt with in papers by W. E. Moran, P. T. Bauer, L. H. Dupriez, and G. Leduc. The sectoral questions of agriculture and industry form the core for the studies of H. W. Singer, D. J. Viljoen, Roland Pré, and M. Yudelman. The final two topics included relate to monetary and fiscal policy (papers by A. T. Peacock and L. H. Samuels) and what might be considered an exposition of the *raison d'être* for the conference itself in W. P. Stolper's appraisal of the contribution of economic research to African development.

Emerging from the 26 articles and the succeeding discussions of 42 participants are several principal lines of thought. Contrary to impressions of a slow rate of economic development, the consensus reveals that African economies are far from being stagnant, witnessing relatively high rates of growth (4 per cent to 6 per cent of the GDP) which compare favorably with other developing and some developed nations. If this trend is maintained, the per capita income could double in about 25 years. Such development, however, is considered contingent upon the inflow of capital from abroad, which has been drastically reduced after independence. Unless foreign aid replaces the gap left by declining foreign investment, growth could be impeded. In discussing the supply of capital, however, the conference did not stress sufficiently the impact of foreign aid on the stimulation of trade as well as a means of attracting further investment.

On population problems relating to Africa, Spengler emphasizes the fact that the area is relatively underpopulated; that Africa could probably sustain a larger population, as much as three times the present level. Nonetheless, Spengler stresses the cost of population growth and points out the danger of diverting from increasing per capita wealth to equipping new increments of population. If further population growth is found desirable, "it had best take place slowly" because of the present inverse relationship between the rate of per capita saving and the rate of natural increase.

The possibilities of raising per capita output were widely discussed throughout the conference. It was noted repeatedly that the rate of economic development in Africa is basically dependent upon the degree of change from the traditional, low productivity subsistence economies to a market exchange economy. In both the agricultural and industrial sectors, Houghton locates the main difficulty in the inability of African labor to seize fully upon the causal relationship between productivity and wages. While a backward-sloping supply curve of labor still exists, this is mainly in the mining areas where alternatives for employment are limited. In other industries, however, the level of wages is becoming the most significant single factor in attracting a stable labor force. The most difficult of all labor problems is the low productivity in agriculture with 30 per cent of the GDP arising from self-subsistence agriculture and where response to price incentives is weak. Most of the topics presented in the conference sooner or later had to deal with the intricate question of agriculture. Yudelman's specific treatment of this problem singles out the ramifications of low productivity in the inadequate level of investment in the sector and the failure to recognize that spending for educational training and research is actually a developmental expenditure. Moreover, it was agreed that increased productivity is more of a general problem than diversification.

Singer provides the case for developing small-scale industry as a means of diversification and raising the per capita income. He points out that small-scale industry could best utilize the localized or tribalized savings potential and that industrialization of this scale would spare the area from a condition of isolated enclave economies. Unlike Hirschman, Singer views small-scale industry as capable of producing skills and technological knowledge which could in turn be transferred to medium- and large-scale industrial operations, thus avoiding the high cost of trial and error through a lack of managerial and technological know-how in heavy industry. Finally, the question is posed whether, at this present stage where the political boundaries are largely arbitrary and do not recognize economic considerations, it would be wise to embark on costly large industrial projects which could create obstacles to regional development. The reviewer agrees here with Singer, but finds it unfortunate that regionalism was not treated as a factor to explore concurrently with the drawing up of national plans for industrialization and development. Indeed, a paper on regionalism would have been a valuable addition, especially since 1961, the year of this conference, saw something of a gestation period for ideas of coordinating institutions such as the African Development Bank.

Most works on Africa run the risk of rapid out-dating because of the dynamic aspects of the continent. Since the presentation of the papers in this volume, a number of new and independent African nations have emerged, with consequent interactions between the economic and political spheres. However, the recent alterations on the economic scene do not detract from the general validity of this volume, which offers a basis of knowledge sorely lacking in the field of African studies. The broad scope, the depth of analysis, and the grasp of the basic developmental elements of Africa make this volum-

inous collection of papers and discussions worth close study.

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Economic Transition in Africa. Edited by MELVILLE J. HERSKOVITS and MITCHELL HARWITZ. Northwestern University African Studies, No. 12. Chicago: Northwestern University Press, 1964. Pp. 444. $7.95.

These 16 essays originated in papers presented at a conference in 1961 sponsored by the Committee on Economic Growth of the Social Science Research Council. Each surveys a broad topic applied to all or a large part of Sub-Saharan Africa. Much of the effort consists of bringing together materials from the specialized literature, although anthropologists' field notes and major statistical reformulations introduce fresh evidence. Considered as a compendium of information, the volume has the advantage of the expert's touch, but is inferior to standard references such as Hailey and U.N. surveys, which are in the nature of the case more systematic and comprehensive. This is in the way of information, not criticism. The enterprise should be taken on its own terms.

The late Professor Herskovits' introduction makes clear that the uniqueness of the contribution is to be found in its interdisciplinary approach. The subject of the conference, "Indigenous and Induced Elements in the Economics of Subsaharan Africa," is made to order. The particular version of interdisciplinary study represented here recognizes sharp distinctions among disciplines, notably between the poles of a holistic and cross-cultural anthropology and an economics which emphasizes one aspect of human behaviour and confines its applications to industrialized societies. The vacuum created in the study of nonindustrial societies by the latter circumstance is to be filled by a more broadly based anthropology practising a kind of "microeconomics." The affinity of the two disciplines, especially in the study of transitional stages, is largely confined to the exchange of materials and insights, the work being essentially additive. The task of one professional is to communicate relevant information to another, all the while eschewing jargon. Whatever view one might take of this underlying philosophy, the results fill an obvious need. And the generally high quality of the work serves this need admirably. It follows that economists should have the most to learn from the contributions of their sibling disciplines.

Two not entirely consistent messages are contrived by the anthropologists, who are the chief protagonists for the present purpose. The first is that certain modern economic forms are present to a surprising degree in aboriginal and transitional societies. Elliott Skinner describes the ubiquitous markets and traders in pre-colonial West Africa. A kind of entrepreneurship is discovered by Margaret Katzin in a wide range of activities. Harold Schneider's thesis is that cattle in East Africa served an economic function as well as that of status symbols. These are useful correctives to the primitivist view. Schneider, however, swings the pendulum far. The issue has been aired before in

different contexts, and the late Karl Polanyi and his colleagues have made the point in connection with classical Greece that a swallow of modern economic forms does not make the summer of a market system. Schneider's finding that cattle were a means of exchange and a measure and store of value and that there was considerable orientation to economic gain cannot be accepted as sufficient evidence for "money and a market system which are not different in kind from those of Europe and America" (p. 64). The use of money and a penchant for gain do not necessarily imply individualistic, price-oriented commodity demand and especially factor supply, which are the hallmark of a market economy.

Lloyd Fallers' survey of stratification in indigenous societies serves as a summary of the anthropologists' viewpoint on the theoretical level. In a particularly illumiating way he distinguishes African from "peasant societies" on the grounds of the absence in the former of elite versions of the common culture, largely due to the egalitarianism of corporate descent groups. The general force of his arguments is to downgrade the consciously and separately economic and the significance of modern forms in pre-contact societies. More than that, his major conclusion is that economic structures are submerged by political ones. Insofar as the grounds are the absence of full-time occupational specialization in production, it would seem that political preoccupation could be similarly characterized. Elsewhere, the position rests on labor being the key resource in a land-rich and technologically simple economy. It would seem sufficient to characterize the functions of such social units as "diffuse" without giving pride of place to the political.

Two surveys of landholding, past (Daniel Biebuyck) and present (Paul Bohannan), convey the complexity of land tenure, its embedment in political and social organization, and the inappropriateness of Western concepts of alienability. There remains an ambivalence as to the extent to which land has become a commodity in recent times.

On the whole, the economic essays consist of straightforward summaries of the more recent situation, enriched by some analytical observations and generally stated in as nonesoteric language as the subjects permit. The topics covered include agricultural productivity (Bruce Johnston), real income in West Africa since 1939 (Elliot Berg), demographic trends (H. W. Singer), infrastructure (A. M. Kamarck), the impact of foreign trade and of settlers (William Barber), exports and capital inflows (Walter Chudson), and a somewhat utopian consideration of international aid (Lattee Fahm). The changing labor force is described by the sociologist, Wilbert Moore. Most of the ground is well covered if familiar. Perhaps the most original contribution is Berg's comparison of real income of wage earners and farmers in ex-British and French West Africa. Also, causes and implications of the differences are usefully canvassed.

The final impression created is somewhat anomalous. A picture emerges of African economies, as presently structured, faced with overwhelming problems: a unique dependence on undependable foreign trade and capital inflows, a build-up to demographic difficulties, and so on. At the same time the prospects of social change, both predictively and prescriptively speaking,

are treated with a caution which is disproportionate to the challenge and which also belies the course of recent events in some countries and serious commitments in others. It is left to a political scientist to take into account the radicalness of the African response. James Coleman's comments are understandably centered on the political arena, but he also outlines some of the massive economic programs as well as the economic implications of the ambitions represented by Africanization, neutralism, Pan-Africanism, and a pragmatic socialism. It remains true that economists may continue to learn from their anthropologist brethren about the intractability of institutions, and in this lies the special value of this volume. However, we are no longer unaware of the principle. The interdisciplinary approach which informs this work may now call for attention to the political scientist's sensitivity to the imperatives for change.

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Investment in Australian Economic Development 1861-1900. By N. G. BUTLIN. New York: Cambridge University Press, 1964. Pp. xv, 477. $11.50.

Industrial Development in Australia 1920-1930. By COLIN FORSTER. Canberra: Australian National University, 1964. Pp. ix, 256. 45s. Australian.

The study of Australian economic history is undergoing an impressive advance. Previously represented by a relatively slender body of literature, much impressionistic and some highly tendentious, it has in recent years moved to a new level of sophistication and scholarship, and the formulation and testing of relevant models of economic growth become increasingly possible. Much of the credit is due to Professor Noel Butlin, who has made economic history one of the most fruitful fields of research at the Australian National University's Institute of Advanced Studies. The development of this research interest has been associated with the establishment and expansion of the University's Business Archives, where much of the material used for both of the studies discussed in this review is deposited.

Butlin's essay, a companion volume to his previously published estimates of production, investment, and foreign borrowing,[1] not only draws heavily on these estimates but also makes use in other ways of the mass of material which he assembled in the course of making them. It is essentially a study of three sectors of the economy—the pastoral industry, urban housebuilding, and communications—which absorbed all but a small proportion of Australian investment between 1860 and 1890. There is also an introductory survey of the main features of economic growth in this period and a brief discussion of the relationship between urbanization and the development of secondary industries; but these parts of the book are somewhat peripheral to the main theme. The period is analyzed as a self-contained one: Butlin refrains from

[1] *Australian Domestic Product, Investment and Foreign Borrowing 1861-1938/39* (Cambridge: Cambridge University Press, 1962).

relating the legacies of the late nineteenth century to the economy's subsequent performance.

The stated purpose of this book is to provide "an institutional and historical account of Australian investment and economic development," rather than "a proper economic appraisal of economic growth." If it had in fact been confined within these limits it would have been very dreary indeed. There is, however, no avoidance of economic analysis; on the contrary, an important and forcefully argued analytical theme pervades the whole study. The main discernible consequence of Butlin's intention of avoiding analysis is his refusal to use any but the most elementary statistical techniques. As a result numerous tables and plotted time series must be examined to get impressionistic evidence of relationships which would emerge more clearly and precisely from ordinary correlation analysis.

Butlin argues that sustained investment in the three sectors mentioned above was the result of a decline in the quality of the decisions taken by those responsible for investment outlays. The commitment of capital to these sectors was raised to excessive levels, inflicting on the economy a structural distortion whose liquidation was a major function of the depression of the 'nineties. Initially all three sectors presented very favorable investment opportunities. Highly productive land was available for pastoral occupation and development; an unsatisfied demand for housing existed after the population influx of the 'fifties; and the colonial governments could get adequate returns from railways built under the stimulus of dispersed settlement and the costliness of other forms of transport. By the 'eighties, however, the remaining opportunities, viewed objectively, were much less satisfactory. The pastoral industry expanded into inhospitable regions at a time when the proceeds of wool sales were unresponsive to increased production; it was found that stations in these areas could be made productive only by expensive development, and good money was accordingly sent after bad. Residential building now outstripped the growth of demand and the existence of unoccupied houseroom depressed the building industry throughout the 'nineties. In railway-building, colonial governments, anxious to secure for their own capital cities the advantages of trade with regions close to the colonial borders, embarked on the construction of competitive lines which were uneconomic additions to the total transport network. All of this investment both encouraged and was in turn sustained by the development of financial institutions which directed funds to the sectors concerned but left other industries, notably agriculture and manufacturing, undercapitalized.

The internal distortion of the economy also generated an external disequilibrium, for the prolonged investment boom was accompanied by a large capital inflow that encouraged a high propensity to import but failed to promote a commensurable growth of export potential. However, Butlin believes that the downturn after 1889 (less fully analyzed than one might wish) was due initially to local disillusionment with the results of the investment boom. External influences such as the Baring crisis of 1890 and falling wool prices merely accentuated a collapse which Butlin interprets as the denouement of the previous pattern of growth.

This underlying model is perhaps less satisfactory than many of the subsidiary hypotheses which Butlin develops. It differs from previous interpretations mainly in its emphasis on sectoral rather than general overextension of investment relative to absorptive capacity. An important aspect of this thesis is the view that other sectors of the economy could have absorbed profitably much greater amounts of capital than they received. Surprisingly, the careful argument which this view requires is absent from the book, so that in a quite fundamental respect Butlin's interpretation appears to be unsupported. Perhaps this deficiency will be repaired in the subsequent analytical essay which he intends to write.

Dr. Forster's book contains a number of industrial case studies, relating to the manufacture of motor vehicles, cement, textiles, electrical equipment, and iron, steel and related products, and also provides a more general survey of factors limiting industrial growth. The factors selected for examination are the availability of suitably skilled labor, the supply of funds for investment, and relative wage costs and labor productivity in comparison with those of other countries. Within its limits this inquiry is carried out in a thorough and scholarly fashion. The development of Australian secondary industry still has a very thin literature, and this contribution is the more welcome for that reason.

If one nevertheless finishes this book feeling dissatisfied, the reason is its relatively unambitious scope. Essentially an entrepreneur's-eye view of the problem, it rarely reveals the economist's ability to form a broader and more integrated judgment. This is especially surprising when one remembers the important debate which occurred in the 'twenties about the economic models appropriate to Australian industrialization. The ideas of Benham, Brigden, Giblin, and others have an enduring interest and significance, and they illuminate the interrelationships between tariffs, wages, income distribution, population growth, and industrialization, which Forster does not explore. Tools of analysis that have become more fashionable since that time, such as the external economies concept, should also be highly relevant to Forster's problem, but these too receive little notice. There is clearly a need for a more analytic appraisal of the growth of Australian secondary industry, encompassing a longer period than Forster's. However, the economist who undertakes this task will benefit from the factual material which Forster has assembled.

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The Development of Australia. By J. B. CONDLIFFE. New York: Free Press of Glencoe; London: Collier-Macmillan, 1964. Pp. x, 294. $9.00.

Originally commissioned and distributed as a private report by the Stanford Research Institute to an Australian businessman, this work is now presented to a wider audience as a general diagnosis and prescription for Australian economic development, with a focus on a period fifteen years or so hence. Based as it was upon a four-months tour by Professor Condliffe, accompanied by a geochemist and an industrial engineer, and written up in a

further four months, the report does not claim "to be more than a summary impression gained from a rapid survey of existing data and current debate." The data are presented, however, in the context of a compact and eminently readable introduction to the Australian economy, and the book does not hesitate to make its own contribution to the policy debate. Its conclusions will inevitably be compared with the soon-to-be published findings of the special Committee of Economic Enquiry appointed by the Commonwealth government.

As an introduction to a country whose development problems are attracting growing interest, the book is a valuable addition to a limited literature. A summary of conclusions is followed by chapters covering population growth, assisted migration, decentralization versus urban development, mining ventures, oil discovery, water and power resources, export of manufactures, the balance of payments, transportation, sources of capital, entrepreneurial limitations, national development needs, and applied research. Appendices furnish additional data on selected regions, mineral and water resources, and agricultural development schemes. Illustrative maps and photographs and a subject index are included.

The key to Australian development is seen to lie with manufactured exports, aimed primarily at the U.S. market. Some returns are also expected from pastoral expansion, which in Australia has low labor/capital ratios and is frequently corporately organized, and possibly from further wheat exports, but limited future is seen in general agricultural extension or in remaining an 87 per cent primary-produce exporter. The case against relying on Asian markets is based less upon fear of Japanese competition than upon the supposed inability or unwillingness of poorer Asian countries to import capital goods without corresponding grants of aid, and their determination to substitute local production for other manufactured imports. Servicing growing food and materials requirements in Asia raises uncertainty as to the terms on which this could be conducted and is deemed inadequate as a mainstay of foreign exchange procurement. Whilst forecasting world markets at 15 years remove must necessarily be an intuitive exercise, it is arguable that survival in U.S. markets might prove more precarious than carefully worked-out agreements within ECAFE or under the Colombo Plan, for which type of bargaining Australia has shown exceptional talents. Recent success in exporting quality manufactures to selected Asian markets, as well as in exporting wheat to China, augurs well for similar promotion efforts, provided that relative political stability is maintained. If Korean-type wars proliferate, Australian primary exports should yield exceptionally good returns.

Critical importance is attached in the report to the location of new manufacturing production, which has hitherto preferred proximity to domestic markets, especially the conurbations around Sydney and Melbourne where over half the nation resides. Broader "development effects" are anticipated from decentralized location as part of a program for systematic regional development. Intensive schemes for water and power supply, supported by extensive road and railway networks, are foreseen as likely to attract balanced growth of a wide range of mutually supporting activities. Such programming

(the report judiciously avoids the word "planning") should be drawn up by a National Development Council, "drawn from private citizens representative of farming, business, professional and labour interests, with due regard to regional representation," their recommendations to be implemented by, and after consultation with, both the Commonwealth and the States governments. A university-based Institute of Applied Economic Research is also recommended to help research development possibilities, to advance scientific management, and to reduce what is diagnosed as excessive temerity and lack of judgment of local enterprise in assessing new development opportunities.

It is questionable whether programming by committee is an ideal, or even a feasible, method of assembling a portfolio of development projects or of striking a balance among the multiple objectives to be applied as criteria. The most dedicated group of amateurs could hardly fail to be overimpressed with dramatic hydroelectric or highway proposals, and could not be expected to take an incisive opportunity-cost approach, giving appropriate weights on a national scale to economic, social, and strategic considerations. Surely a National Planning Institute, responsible to the Commonwealth government but sensitive to the needs and ideologies of the various States, is a preferable alternative to a Babel of conflicting interests or simply the facade of a new development agency?

Condliffe's case for regional development is related mainly to the anticipated employment problems of a nation, a third of which is currently below working age, and which is set on encouraging further immigration from Europe. In spite of alleged city congestion, however, it seems certain that full employment could be maintained with less public outlay by improving amenities in areas where both people and businesses prefer to be. The decentralization objective was in fact written into the report by its terms of reference, which were "to investigate the possibilities of attracting settlement to the undeveloped areas of Australia and to suggest a programme of action directed towards realising these possibilities." It says much for the professional integrity of the SRI team that the traditional arguments for closer agricultural settlement, based upon sentimental pride in Australia's "natural advantages" and the irrational belief that an "imminent Asian invasion" will thereby be discouraged, are quickly dispatched, and that it is taken for granted that areas chosen for development "must inevitably be based on access to the sea." In effect, no economic or strategic case seems to exist for inland settlement other than the necessity of extracting and economically processing mineral deposits. Given closer settlement as a political or social objective, however, integrated development of selected regions through a permissive incentive arrangement would seem to be the most sensible "programme of action" available.

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The Economy of the Chinese Mainland: National Income and Economic Development, 1933-1959. By TA-CHUNG LIU AND KUNG-CHIA YEH. Princeton: Princeton University Press, 1965, Pp. xxvi, 773. $10.00.

As a quantitative analysis of Mainland China's economic development dur-

ing the years 1952-59 and a comparison of this development with China's pre-Communist, prewar economy of 1933, this lengthy two-part study clearly warrants description as a major contribution to the literature. Overwhelmingly national income account-oriented, it is uniformly highly technical and analytical in its approach, detailed, comprehensive and well written. In this reviewer's judgment, the authors' conclusions are well founded and likely to influence professional thought on the subject for many years to come.

The 274 pages of Part I include an introduction to the setting of the problem and four substantive chapters which deal with "Domestic Product and Expenditure: A Summary of Data, Methods and Findings," "Pattern of Economic Development: Preliminary Analysis of Findings," "Estimate of Domestic Product by Industrial Origin," and "Estimate of Domestic Expenditure by End Use." The distinctly readable prose receives excellent support from 11 charts and 94 tables. The 496 pages of Part II contain 12 data-laden statistical appendices and a 45-page bibliography, of which 39 pages deal with references in the Chinese language and the remainder list Japanese, Russian, and English source materials.

Selection of the years covered by the estimates and the detail of analysis offered were determined by the authors in accordance with conditions in China in specific years and the quality of the statistical data available. To them 1933 seemed to be a reasonably normal prewar year offering relatively good data coverage for both China proper and Japanese-occupied Manchuria; 1952 was selected as the starting point of the postwar series to eliminate the first three years of the Communist regime (1949-51) when the statistical coverage was meager and even less reliable than for the later years. For 1952-57, detailed estimates of domestic product by industrial origin and domestic expenditure by end use are offered by Ta-Chung Liu and Kung-Chia Yeh; however, the distinctly low quality of the statistical data issued for 1958 and 1959, the period of the "Great Leap Forward," makes possible only rough and tentative adjustments of the data and construction of estimates based thereon. For most of the estimates presented, three alternative sets of price weights are used: 1933, 1952, and 1957 prices.

The authors present six major conclusions with respect to developments during 1952-59. First, net domestic product grew in 1952-57 at an average annual rate of 6 per cent in 1952 prices, 5.7 per cent in 1957 prices, and 4.4 per cent in 1933 prices, rates substantially below the 9 per cent claimed in the official statistics. Implied in the data is an annual increase in per capita product of 2.1 to 3.6 per cent. Second, widely divergent annual growth rates prevailed in various sectors of the economy. At one extreme, for factories, utilities, mining, and construction, they ranged from 17 to 23 per cent (in 1952 prices); at the other, agricultural output increased by only 1.7 per cent. Third, major shifts occurred during 1952-57 in the shares contributed by the various sectors as a result of the forced Soviet-type industrialization program. These shifts were even more pronounced in relation to the 1933 structure of net domestic product. In 1933, modern factories contributed 6 per cent of net domestic product, in 1952, about 9 per cent, and by 1957 had reached 17 per cent. Conversely, agriculture, which in 1933 accounted for 57 per cent, con-

tributed 48 per cent of domestic product in 1952 and by 1957 was down to 39 per cent. During the years 1952-57, significant increases in the shares contributed by the mining, utilities, modern transportation, and government administration sectors occurred, while those of the traditional, technologically static sectors and subsectors such as handicrafts, peddlers, and native transportation declined.

Fourth, in the years 1952-57, the absolute amount of gross domestic investment increased regularly, and a high rate of investment was attained. In 1933, gross investment accounted for 5 per cent of total domestic expenditure. By 1952, it accounted for 19 per cent in 1952 prices and 16 per cent in 1957 prices. Between 1952 and 1956, it increased steadily to a peak of 26.5 per cent in 1952 prices or 23 per cent in 1957 prices. In 1957, it declined slightly to approximately 26.2 per cent and 22.7 per cent in 1952 and 1957 prices, respectively. Fifth, a resource-allocation pattern weighted heavily on the side of producers-goods production resulted in depressed consumption levels. Even though 1952-57 per capita consumption increased at an annual rate of 0.5 to 0.8 per cent in 1933 prices or 1.2 to 1.5 per cent in 1952 prices, by 1957 it was still at least 10 per cent lower than in 1933. These data conflict sharply with the official data which claim a 4.4 per cent annual increase in per capita consumption in 1952-57 and a 40 per cent gain by 1956 over 1936. Sixth, the lack of reliable data for 1958 and 1959 requires that estimates for them be couched in the most tentative and reserved forms possible. The Communist claims of 34 and 22 per cent increases in national income in 1958 and 1959, respectively, are probably grossly exaggerated. The actual increases were at most 14 and 15 per cent, respectively, and more probably a good deal less. Because the margin of error in making estimates for 1958 and 1959 is so much greater than for 1952-57, direct comparisons between them are inadvisable.

To those statistical methods-oriented economists who have followed events in China since 1949, neither the basic tenor of these findings nor some of the data come as a surprise. Substantially similar preliminary findings were presented by Liu and Yeh at the AEA's annual meetings in 1958 and 1960 and were published in each following year's *Proceedings*. What is new, however, are the detailed calculations presented and the complete and final analysis of them. Unfortunately, the statistical problems, the need to elaborate on them, and space limitations prohibit comparison of the details of the Liu-Yeh study with those in the work of W. W. Hollister, Choh-ming Li, and Alexander Eckstein. However, what can be said is that Liu and Yeh are far more critical of the statistical data issued by Peking and are far more prone to make what are regarded as necessary adjustments in them than the others, such adjustments being more frequently of a minimizing than maximizing nature. To illustrate, Li, using the Western concept of national income (which unlike Mainland China's includes numerous services), found the annual growth rate for the period 1952-57 to be 8.9 per cent. W. W. Hollister, using the final-expenditures approach, found the rate for the years 1953-57 to be about 7 per cent. Unfortunately, Eckstein, who combined the value-added approach for agriculture with the factor-shares method for the other sectors, considered

only the year 1952 and did not extend his findings to subsequent years; hence no estimate of the growth rate was made by him. However, he states that the official data for 1952 may be "rather high," as compared to his own estimate, even after allowing for conceptual differences. Clearly the Liu-Yeh study represents a more critical, more disenchanted view of the official Mainland China data than its predecessors.

In closing it should be stated that this study will be of interest to students of Sovietized economies in general and the China scene in particular for many years to come. The methodology and the workmanship command respect and call for imitation.

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Histoire des faits économiques et sociaux de 1800 à nos jours. By ANDRÉ PHILIP. Paris: Aubier, 1963. Vol. 1, pp. 383; Vol. 2, pp. 235.

The author, professor at the Faculté de Droit et des Sciences Economiques in Paris, has developed the present volumes from his economic history course for second-year university students. Dr. Philip's purpose, of course, is to introduce the broad framework and content of economic and social changes in the industrial countries. He begins with the decline of feudalism and the emergence of commercial capitalism and traces the economic and social development of the modern industrial economies of the United States, England, France, Germany, Russia, and Japan.

Philip eschews any theory of history. He attempts to sketch the social and economic facts with a historiography that is neither "scientific" nor stage-ridden. For him nothing is determined. Rather, developments of the past appear to be almost random, without limits or central tendencies. Of course, we know from Carr that even the most unscientific historians must do some implicit theorizing, and André Philip is no exception. He chooses to consider certain institutional, legal and cultural problems in the process of economic development insofar as these influenced the economic structure, the development of a modern economy, and the general level of well-being of the population. From Philip's non-theory, four variables appear unsystematically in most of the descriptions: attitudes toward work generally and entrepreneurship in particular; the money and credit system; the poles of growth; and the significance of trade, especially international trade.

Volume I begins with a concise presentation of feudalism and the manorial economy, traces the emergence of modern industrial societies, and ends with the collapse of the 1930's. Volume II is a mélange of essays on the world economics since the World War—the United States, the USSR, China, underdeveloped economies, United Europe, and France, in that order.

The first 40 pages are a brief but adequate summary of developments to the Industrial Revolution. The impetus to change and to early commercial capitalism comes from the Crusades, the growth of population and towns, which in turn spurred agricultural improvements. Following the disruptions of wars, epidemics, and famines, the process resumes in the sixteenth-century acceleration of the development of capitalism. All of this is well told and in-

cludes the familiar treatment of the discoveries, merchant adventurers, mercantilism as development policy, the consequences of inflation, the appearance of capitalists as a class, and the Protestant ethic.

It is at this point when the author commences to trace the development of the major industrial powers that the methodology creates the greatest weakness of the work—rambling, scarcely connected, barely analytical descriptions. Loosely defined institutions provide a difficult format for the discussion of the economic facts of an increasingly complex and quantitative epoch. Philip rarely rises above the limitations he has imposed upon himself.

The industrial revolution in Britain is cast in terms of the familiar chronicle of Puritanism, agricultural, transportation, and key industrial changes. He follows Ashton without Ashton's detail and accuracy. For example, fuel oil and coal are both cited as fuels in the early nineteenth-century industrialization. The Watt engine is mentioned only in passing.

The author's treatment of nineteenth-century French development is, as one would hope, more thorough. His presentation of child labor conditions and the probable course of real wages is illuminating, as is his analysis of the credit system in terms of large vs. small savers and institutions. However, when he comes to what should be a key element—the growth rate and its determinants—the reader is given only the reference to Jean Marczewski and a few rates of change. Nothing is presented to summarize this ambitious and perhaps monumental study in French quantitative economic history. No reference is made to the volumes already published on population and agriculture in the proposed 12-volume study under Marczewski's direction in the *Institut de Science Economique Appliquée.* This seems an important and curious omission, given Philip's concern with the decline of agriculture.

His treatment of the United States (20 per cent of the first volume) concerns the origins and structure of institutions—Interstate Commerce Act, holding companies, and the CIO. One reads about the details of the New Deal, but nothing of the *economics* of railroads or steel. Partisans of the Innis theory of regional development as applied to the United States will be disturbed to read that exports played only a restraining role in the U.S. economy. The social problems of American development are seen as resulting largely from the expansion of the market and the growing dominance of merchant and financier over the artisan. On the other hand, in Philip's view the concentration movement tended to create the conditions for coincident rapid growth in income, for this consolidation permitted rationalization of production processes, fuller utilization of by-products, and lower prices.

The best aspect of a generally inadequate work is the author's survey of attitudes toward work and of the forms of entrepreneurship. He deftly sketches the evolution of work characterized as punishment, through Luther and Calvin into eighteenth-century France where work attitudes for the *bourgeoisie* became a confident optimism, giving rise to the myth of progress. Concerning entrepreneurship and the role of state and private sectors, it is refreshing to relearn the degree of state direction and enterprise in Germany and Japan, especially in the light of the currency of the view that they are paragons of free enterprise and *laissez-faire* virtue.

With the second volume this reader has the curious feeling that the question of the economic and social facts of development are not the central issue. Rather, the author may be addressing *himself* to the underdeveloped economies now beginning the great ascent and urging them not to make the mistakes and incur the great social costs of either the market economies or the planned economies of authoritarian stripe—and instead to become a viable Third World.

Unfavorable comparisons between the two groups abound. For example, he likens the public service spirit and obedience to authority of the Japanese autocratic capitalists to that of the professional revolutionaries at the beginning of the Russian revolution. Or consider: Japan would be little different under either capitalism or communism, for in either case the heirarchy of social relationships, the concentration of economic power, and the simplicity of the élite and their high marginal propensity to save would have resulted in the same hard life for the mass of population.

With little of value to choose between the paths of development of the United States and the USSR, Philip argues the need for planning generally on a relatively short-term basis, the merits of French planning techniques, the possibility of lessening foreign aid dependence, and the need for developing large middle-level cadres in the developing countries. Thus the countries of the Third World will be less dependent on either bloc and more able to carry out their own transformations. Perhaps, then, *men* could be placed before *things* in the scale of things. If this admirable goal is the author's purpose, then he would spare us anguish through a more direct approach.

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Strukturwandlungen einer wachsenden Wirtschaft. Edited by FRITZ NEUMARK. (Verhandlungen des Vereins für Sozialpolitik in Luzern 1962.) Berlin: Duncker & Humblot, 1964. Vol. 1, pp. viii, 526; Vol. 2, pp. vi, 591.

The *Verein für Sozialpolitik,* an instrument for the early promotion of German social policy, vanished during the Nazi regime. Its resurrection in 1949 constituted a revival only in the form of its organization; its annual meetings are usually devoted to a specific topic followed by extensive discussion and publication. Missing is the original purpose, the social science view of economics, and the preference for the historical approach to any problem. Not only has the scope of economics been substantially reduced, but also the preference for the deductive approach has become so prounounced that a well-known economic historian, at the end of the meetings in 1962, protested against the excessive emphasis upon laws and lamented the decline in the skilled use of the historical method in economic affairs.

Considerable effort is spent in the preparation and execution of these conferences. For the topic on structural changes in industrialized countries, 14 special subjects were selected which led to the preparation of 28 treatises that were made available prior to the conference. While the authors of these pa-

pers merely summarized their views, most of the subsequent discussants gave evidence of having digested the written papers. A reader of these two volumes which contain the written papers as well as the statement of the discussants, gets the impression that these meetings allow for considerable cross-fertilization of ideas among participants.

A particular role is played by the scientific director (*wissenschaftlicher Leiter*) of a conference, whose task is to supply the theoretical framework for the speakers. In defining structural change, Mr. Bombach sharply distinguished historical from statistical and econometric approaches to such change. Denying the validity of the first, the main characteristic of the econometric approach was seen in the constancy of its parameters. In dealing with the quantities of values, volumes and prices over time, the statistician is interested in finding their particular interrelationship but he is not concerned with particular levels or mere cyclical variations. While not binding the speakers to these two recommended approaches, the expectation was that all would be guided by them. As a matter of fact, not more than half accepted this recommendation, while the others either could not find relevant statistical data or they deliberately emphasized the qualitative aspects of structural change. In speaking either of a sequence of stages or long-term tendencies, these latter contributions were very much akin—in their method—to a previous publication on the same topic, prepared and edited by Bernhard Harms in 1928.

The papers of both the quantitative and the qualitative approaches are of unequal quality. The thesis that foreign trade is a declining percentage of national product could have turned out differently if continuous statistical series had been used and allowances had been made for the changes in political and economic border lines of states. The three periods in world trade in a statistical study were no more precise than the asserted historical phases in the changing composition of the capital stocks during the last 200 years. One classification lumped manufacture, mining, and agriculture together into "production," and all other outputs into "services" for the sole purpose of attributing the increased qualitative improvement of employees exclusively to the service industries. Although there are some impeccable papers among these studies, others—of both approaches—could have been considerably improved.

The projections of both approaches also seem to suffer from a high degree of uncertainty. In the debate over the future chances of newcomers entering German trade, one opinion asserted a decline of free entry because of the rising capital requirements, while the other view believed that newcomers would certainly break in as soon as the restrictive concerns had reached their phase of vulnerability. Neither of these qualitative interpretations specified the conditions under which these particular forecasts could be correct. Some of the statistical estimates suffered from a similar degree of uncertainty. In a study of German agriculture, a law on the constant size of farms was derived from the difficulties in transportation and supervision which prevent an increase in the cultivated area. Overlooked was the obvious qualification that this limit is true only if transportation is by animals and operation centers on one cluster of buildings. Yet this "law" became the basis for a statistical forecast: Given

the projected demand and a decline of the annual growth rate from 3 per cent in 1960 to 1.5 per cent in 1975, the competition from the other Common Market countries will reduce the number of full-fledged farms from 800,000 to 600,000 in West Germany. Of course, if the more efficient farmers should increase their size of operations because of a shift to tractors and cars, the surviving number of German farms could be only 500,000 in 1975. The uncertainty as to what has to be taken as a constant parameter foiled this particular statistical estimate. This pitfall of the econometric approach can hardly be avoided by the dictum that any set of decisions will turn out to be correct as long as the selected over-all policy for the structural change is implemented consistently.

The discussion on the goals for a deliberate structural policy produced an interesting difference of opinion. Having full confidence in the reliability of their sectoral projections, the determinists suggested that their goal of optimum rationality should be implemented by a plan for the whole economy. The believers in a competitive market economy, however, insisted (a) that their goal of an ideal system has to be the fountainhead for all policies, (b) that structural policies have to be limited to particular sectors, (c) that all restructuring policies have to aim at increasing competition. The prevailing climate of opinion was such that the planners assured their opponents of having thought only of indicative planning which would contain no directives for private firms. The victory of the *Ordnungspolitik* was further enhanced by the concluding remarks of the President who expressed his satisfaction that neoliberalism had become the universally accepted ideology of the professional economists.

These disputes on the reliability of statistical estimates and the choice of goals for a structural policy are very instructive, since they reveal the three inherent defects of modern structural analysis. Too frequently, structural projections cannot reveal the nature or the coming of an immanent structural change so that scientific prognoses of such changes are not possible. The recommended goals for structural policies, derived from optimal rationality, are usually not acceptable to the public whose preferences are frequently more influenzed by the particular interests or ideologies of the most influential groups. Finally, the structure of economic and political power was completely excluded from the analysis so that indicators for the direction of expected structural changes could not be ascertained. These largely unavoidable defects do not recommend structural analysis as a reliable method for the study of actual and potential economic systems.

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Statistical Methods; Econometrics; Social Accounting

Econometric Model Building: Essays on the Causal Chain Approach. Edited by HERMAN O. A. WOLD. (Contributions to Economic Analysis XXXVI). Amsterdam: North-Holland Publishing Co., 1964. Pp. xiii, 419. $12.80.

The first, and focal, chapter of this volume is Herman Wold's "Forecasting

by the Chain Principle," which brings us up to date on his thinking on multi-equation models. For some years now, Wold has made a case for the use of least-squares estimation in such models. Underlying this case was the argument that models should have a causal interpretation; a causal interpretation demands a recursive rather than a simultaneous structure; and least-squares estimation is justified in a recursive structure. Many econometricians have been unable to accept without qualification each of the steps in this argument, and at many points in the ensuing debate the discussants seemed to be at cross-purposes. Nevertheless, the years did lead to a clarification of the respective positions. The present piece, it seems to me, takes a considerable step in this constructive direction.

The central concept in this chapter is that of an *eo ipso predictor* (EIP, say) which is simply the conditional expectation function of a random variable. Consider $y = \beta_1 x_1 + \beta_2 x_2 + u$: If the conditional expectation of y given x_1 and x_2 is $\beta_1 x_1 + \beta_2 x_2$, then $\beta_1 x_1 + \beta_2 x_2$ is an EIP of y. Now substitution of one linear EIP into another yields an EIP (Theorem 4): thus if the conditional expectation of x_1 given x_2 is $\gamma_2 x_2$ then $(\beta_2 + \beta_1\gamma_2)x_2$ is again an EIP of y. The implication is that, if a model is formulated as a chain or recursion of EIP's, successive substitution continues to yield EIP's. Thus in a recursive model both the structural and reduced forms consist of EIP's, while in a general interdependent model the reduced but not the structural form consists of EIP's. As Wold notes, the disturbance to an EIP is necessarily a zero mean random variable uncorrelated with each of the conditioning variables (Theorem 1). Therefore under general conditions least-squares regression provides a *consistent* estimator of an EIP (Theorem 2). Thus in a recursive model both the structural and reduced forms are amenable to least-squares estimation, while in a general interdependent model the reduced but not the structural form is so amenable.

This constitutes a fresh case for the convenience of recursive models with an emphasis on the flexibility they possess for a variety of prediction situations. Also presented is a rather fresh case for the desirability of recursive models which rests on an analogy between recursiveness between time periods and recursiveness within time periods. Other sections of this chapter also make an important contribution to clarifying our views on multi-equation models. Of particular interest is the EIP interpretation of structural equations in an interdependent model, obtained by replacing endogenous variables by their reduced form EIP's—a device in the spirit of Theil's two-stage least-squares estimation method.

Wold's chapter does suffer from characteristic features which will be familiar to readers of his remarkable treatise *Demand Analysis* (New York, 1953). Concepts are often named without being clearly specified (the "chain principle" is never actually defined); some key pieces of literature are ignored (e.g., Hurwicz's 1950 article on prediction and least squares); and within a rather formal structure there are disconcerting logical gaps (recursive systems are said to have the desirable property of being "minimum-delay": while this property is defined in another chapter, and references are provided, the only clear explanation of its desirability I have been able to

find in this book is on its dust jacket!). It is to be hoped that these irritants will not distract from the value of this presentation of the current thinking of a very distinguished and stimulating economist.

Nor should the attention given in this review to the first chapter distract the reader from the wealth of other material in this volume. Some of this bears quite closely on the themes introduced by Wold. Falling in this category are three pieces by Ejnar Lyttkens: In Chapter 4 he derives asymptotic standard errors of least-squares estimators of EIP regressions in the presence of both lagged endogenous variables *and* autocorrelated disturbances. In Chapter 6 he attempts to formalize an earlier suggestion of Wold's for determining causal direction by purely empirical means—unfortunately, the rationale of the suggestion is not given here—and in Chapter 9 he provides some instructive notes on econometric models. The two long chapters (2 and 3) by Enders A. Robinson discuss in considerable depth the modern theory of time-series analysis—which is coming rapidly to the fore in econometrics—aspects of which underlie Wold's view of multi-equation models. The first of these, at least, is a beautifully written exposition. Also George Stojkovic in Chapter 13 presents empirical studies of U.S. agriculture which are relevant to a comparison of the recursive and interdependent approaches.

The remainder of the volume has less connection with the causal chain approach. In brief Chapter 5, A. Gadd and Wold propose a new statistic—the "Janus quotient"—for measuring prediction accuracy. In a similarly brief chapter (12), Stojkovic and Wold present a "force-field" interpretation of Cournot's competitive-market model. There are two empirical studies of Swedish agriculture: In Chapter 10 L. Hjelm and E. Sandqvist estimate cross-section production functions and deduce profitability implications; in Chapter 11 Odd Gulbrandsen provides an interesting decomposition of price fluctuations. Of considerable interest also is the treatment of monopolist price strategies: Sten Thore presents a theoretical specification in Chapter 7 and then, with Frithiof Billström, reports on several simulation experiments—this Chapter 8 is a model of lucid exposition.

All of the chapters in this handsomely printed volume are based upon reports presented in the research seminar of the University Institute of Statistics, Uppsala. They illustrate in an impressive fashion the range of activity of the Swedish econometricians across mathematical economics, statistical methodology, and empirical research.

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Nonlinear and Dynamic Programming. By GEORGE F. HADLEY. Reading, Mass.: Addison-Wesley Publishing Co., Inc., 1964. Pp. xi, 484. $10.75.

The deluge of new theoretical concepts and improvements in computational techniques associated with linear, nonlinear, and dynamic programming is probably viewed ambivalently by the average economist. There is the realization that many of these new results throw much needed light on questions of efficient allocation and valuation of scarce economic resources—which are at

the center of traditional economic analysis. On the other hand, there is the well-founded feeling that it is virtually impossible to keep up with the new literature unless one is prepared to become a programming specialist. Professor Hadley's book provides a welcome potential escape from this dilemma.

The reader is generally advised to have some background in linear programming and linear algebra, such as is provided in the author's earlier two texts. Although a summary chapter on mathematical background is provided, prior mathematical work would really be necessary for those readers not in the genius category. The level of mathematical sophistication required is somewhat above that found in S. Vajda, *Mathematical Programming*, but is certainly accessible at a reasonable price. The work is written as a textbook with many problems at the end of each chapter.

Although Hadley protests that the current state of programming theory does not lend itself to a unified treatment, he does manage very well to take traditional optimization techniques associated with calculus—with which economists are familiar—and relate them to and use them with the new programming methods. Chapter 3 is entirely devoted to classical optimization problems with equality constraints, including a valuable extensive treatment of Lagrange multipliers and duality. In a later chapter, he develops the Kuhn-Tucker theorem in some detail. However, the author emphasizes that even for those actual problems of any complexity where classical marginal analysis may be formally applicable, the classical methods are usually not much use from a computational point of view, having only descriptive value.

The author discusses those kinds of nonlinearities—involving convex and separable objective functions and constraints—which can be handled by nonlinear programming methods. He gives approximate techniques for solving such problems, with an extensive discussion of the relative costs involved of using linear approximations to nonlinear problems. As an interesting illustration of a problem relevant for traditional economic analysis (of the many he considers), he treats the so-called "fixed charge" case in some detail. Consider an economic process which uses variable inputs in the production of a given output but is also subject to a fixed charge (overhead cost) for any level of output greater than zero. Various other equality and inequality constraints and output possibilities may exist. The author sets up a solution using a modified simplex procedure and later shows how it can also be formulated as an integer programming problem. He continually demonstrates how different mathematical structures can be applied to the same problem and, conversely, how seemingly different physical problems may have the same mathematical structure.

The general question of sequential decision-making, when some of the parameters facing the decision-maker are stochastic, is treated extensively and is linked to the newer techniques of dynamic programming. The author emphasizes that almost all real problems—such as inventory control, loading, equipment replacement, and capital budgeting—are stochastic and sequential in nature and only by approximating can they be reduced to simpler deterministic and nonsequential formats. However, he does show that the mathematical structure of dynamic programming can be developed independently of any

time-sequence applications, and this structure is neatly linked to the classical calculus of variations. Gradient methods for solving nonlinear problems are also investigated.

In summary, Hadley has produced an extensive, well-organized, and unified view of the most important recent developments in nonlinear and dynamic programming.

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Statistics, an Introductory Analysis. By TARO YAMANE. New York, Evanston, and London: Harper & Row, Publishers, 1964. Pp. xxiii, 734.

This book is mainly intended to serve as a text for an introductory course in statistics for students in business and economics. It is composed of a preface, 23 chapters, and a separate booklet of statistical tables designed for convenient use in exams. According to the author the first 15 chapters are designed for a one-semester lower-division undergraduate course, and the remainder for an upper-division or beginning graduate course. A brief introductory chapter is followed by three chapters dealing with the traditional description of frequency distribution, central tendency, and dispersion. Chapters 5 through 10 contain basic concepts of probability, the normal curve, sampling distribution, estimation, and classical statistical inference with a brief digression into decision theory in Chapter 9. Chapters 11 through 15 cover index numbers, trends, seasonal variations, cyclical fluctuations, and simple regression and correlation. The following six chapters discuss various probability distributions such as the Poisson, the Chi-square, and the F-distribution, and their applications to statistical inference. Multiple linear regression in Chapter 22 is followed by a final chapter on further topics in time-series analysis.

The book is generally oriented toward the understanding (on a nonmathematical level) of fundamental concepts rather than a "cookbook" approach to statistics, and it has several novel features which are not usually found in other texts in this area. There is an especially good exposition of different approaches to the interpretation of probability in Chapter 5; a discussion of desirable properties of different estimators, including the maximum-likelihood estimator in Chapter 10; a well-balanced treatment of derived probability distributions and their applications to various problems throughout Chapters 17 through 21; and a presentation of the notion of random variable and mathematical expectation in Chapters 5 and 16, and even a topic in decision theory in Chapter 9. Example problems throughout the text are clear and computationally simple enough for a reader to follow easily without becoming bogged down in a "jungle of calculations."

One cannot help but agree with the author's contention that the approach to statistics should be geared to understanding the manner of logical thinking and reasoning rather than to a mere acquisition of mechanical techniques for handling statistical problems. The reviewer, however, feels that although the author has made some advances in this direction, a more unified treatment of fundamental concepts could have been achieved without necessarily introduc-

ing more difficult concepts. The author chooses to introduce the concept of statistical independence, random variable, and mathematical expectation at an early stage (pp. 107-12), but unfortunately fails to take advantage of this knowledge in explaining that the population mean of the sum of independently distributed variables is the sum of the population means (p. 122) or that the transformation for standardizing a normal variate leads to a standard normal variate (p. 118). The difficult part for beginning students is not the latter but the former, especially without an illustration of its usefulness.

After an introduction of the theorem that the sum of independent normally distributed variables is itself normally distributed (p. 122) and the assumption that the regression disturbance is normally distributed (p. 372), the author could easily (in the simple regression analysis in Chapter 14) have extended his discussion to explain why the sampling distribution of the estimated regression coefficient is normally distributed. A more unified approach to data collection and statistical methods would have been achieved if the principles governing data collection and the reality of the basic assumptions required were discussed in the chapter describing the various methods employed. No mention is made, however, of the implications of the required assumption (especially for the use of regression techniques in Chapters 14 and 22) in the light of either available cross-section and time-series data or some other real data that may be collected in business and economic research.

Perhaps more disappointing is his outright omission of basic assumptions in the discussion of multiple regression in Chapter 22. Also, the usual dummy-variable technique in multiple regression is not included. The principle of maximum-likelihood estimation could have been brought in more clearly by using a graphical presentation of simple likelihood function instead of an example of a discrete choice of likelihood with a final statement that the knowledge of calculus is required (pp. 245-51). There are a number of lapses in defining statistical terminology. For example, estimator is defined as an actually computed statistic (p. 237) rather than a function or a rule of combining a sample; consistent estimator is described as an estimator that approaches the parameter as the sample size increases when, in fact, this should be called the asymtotically unbiased estimator (pp. 241-42); and the term "endogenous variable" is used for "exogenous variable" and vice versa, though this may be a typographical error (p. 728).

Despite the weaknesses mentioned, the book can be recommended as a text for a comprehensive course in business and economic statistics if a competent instructor adds flesh to the conceptual and logical bones of the author's exposition.

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Economic Systems; Planning and Reform; Cooperation

The Economics of Soviet Planning. By A. BERGSON. New Haven: Yale University Press, 1964. Pp. 394. $7.50.

This is the fifth of the Yale Studies in Comparative Economics and is an

important contribution to our understanding of the functioning of the Soviet planning system. Professor Bergson seeks to submit this system to the test of efficiency criteria, to see how far it conforms to the principles of economic rationality. He applies, in his own words, "a familiar body of analysis," based on Western value and welfare theory, to which he has himself made a notable contribution. Soviet procedures, decision-making, organization are judged by reference to the attainment of an "economic optimum," the latter being defined uncontroversially as the principle that "with available resources, values sought must be achieved to the maximum degree feasible." He is careful to stress that these values could relate to planners' preferences or to consumers' preferences, and he grants that the former might be preferred when choices are made between present and future (i.e., about the volume of investment). He also rightly points out that, in the disposition of resources available for consumption, consumers' preferences ought to be the determinant, even from the standpoint of the planners.

One of the main problems of writing a book on the Soviet economy is that there is too much to say. It is therefore right to select a unifying theme and to survey the economy by reference to it. This simplifies the task of deciding what to leave out and concentrates the attention of the reader on those aspects of a complicated and often confusing system which bear on the selected theme. Bergson's approach is thus a very proper one and affords a number of valuable insights into the specific characteristics of Soviet planning. As with all possible approaches to so complex a problem, this one is not without certain disadvantages. Thus the description of the apparatus of planning is highly condensed; administrative and organizational matters are referred only to the minimum extent required to set the scene for the analysis of resource allocation. A more systematic account would have led to a clearer view of the very large number and overlapping functions of the various planning and control organs, and this would have brought more vividly into relief the sheer muddle and confusion inherent in the existing organization of Soviet planning. Not, of course, that Bergson fails to mention the fact that planning organs are overburdened, and he cites a great deal of convincing evidence about irrationalities of many kinds. In some particularly successful chapters—on the management of agriculture, choice of technology and capital formation—he presents with subtlety and thoroughness the relationship between the institutional structure and planning methodology. Taken as a whole, the book will undoubtedly provoke much thought and will add to our understanding both of the Soviet economy and of the tools of economic analysis. One wishes that it were written in a more relaxed style. Such phrases as "several kinds of deliveries to the government, including chiefly two" (p. 185) ought to cause even economists to wield a blue pencil.

It is part of the merit of Bergson's book that it sheds light on the validity and the limitations of efficiency criteria. No country or system lives according to the formal principles of welfare economics, and our author knows this well. The use of such criteria is nonetheless justified insofar as adherence to or departure from such efficiency rules might be an indicator of relative "economic merit," to use Bergson's words. But in this respect one of his efficiency rules

seems singularly inapt, even as an expository device. His welfare theory leads him to reject all forms of income taxes and sales taxes, turnover taxes, etc. Leaving aside all argument as to the theoretical basis for such a view, it must surely lead to the unhelpful conclusion that, say, Libya is closer to an optimal structure of revenue than either the United States or the Soviet Union. To use such criteria is to depart too far from any conceivable reality.

The other efficiency rules are not open to this kind of objection, but they cannot be described as uncontroversial. They are essentially static in character. The word "dynamic" is mentioned once (p. 269). Should it not have been further explored? The entire literature on economic development is virtually disregarded. Yet the whole issue of what constitutes economic rationality in the context of growth, in East and West alike, raises issues of great complexity which are not effectively encompassed by traditional welfare theory. It is true that Bergson does make it clear that he regards the tempi of Soviet growth as relevant to the assessment of economic merit of the system. However, growth can be achieved at grossly excessive cost. Bergson vividly demonstrates that Soviet planners often made arbitrary decisions based on no economic criteria at all. Developing countries presumably wish to avoid waste and resource misallocation, while continuing to grow fast. It will not help them greatly if we refer them to criteria which they would regard, not without reason, as irrelevant to their problems.

A rather different point may be the cause of some perplexity. Though with many reservations, Bergson tends to see a logical pattern in Soviet prices, and to discern in them perhaps more economic meaning than they in fact possess. Thus on page 281 it is stated that prices of consumers' goods (with many exceptions, *bien entendu*) are "normally more or less proportionate to costs as estimated." Surely this is not the case either in theory or in practice. This view gives rise to another questionable interpretation. On page 67, the (correct) proposition that the "system's directors" have "supplanted household by planners' preferences" is proved by reference to the fact that retail prices have often diverged from clearing levels. But such an argument would seem to suggest that, if prices had been at clearing levels, household preferences would not have been supplanted. This would only be the case if retail prices were proportionate to costs, which they were and are not. Oscar Lange, in his well-known prewar piece on the economics of socialism, defined with clarity and precision a situation in which planners decided what to produce and then so varied prices as to clear the market. He rightly took this to be a manifestation of planners' and not of household preferences, adding (wrongly) that such a situation would not be long tolerated in a socialist society!

A fascinating final chapter discusses the relative economic merit of the system. It is full of good things, and one only wishes that it were longer. Much of it is devoted to an analysis of productivity comparisons, and it is to be hoped that the readers will pay heed to Bergson's own warnings about the complexity of the factors involved in comparing efficiencies. Material resources, historical traditions, climate, soil, human character, all make a difference to the end result. Obviously the superior productivity of United States as against Soviet (or Italian or British) industry does not necessarily

tell us anything about the superiority of the U.S. *system*. With all this Bergson clearly agrees. Yet so much space, in this chapter and in the appendices, is devoted to productivity comparisons with the United States that many readers may well come to believe that such comparisons provide decisive evidence of "merit."

However, to repeat, this book is full of valuable insights on a great many aspects of the Soviet system, and the fact that it also raises some controversial issues is entirely to the good.

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Business Fluctuations

Sovremennyi kapitalizm i ekonomicheskie krizisy (Contemporary Capitalism and Economic Crises). By E. S. Varga. Moscow: Academy of Sciences of the U.S.S.R., 1962. Pp. 504.

The title chosen for this volume is perhaps a little too synthetic to provide an accurate description of its contents. Hence the reader needs to be warned, at the outset, that the present volume does not specifically concern itself with the nature of the business cycle within the capitalist world during the 1960's. Nor will the reader find in this volume, as he might expect on the basis of the title, 500 pages of the recent writings of the Hungarian-born economist Eugene Varga, who rose to prominence as a writer and editor in the field of international economics within the Soviet Union, after the collapse of the communist revolution in Hungary in 1919, and who died in Moscow in October 1964 at the age of 85. Only the excerpt covering the last 100 pages of the present volume of "selected works" embodies an exemplar of his recent work. This particular essay was originally published in 1961 under the heading of "Capitalism of the Twentieth Century."

While the other selections are drawn from a wide range of his earlier work dating back to the year 1927, it is relevant to note that Varga is most generously represented in the present collection by his more militant literary output of the 1930's. His essays from the latter period contribute four out of the nine selections and occupy over half the total number of pages of the present volume.

By contrast, the controversial postwar study that has become established as Varga's best-known single work abroad, namely his "Changes in the Economics of Capitalism as a Result of the Second World War," originally published in 1946, emerges so severely pruned in the present collection as to be beyond recognition. The editorial process in fact has been so drastic as to leave no trace of the several unorthodox ideas of the study that had brought down upon him the wrath of the party bureaucracy, stripped him of his institute (The Institute of World Economics and World Politics), of his theoretical journal, and proceeded, despite his official recantation in March 1949, to condemn him to obscurity for about one decade. He was fully rehabilitated only in 1959 on the occasion of his eightieth birthday and received the coveted "Lenin Prize" four years later.

As reproduced here, the excerpt from the 1946 volume covers 65 pages, out of the original 320, and by limiting itself to the first five of the original eleven chapters manages to evade the whole issue of the "changes" in the economic structure of the major capitalist nations generated by the wartime policies in the West (the heightened sense of social responsibility, the improved diet, the impact of the welfare state on foreign trade, the growing economic maturity of the former colonies, etc.).

When he was permitted to return to the fray, however, Varga appears to have left behind him whatever doctrinal doubts he may have temporarily entertained with respect to the essential character of the capitalist method of production and social organization. His 1961 volume, reproduced in full in the present collection, preaches the same kind of fierce, fundamentalist Marxist doctrine of the moral iniquity and inevitable doom of capitalism that breathed from his imprecatory prose during his most productive period, namely the 1930's.

As a competent, if hostile, observer of economic conditions in the West, Varga cannot, of course, fail to report that Western capitalism has changed in regard to "some of its external manifestations" and, above all, that it has become more productive, especially since the end of World War II. This massive increase in productivity, however, does not seem to be sufficient cause to make him alter his basic doctrinal appraisal of capitalism. To him, productivity itself is not sufficient to save the "doomed" capitalist order. "Despite the relatively great growth of production and the enormous accumulation of capital in the more developed capitalist countries," he concludes, "the capitalism of our days is in many respects not only politically but also economically weaker than the capitalism of the beginning of this century" (p. 478).

To adduce evidence in support of this sweeping thesis, Varga draws up a multifarious list of the ailments afflicting present-day capitalism. First on his list of "basic contradictions" is the fact that the richest capitalist country, namely the United States, cannot employ all its workers. Another source of weakness, as he sees it, is the "chronic agrarian crisis" (of overproduction) in the West. He sees trouble ahead, furthermore, as a result of the continued agrarian overpopulation, especially in Italy and Spain. There is also, in his view, further proof of the "tottering character of the economics of capitalism" in the disappearance of the erstwhile "single capitalist market" as a result of the emergence of a "parallel market" under the aegis of the communist powers of the world.

Nor is this all. As further evidence of the increasing weakness of the capitalist world order Varga also cites: the uneven distribution of the world gold supply; the absence of completely free mobility for capital; and the device of selling goods on a deferred-payment basis, brought on by the "narrowness of the market," a device that draws in advance upon the future earnings of the consumer. Furthermore, he sees still another clear portent of the coming of doom in the factor of inflation, by means of which "the monopolies are carrying out an offensive against the living standards of the toilers" (p. 480).

Varga concludes his analysis of the evidence of the steadily ebbing strength of the capitalist system with a consideration of defense expenditures. "Mili-

tarism," he asserts, "is the most graphic proof of the stagnation of contemporary capitalism." Here he sees not only a symptom of economic debilitation but a proper arena for the inevitable social struggle within the world capitalist order. In this arena, he declares, "monopoly capital, which finds its most profitable business in military production," comes into open conflict "with the demands of the people to discontinue the 'cold war' and to carry out genuine disarmament."

In his last essay, in particular, Varga attempts to establish a theoretical link with the analysis of the parasitic, moribund character of the capitalism of the twentieth century provided by Lenin in his "Imperialism: The Highest Stage of Capitalism" written in 1916. Nothing that has happened since then, he asserts, "can possibly halt the historic process of the crash of capitalism" (p. 468). In keeping with Lenin's thesis of the dominant role of "finance capital," he argues that the power of the financial oligarchy has continued to increase during the century. But if the economic power of finance capital may not be as easy to quantify as it was in Lenin's day, it is only because "the method of its rule over the capital of others has become more complex and veiled" (p. 474).

At the same time, however, Varga's own research is sufficiently systematic to bring to the surface the kind of data which show that industrial capital has become "more or less independent of the banks" during the past several decades. But he dismisses this particular trend as "a question that is more and more losing its significance, inasmuch as the same narrow group of the financial oligarchy dominates to an ever greater degree both the banks and the industrial monopolies" (p. 477).

On the whole, the present volume was evidently intended by the relevant authorities in the USSR as a monument to the memory of a loyal servant to the cause of orthodox Marxism, Soviet style. In the same vein, his own close associates chose to describe him in their collective obituary (*Mirovaia Ekonomika i Mezhdunarodnyie Otnosheniia*, November 1964, p. 156) as a "veteran of the international workers' movement." Probably, too, Varga himself would have been the first to agree that, by his own lights, his mission as a writer was not to interpret the social order of the West but to change it.

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Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

Modern Banking (Sixth Edition). By R. S. SAYERS. Oxford: Clarendon Press, 1964. Pp. xi, 330. $4.80.

There are two ways of looking at Professor Sayers' sixth edition: from the point of view of its immediate predecessor, the fifth, and in comparison to the *Radcliffe Report*. The fifth edition was published in 1960. Sayers obviously did not then have the time to undertake a major revision of his classic work along the lines he himself had laid out for the *Radcliffe Report*. The fifth was therefore limited to a restructuring of two chapters on the art of central

banking and to an addition of a brief chapter-by-chapter outline of the Radcliffe Committee's *Report,* including an even briefer description of its *Minutes and Memoranda of Evidence.* No attempt was made to summarize the findings of the Committee in the fifth. The sixth edition, however, fully reflects the impact of the *Radcliffe Report.* For all practical purposes a completely new book has been written.

The first six chapters are more concerned with the mechanics of commercial and central banking and the technical and institutional findings of the *Radcliffe Report*—although the emphasis on liquidity throughout these chapters reflects the theoretical underpinnings of the *Report.* The major change in the sixth is to be found in Chapter 7, "Other Financial Intermediaries," and in Chapter 10, "The Process of Creation of Credit and Financial Intermediation." These two chapters make up the theoretical core of the book. Sayers' presentation, however, is rather matter of fact. Nonbank financial intermediaries are introduced, and their effect on the flow of credit considered. Sayers is concerned with changes in the liquidity structure of the economy and their implications for monetary control over the flow of credit. But nowhere in Chapter 7 is a systematic, empirical evaluation given of their *actual,* as opposed to their *potential,* threat to the monetary and credit control functions of the central bank. Chapter 10 carries the argument further by examining the process of credit creation (in which, of course, all financial intermediaries have a role to play), and its relation to the saving and investment processes. The wealth and liquidity aspects of financial claims are brought directly into the analysis, and an institutional complex is introduced where financial claims of infinite liquidity gradation are available. The next section considers the impact of changes in financial claims and is, in turn, followed by a section on the leaks to be found in the process of credit expansion. The writing style of Sayers is generally of a high caliber, but in this particular chapter it is as tight as the analysis it describes. A few schematic equations would have added materially to the clarity of his argument.

The leakages to credit expansion are classified by Sayers into two broad categories: "(1) leaks that result directly from decisions of economic units to redistribute their financial claims, and (2) leaks that result from an increase in income induced by the expansion of financial claims." The multiplier effects of an expansion of financial claims are linked directly to the proliferation of nonbank financial intermediaries. In Sayers' words:

> This increase in the variety of financial intermediaries is of course one of the main characteristics of the financial development of an economy. As the financial intermediaries develop, the *possibilities* [*sic*] of multiple expansion of credit increase: the economc units of all kinds become more liquid (p. 252).

Again, there is a failure to distinguish the actual from the potential. To American readers this would be an interesting issue for monetary policy. Our own experience has been that offsetting changes in income velocity have *not* been the result of nonbank financial intermediaries (though the possibility has always been there). Rather, the explanation is to be found in the dump-

ing of short-term Treasury bills by commercial banks and nonfinancial corporations. It seems that Sayers is more intent on merely stating the possible implications of the Radcliffe findings than in proving their applicability. Virtually no data are provided and no references are made as to the availability of the data either in the *Report* or in the *Minutes and Memoranda of Evidence.*

Given the heavy emphasis by Sayers on the role of liquidity, credit (as opposed to the narrower concept of money), and the role of nonbank financial intermediaries, one would have expected some acknowledgement of debt to Professors Gurley and Shaw. I regret to report that the current volume under review follows in the footsteps of the Radcliffe Committee, which also failed to pay its respects to the Gurley-Shaw thesis. Indeed, except for four very minor references (footnotes on pp. 78, 99, 137, and 271), there is not a single reference in Sayers' book to anyone else's work in the field of money. And this is where the major disappointment with the sixth edition comes in. One would have expected (as I, for one, did) that he would have made some attempt at a more cohesive and theoretical presentation of the liquidity thesis accompanied by a systematic reply to the many critics of the *Radcliffe Report.* No such attempt was made.

On one point, however, I sense a retreat from the *Report.* The sixth edition amply reflects the Radcliffe Committee's conclusion that "the factor which monetary policy should seek to influence or control is something beyond what is known as the 'supply of money.' It is nothing less than the state of liquidity of the whole economy." From this conclusion, the critical problem arises as to the *forms* of monetary control. Should they be selective or general? It would seem to follow from the liquidity thesis that selective controls over nonbank financial intermediaries would be unavoidable. Yet the *Radcliffe Report* found them unnecessary except for extreme situations. The reason for this fortuitous state of affairs can be attributed to the interrelation between the interest-incentive and the liquidity-incentive effects of the *Radcliffe Report.* By open-market operations over the full maturity spectrum of government securities the locking-in effect permits the central bank to exercise indirect, but nonetheless effective, control over the entire range of nonbank financial intermediaries (cf. the *Radcliffe Report,* pp. 131-34).

I have always felt that the Radcliffe Committee's substitution of the notion of general liquidity for the narrower concept of the supply of money was a step in the right direction. But I have also felt that in so moving, the Committee lifted the lid of a Pandora's box and seized Hope on the wing as she emerged in the form of the locking-in effect. The Committee was quite daring and innovating in raising the liquidity issue. However, it quickly took flight from the implications of its position and retreated back into manipulations of the interest rate structure. The *Radcliffe Report* was unequivocal in stating its position:

> (T)he further growth of new financial institutions would allow the situation continually to slip from under the grip of the authorities. The fact that operations on the structure of interest rates do, for institutional reasons, change the liquidity of financial operators throughout the econ-

omy should make it possible to avoid any such complex of direct controls (p. 134).

The retreat of the *Report* from the implications of its own liquidity thesis has been turned into a rout in the sixth edition of Sayers' book. On page 70 he discusses the relationship of the Treasury to the Bank of England and quotes the first two sub-clauses of Clause 4 of the Bank Act of 1946, viz.:

(1) The Treasury may from time to time give such directions to the Bank as, after consultation with the Governor of the Bank, they think necessary in the public interest.

(2) Subject to any such directions, the affairs of the Bank shall be managed by the court of directors in accordance with such provisions (if any) in that behalf as may be contained in any charter of the Bank for the time being in force and any by-laws made thereunder.

In short, the Governor of the Bank of England has no power of veto over Treasury decisions affecting it. The only guarantee is that he will be consulted. The ultimate responsibility is the Treasury's. Furthermore, in its relation with the banks the Bank of England cannot force compliance except with Treasury support. Usually, however, such compulsion is not necessary, and central bank policies are to this day effected via persuasion and informal communication. An illustration of this point is the "Special Deposits" provision which permits the Bank of England to require that banks hold supplementary balances in addition to the 28 per cent liquid assets requirement. The Special Deposits requirement, however, would not necessarily impinge on a bank's ability to maintain its loans so long as it could meet the special requirement by dumping a part of its short-term governments. But in 1961 when Special Deposits were imposed "the banks were told by the Bank of England that the adjustment in their assets was to be made by reducing advances rather than investments, and they were told the broad lines on which they were to curtail lending to customers." Sayers points out that the threat behind this directive was that if the banks did not cooperate voluntarily, then the Bank of England with the full support of the Treasury would either increase the Special Deposit requirement to whatever level necessary, or inflict potentially punitive capital losses on the banks by raising interest rates sharply (thereby locking them in to government). Thus British monetary policy becomes effective by means of what I would like to call "moral suasion with a stick." The Bank of England provides the suasion and the Treasury the stick. Indeed, the Bank Act of 1946 provides that the Bank of England may direct banks to alter the flow of credit in the economy in order to favor one industrial group over another. Banks would rather not be so told, and it is the threat that this authority may be used that tends to keep them within reasonable lines.

Sayers relies heavily on this type of coercive moral suasion to achieve a level of monetary effectiveness. Indeed, the troublesome problem of nonbank financial intermediaries fits nicely into this approach. In the words of Sayers:

(T)he Bank of England has extended more widely the circle of finan-

> cial intermediaries to which instructions ("requests") are issued. Insurance companies, merchant bankers, "industrial bankers", and hire-purchase finance companies are all nowadays regarded as proper recipients of "requests". The Radcliffe Committee assumed that such requests would sooner or later have to be underpinned by statutory powers, and was fearful of the difficulties of any statutory regulation. The authorities have not lost much time in showing that in this the Radcliffe Committee was too timid: the British financial intermediaries, over a wide range, can in fact be given instructions by the Bank of England, and these instructions can continue for considerable periods without any statutory basis (pp. 127-28).

This weapon of moral suasion has, in Sayers' view, many advantages: (1) it is flexible and hence easily reversed if need be; (2) its administrative limits are dictated by convenience rather than by statutory specification; and (3) it need not wait for legislative approval or enactment. Of course, this type of moral suasion is peculiarly suited to Britain. The Governor of the Bank of England can exercise the morality of his suasion within the confines of a drawing room which could easily contain the whole of the British banking system along with the nonbank intermediaries.

The locking-in effect of the Radcliffe Committee has now become irrelevant. It has been replaced in the sixth edition with the seemingly more reliable weapon of moral suasion with a stick. Though I would have my doubts about the effectiveness of this new weapon even for Britain, I have no doubts whatever concerning its inapplicability for the United States. Apart from differences in the structures of our banking systems, Congress cannot be expected to pass a bank act which would allow such back-stop discretionary powers to the Federal Reserve. Then again, there is the problem of the Treasury and the absurd notion we have in this country regarding the inviolable independence of the Federal Reserve System.

Sayers' influence on the *Radcliffe Report* and his widely respected reputation in the field of monetary economics warrants that serious attention be paid to the sixth edition. Yet there is a gap which the sixth does not fill. One would still like to see Sayers sit down and apply his not inconsiderable talents to a theoretical monograph on monetary theory which would take into account the major suggestions and criticisms that have been made concerning the Radcliffe Committee's liquidity thesis.

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The Management of Money—A Survey of American Experience. By HAROLD BARGER. Chicago: Rand McNally and Company, 1964. Pp. ix, 422. $8.00.

Harold Barger presents a historical survey of central banking and related policies in the United States, followed by a series of analytical chapters dealing with the goals, techniques, and organization of policy-making. The book is apparently intended for a nonprofessional audience and could easily be

read by college undergraduates. Barger's gifts for stylish exposition are frequently in evidence.

About half of the book is devoted to the history of the period since 1914. Emphasis is given to the background of policy actions, in terms of the thinking of policy-makers (documented by extensive quoted material) and their personalities. The play-by-play account of the episodes attending the Accord is quite useful.

For the most part, however, the book seems a rehash of what we all know already—or thought we knew five years ago. One would hardly suspect from this book that the problems of monetary theory and policy have recently been the area of renewed interest and controversy. The provocative, if sometimes infuriating, sallies of Brunner and Meltzer, of Culbertson, of Harry Johnson, find no reflection here. Nor does Barger himself set out to add a comparable voice to the dialogue.

As an original contributor to financial history, Barger has been scooped by Friedman and Schwartz, but their monumental work has not eliminated the need for good, brief historical surveys. Such surveys are valuable, however, only when held together by some analytical framework which permits evaluations of past actions. Barger's book does not possess such a framework. His monetary theory, given in only brief references, is not very explicit (it particularly ignores the demand schedule for cash balances) and lacks empirical content. The most damaging weakness, particularly in view of the title, is that the book generally ignores the quantity of money. The rationale for this is, I think, implied by Barger's view that "if the control of aggregate spending for output is the desideratum, the terms on which money is supplied and the cost of holding it are the important things to regulate, rather than the size of the money stock" (p. 315). This statement, if not technically incorrect, is potentially very misleading. It is associated in Barger's book with excessive stress on interest rates as the measures and immediate objectives of monetary policy.

Failure to deal with the quantity of money (and its determinants) weakens much of the historical narrative, particularly for the period since 1946. Barger reaffirms the view that Federal Reserve policy was "easy" until the Accord, and that, as of 1949, "general controls never had been tightened appreciably" (p. 158). Inspection of the behavior of money and bank reserves reveals a very different story. Despite the bond-support program, the Federal Reserve was able to impose a drastic monetary slowdown beginning in 1947 and continuing well into the recession of 1949. Barger's view of Federal Reserve actions over the decade 1949-1959 is relatively favorable, on the basis of FOMC directives and interest-rate movements. Karl Brunner's testimony in the recent Patman hearings indicates how somewhat different conclusions may emerge if one looks at the behavior of money and reserves. Finally, Barger's preoccupation with interest rates leads him to ignore completely the remarkable shift in monetary policy after 1961.

This lack of analytical focus also weakens Barger's later chapters on monetary policy. He gives no attention to the critical problem of determining quantitative policy targets. His discussion of open-market policy deals chiefly

with the choice of assets to acquire, and the central issue in setting reserve requirements appears to be who should receive the profits from monetary expansion.

I do not criticize Barger because he has failed to accept the contentions of the new quantity theory, for these remain in dispute. But it simply will not do any longer to ignore completely the monetary magnitudes with which quantity theorists have concerned themselves. Barger's substantive views on monetary history and policy are not so much right or wrong as they are irrelevant because so much of the evidence has been ignored.

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Problèmes monétaires d'aujourd'hui—étude des fondements d'une théorie et d'une politique monétaires modernes. By EMILE JAMES. Paris: Editions Sirey, 1963. Pp. 354. F 36.

This volume by one of the leading French monetary economists is part of a series designed to "permit the general public to deepen its knowledge of current economic problems." It combines a survey of contemporary monetary theory with a textbook-like treatment of some of the main issues of monetary policy. The subtitle, *étude des fondements d'une théorie et d'une politique monétaires modernes,* is descriptive of the book's scope only if *fondements* is translated as "elements" rather than "foundations."

While only the first chapter is devoted entirely to theory—a survey of the state of monetary theory at the turn of the century—important contributions to monetary analysis are summarized or mentioned briefly in the following chapters. Some of the recent contributions are considered in Chapter 2, following a quick review of the changes in the setting brought about by the two World Wars and the Great Depression. The following topical chapters deal, successively, with the demand for and the supply of money, the role of money in the economy, and inflation. A chapter on the principles of domestic monetary policy is largely expository and covers credit markets, the banking system, central banking, and the tools of monetary policy. Similarly, the chapter on the principles of foreign monetary policy is devoted mainly to the description of fundamentals, such as the gold exchange standard, exchange controls, and convertibility. The concluding chapter contains a brief survey of French monetary policy and controversy 1958-63 and a discussion of the EPU and international liquidity, not surprisingly draped around the name of Robert Triffin. Generous use is made of equations in presenting the various views on the monetary process, but hardly any reference is made to empirical studies testing their validity. The precise dosage of thoroughly competent exposition of theory and eclectic exploration of policy issues varies from chapter to chapter.

While review ranging from Walras and Wicksell to the Radcliffe (but not the CMC) Report necessarily must limit itself to essentials, the opportunity is taken to pay tribute to Silvio Gesell as a forerunner of Keynes (the author evidently has not caught up with the recent Russian claims that Tugan-Baranovsky had anticipated some of the arguments of *Monetary Reform*), and

considerable space is devoted to appraising "Patinkin's attempt to rehabilitate the quantity theory" (pp. 126ff.).

It is hardly surprising that Aftalion and Nogaro occupy a more prominent place than Robertson or Sayers. When referring to works in English, Professor James crosses the Channel more frequently than the Atlantic. He refers more extensively to publications by German, Scandinavian, Dutch, and Belgian authors than to any similar study published in the United States, even those directed toward a considerably more sophisticated audience. Footnotes overflow with references to postwar French journal and even newspaper articles on monetary theory and policy.

It is perhaps significant for the state of French monetary theory that the author repeatedly tries to drive home the point that money is not neutral and that monetary policy should be regarded as a most powerful tool of a national economic policy. It is no less remarkable that, to convince his readers, James has to argue that "the countries having a planned economy (USSR) and the underdeveloped countries are not the only ones which have used money as a policy instrument" (p. 41). This reviewer has considerable doubt whether monetary policy is a significant instrument of economic policy in Soviet-type economies, and he would be hard up to cite many examples of a successful active use of monetary policy in underdeveloped countries. Anyway, James favors an active monetary policy that would give adequate economic growth priority over the stability of the exchange value of the national currency.

The desire among U.S. economists to establish closer contact with economic thought in continental Europe is growing, as witnessed, for instance, by the series of studies now appearing in supplements to this *Review*. Any student desirous of ascertaining how the monetary landscape appears to a knowledgeable observer in a country where the return to the gold standard is still a respectable topic of academic—and, alas! not only academic—discussion will find James's book rewarding. It is a solid illustration for a recent comment in the *New Yorker* that "the whole level of economic knowledge has enormously risen since the Third Republic days, when France's economic teaching was rated by the English and the Germans as one of the most backward in Europe."

George Garvy

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Money and Finance in Africa—the Experience of Ghana, Morocco, Nigeria, the Rhodesias and Nyasaland, the Sudan, and Tunisia from the Establishment of Their Central Banks until 1962. By Erin E. Jucker-Fleetwood. London: Allen & Unwin; New York: Frederick A. Praeger, 1964. Pp. 335. 45s.; $10.00.

This study attempts to analyze and draw generalizations from the experiences of six African nations with the development of financial institutions and central banking in recent years. Within the selected group of countries there exist considerable historical, social, political, and economic differences, so that a wide variety of experiences is found. It is, in fact, the opportunity for com-

parison and contrast among them which should constitute one of the most interesting aspects of such a study. While this opportunity is frequently taken, Dr. Jucker-Fleetwood is more intent upon offering general prescriptions upon monetary matters than upon conducting such "positive" analysis.

She has drawn upon the accumulated African experience of the members of her Basle Centre for Economic and Financial Research to sketch the origins of the newly created central banks, their principal functions, and some of the major economic problems which they face. Considerable space is also devoted to descriptions of the existing banking systems and other financial institutions. An attempt is made to place the whole in the over-all context of the special development problems of Africa and existing plans to speed economic progress. In view of the absence of any comparable works on current African monetary institutions and problems, and the general interest in the applicability of "Western" techniques of central banking to the less developed economies, the theme is a useful one.

The new African Central Banks have until now been engaged primarily in the handling of the transition to new national currencies, the centralization and management of foreign exchange reserves, the acquisition of government accounts, the creation and support of new financial markets and institutions, and the conducting of economic research, rather than in the pursuit of conventional stabilization objectives. To the extent that it can be used, conventional monetary policy works mainly through the instruments of liquidity requirements, direct credit controls, and to a smaller extent, rediscounting facilities. All of these are likely to be dwarfed in importance, however, by the monetary effects of government fiscal policies and, specifically, the role which the Central Bank is called upon to play in the financing of deficits.

This volume contains a great deal of interesting information which has not been brought together elsewhere. It cannot, however, begin to serve as a basic reference on the subject it attempts to cover.

The descriptions of financial institutions in the six countries are spotty and impressionistic. Institutions which are described in detail in the case of one country are, without explanation, totally ignored in that of another. The coverage of Nigerian finance, for example, which is the only one of the six of which this reviewer can claim special knowledge, is totally misleading. The Loans Boards and Development Corporations, which dispensed large local credits during the 1950's and probably surpassed the commercial banks themselves in their influence upon stability and development, receive only passing mention. The dramatically successful experience with a development bank in the Northern Region is ignored, as are several other flourishing Nigerian financial intermediaries. In view of the author's pretense of comprehensive coverage, these omissions are serious.

The economic analysis, where it appears, unfortunately also leaves a lot to be desired. It may be that a simple-minded quantity theory of money is the best tool for monetary analysis in many parts of Africa, but one expects some defense of its use rather than its employment at every stage as if its applicability were self-evident. Similarly, while it is not unlikely, as is stated herein, that increases in "overall liquidity" (by which is meant the money supply)

frequently create price inflation, that inflation can reduce the rate of saving and have other pernicious effects, and that financial stability is a foundation for economic growth, faith in these orthodox tenets requires a firmer basis than the author's repeated assertions that these hypotheses are facts. The arguments offered herein too frequently lack rigor or clarity.

There is room for argument at many stages of her analysis. I shall confine myself to a firm protest at only one. The argument for larger foreign exchange reserves to serve simply as legal backing for the growing currency supply and, conversely, for restricting the growth of the money supply because of the shortage of foreign reserves, is wholly irrelevant. Reserves should be related to the balance of payments; the money supply to the domestic economy. Arbitrarily determined legal links between reserves and the money supply are by now surely universally regarded as anachronistic.

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Public Finance; Fiscal Policy

Fiscal Survey of Panama—Problems and Proposals for Reform. Report of the Fiscal Mission of the Joint Tax Program of the Organization of American States and Inter-American Development Bank. Baltimore: The Johns Hopkins Press, 1964. Pp. xvi, 212. $7.50.

The Joint Tax Program of the Organization of American States, the Inter-American Development Bank, and the United Nations Economic Commission for Latin America have united their efforts for the purpose of strengthening the fiscal systems of Latin America. As one part of these efforts, detailed studies of individual countries are to be prepared. The first volume to be offered is a "Fiscal Survey of Panama, Problems and Proposals for Reform" prepared by a Fiscal Mission composed of scholars from the United States.

Panama relies heavily on indirect taxes: they produce 53 per cent of its total current revenues. Direct taxes produce 23 per cent, and proprietary activities (Post Office, water charges, the lottery and other gambling activities, the canal annuity, etc.) produce 24 per cent. While Panama has three levels of government—national, provincial, and municipal—the finances of the first dominate the public sector. In the past decade, deficits have been the rule, and the public debt, especially the floating debt, is cause for concern.

The Survey proposes development of direct taxes and de-emphasis of indirect taxes, coupled with a long-run program of improved administration. These general proposals are supported by a detailed analysis of the fiscal system and by dozens of specific recommendations. Throughout, the writing is clear, vigorous, and unambiguous.

Reform of the income tax will require major structural changes. Partnerships and corporations are now treated as entities and subjected to progressive rates. This should be discontinued. The tax now falls largely on unearned income and on a very small number of taxpayers. More than three-quarters of the revenue is from interest, rent, and profits, and in 1959 twenty taxpayers accounted for 65 per cent of total income tax collections. The level of com-

pliance and enforcement is low, especially outside the province of Panama Auditing is almost nonexistent, penalties for negligence or fraud have neve been levied, and delinquency is prevalent and continuous. In view of thes facts, the Survey seems oversanguine in recommending numerous functiona and structural reforms. It does offer the caveat that "any improvement in th income tax would be largely ineffective in practice unless income tax enforce ment is strengthened" (p. 55). Property taxation also suffers from severe de linquency. Taxation of inheritances, the third direct tax, is at low rates. More over, government bonds are exempt, and valuation of property is inadequate

Customs duties are the most important source of revenue, and the Survey is not very critical of them. It does, however, condemn the excessive use o protective tariffs, which are responsible for the high cost of living in Panama A considerable range of internal excises is levied, those on alcoholic beverage being quite productive, although a severe problem of contrabrand exists be cause of the lower tax on liquor imported into the Canal Zone.

The Survey has a chapter on fiscal incentives for development. Panama has offered income tax and other concessions to stimulate private domestic production. The opinion is expressed that the concessions were excessive and that curtailment is desirable even at the risk of the charge of bad faith. Panama has established the Colón Free Zone to encourage a warehousing and distribution center for transshipment of goods. The growth of the Zone has been impressive. A defect in operation is that Panamanian wholesalers and retailers have established bonded warehouses from which goods are supplied (or smuggled) into *domestic* markets. The Survey recommends that, to be eligible for a Free Zone contract, 60 per cent of the sales of each firm be made abroad.

Among the Latin American republics Panama ranks fifth in per capita income, and its economy is booming. An outsider cannot avoid a feeling of dismay to discover, that, despite these favorable circumstances, so much is wrong with the national fiscal system.

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Readings on Taxation in Developing Countries. Edited by RICHARD BIRD and OLIVER OLDMAN. Baltimore: The Johns Hopkins Press, 1964. Pp. 556. $8.95.

This useful anthology contains 33 articles (or parts of books or memoranda). It is divided into seven parts: Fiscal Policy and Economic Development; Comparative Fiscal Systems; Income Taxes; Sales and Expenditure Taxes; Local Finance and Real Property Taxes; Taxes on Foreign Trade; Regional Integration, Tax Administration, and Technical Assistance. There is also a selected bibliography.

Most of the articles or the excerpts stress the prerequisites for a modern tax system, especially the prerequisites for a progressive income tax: money economy, literacy of the taxpayer, accounting records honestly and reliably maintained, a large degree of voluntary compliance on the part of taxpayers, especially the high-bracket taxpayers, and honest and efficient administration.

These prerequisites (cf. pp. 24-26) are obviously required in all countries. The trouble is that most "developing" countries are less developed in these respects as well, so that, in addition to poverty, the absence of some or of all prerequisites is an additional obstacle to a collection of more revenues from taxes. As a matter of fact, the average share of the total government revenue in the GNP of all low-income countries in various years, mainly in the middle-fifties, was 14.7 per cent (p. 53). But it amounted only to 8.8 per cent in Mexico, 9.35 per cent in the Philippines, 9.58 per cent in Argentina, 9.85 per cent in Colombia. Ghana, the most developed of the new African states, has a share of 12.2 per cent.

Yet some developing countries achieve a much higher percentage: Iraq (31.4), Egypt (21.1), Venezuela (18.9), and Panama (16.6). On the other hand, the share of government finance in Spain was only 12.5 per cent of the GNP and in Turkey only 12.7 per cent.

In countries with a much higher than average share of government finance in the GNP (i.e., Iraq, Egypt, Venezuela, and Panama), a major part of the burden of finance is often imposed on foreigners, either by export taxes or by the payments of royalties.

Since tax revenues are quite low in most developing countries—typically 12 per cent of GNP in contrast with 20-28 per cent reached in such advanced countries (p. 54) as Germany (F.R.), Australia, Austria, New Zealand, the United Kingdom, the United States, Finland, Sweden, the Netherlands—and' if one assumes, as Professor Heller does (p. 9), that not more than a third of the government share in low-income countries is devoted to economic development, then "an increase of only 2 percentage points in the ratio of taxes to national income [to 14 per cent] will enable the government to increase its contribution to development expenditures by 50 per cent" (p. 9). This may be far from sufficient in many countries, but even this increase may be out of reach for many of the countries involved.

The main function of taxes in any country is to finance government expenditures, but they are often used for other purposes as well. A case in point is the recommendation of the World Bank Mission to Columbia to penalize poor use of good land by valuing land by its *potential* revenue, punishing landowners whose net return is lower than "expected" by imposing a land tax with rates rising progressively with the decline in the rate of return. Although the recommendation suggests that the "actual rates and the degree of progression should be determined after careful study" (p. 413), the proposal is almost ridiculed by A. O. Hirschman, who describes its impracticability in the Colombian social and political contexts. The foreign advisers, says Hirschman, wish to try out on the "natives" the latest policy "gadgets" which they have not been able to "sell" in their own countries. The warning is appropriate because such tendencies are common, but, to continue with the same example, the Mission's recommendation may still be fruitful, especially if taken with care in certain regions of Colombia itself or in some other parts of the world.

When domestic saving is insufficient, most countries would encourage investment from abroad. Special laws in many countries grant to potential investors, especially to foreigners, considerable concessions in taxes and subsi-

dies and often guarantee to foreign investments free convertibility of funds. How successful these concessions are is an open question. It is clear, as Milton C. Taylor said, that they create a direct conflict between tax policy goals of providing incentives for private economic activity and the allocation of the tax load in a fair and equitable manner. Clearly there are several basic factors that are prerequisites to foreign investments, and the net rate of return is not the first among them. May I mention, for example, that at first the Israeli Law for the Encouragement of Capital Investments more than halved the rate of personal income tax on "approved" partnerships and unincorporated firms, while the income and corporation taxes on "approved" corporations were hardly changed. Yet almost all the "approved" enterprises have been corporations, which implies, I believe, that investors are ready to pay a high price for the security granted by the legal form of a corporation. In any case, a large part of the tax concessions will only increase the revenues of the countries of residence if there is no "tax sparing" paragraph in their mutual tax conventions for the prevention of "double taxation," i.e., if the country of residence of the investor does not credit him for taxes not *actually* paid by him abroad.

What happens when concessions end? Taylor deals with the case of Puerto Rico, and he comes to the conclusion that few firms will leave the island when their present grants expire. The reasons are clear, for Puerto Rico is indeed a special case: there is hardly any fear of expropriation or military actions, and there are no problems of repatriation of profits or trade barriers; add to this the relatively low-cost labor, the similar legal institutions, and the inclusion within the tariff area of the United States. What else could a U.S. investor ask for? Yet the question is important and should be studied in other countries with less "natural" attractions.

The developing countries consist of many countries with many different problems, and they are at various stages of development. No doubt an intelligent reading of this anthology will be of help not only to internal revenue officers in all those countries (and also in fully developed countries), but also to all foreign missions and experts concerned with fiscal matters and especially with taxation.

AMOTZ MORAG

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Public Finance. By OTTO ECKSTEIN. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1964. Pp. viii, 120. $3.95; paper, $1.50.

This short book is part of the Prentice-Hall "Foundations of Modern Economics Series," which includes eight other relatively slender volumes. They provide a flexible set of materials for introductory courses in economics because various combinations of these volumes can be used for courses lasting one quarter, two quarters, one semester, or a full academic year. Otto Eckstein is the editor of the series as well as the author of this particular book.

This volume attempts to cover, for beginning students of economics, the major issues in public finance. It differs, of course, from an ordinary textbook on the subject because it can only hit the high spots. In its 120 pages, however, it does cover a remarkable amount of the territory, including a compar-

atively thorough discussion of public expenditures. Perhaps the chapter headings themselves can best suggest the coverage: the scope of government activity; efficiency in government expenditures; the public finances of state and local governments; economics of metropolitan areas; taxation: principles and issues of fairness; taxes, efficiency, and growth; budget policy for economic stability; and the economics of the public debt.

The volume is described on the jacket as being "highly readable"—and it really is. Numerous points are presented in an exceptionally clear and concise fashion. On pages 71-72, for instance, there is an unusually clear discussion of the "deadweight loss of taxation"—of the reason (other things remaining the same) for wanting taxes to be "neutral." Also, the presentation is relatively objective. For example, the shortcomings of the ballot box in allocating resources are emphasized about as much as the imperfections of the market place.

My only significant objection is to the relative neglect of monetary phenomena in the chapter on economic stability. The determination of national income (pp. 85-90) is analyzed without any reference to the stock of money—a procedure that I am sorry to say is quite common in such discussions. On page 91, Professor Eckstein mentions that the existence of private debt (negative liquidity) may affect the multiplier but still ignores the money supply (positive liquidity) as a factor in the process. In my opinion this is too much of an oversimplification even for (or perhaps especially for) beginning students. Besides, this neglect reduces the clarity of the author's subsequent exposition. On page 93 he does go on to say that the manner of financing deficits can affect the outcome, a topic that would be clearer if the preceding analytical framework had allowed the stock of liquidity to figure in the determination of national income. Along this same line, the role of money is again almost ignored in the discussion of the post-World War II inflation, the Korean War inflation, and the mid-fifties inflation. Instructors using the book should supplement the materials in this chapter to show more of the relationships between budget policy and monetary events in national-income determination.

Perhaps a sample of relatively minor criticisms should be given. Eckstein presents a good summary of the advantages and disadvantages of leaving various decisions in the hands of local governments; he properly stresses the importance of "spillover effects" within the public sector and indicates the possibility that these may make intervention by central authorities appropriate. It might have been desirable, however, to state explicitly that centralization does not *automatically* cope with spillovers better than bargaining among lower-level authorities, for centralization may cause externalities to be neglected. Also, it might have been stressed that apparent inefficiencies associated with spillovers or log-rolling may often simply be costs that are worth paying for checks and balances and the dispersal of power. Finally, I wish to record one quibble: as a former student of Henry Simons, I still wince when discussions of fiscal policy and debt management imply that one must not retire national debt during recession. I would still like to see students reminded that, with different rules about the monetization of debt, there would be no better time to retire debt than during a depression.

To put these various points in perspective, let me hastily add that such shortcomings can be "handled" by instructors and are far outweighed by the book's good qualities. Eckstein's volume is a well-written and valuable survey of the issues in public finance. It will prove to be very helpful to beginning students, and it may be useful to more advanced students as an exceptionally clear and compact summary of the issues.

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International Economics

India's Export Trends and the Prospects for Self-Sustained Growth. By MANMOHAN SINGH. Oxford and New York: Clarendon Press, 1964. Pp. xiv, 369. $7.20.

While India's export earnings during her Second Plan (1956-57 to 1960-61) were within 1 per cent of the First Plan's exports, the Third Five Year Plan calls for doubling India's exports by 1970-71. Manmohan Singh's book is the first to examine critically India's export performance during the 1950's. It also projects India's 1970-71 exports and discusses some major foreign trade policy issues.

Singh devotes the first half of the book to an analysis of past Indian exports. The case studies are introduced by a chapter which discusses the foreign factors—foreign demand and the structure of international markets—and the domestic factors—maximum export quotas, export duties, and inflation—which affect a country's export earnings. This general discussion is supplemented by a later chapter which analyzes the domestic factors affecting each major Indian export; these two chapters might usefully have been combined.

For 12 important Indian exports Singh, relying mainly on other published studies, discusses the trends in world exports. For each commodity the volume of world exports rose, sometimes slowly, during the 1950's. He also finds that for each commodity except iron ore and coffee Indian's share of world exports declined between 1948-50 and 1958-60; even for iron ore India's share of the imports of her major customer—Japan—fell from 18 per cent in 1955 to 16 per cent in 1960.

From these case studies Singh concludes that, for those commodities for which India either has a near monopoly of world exports or is one of a few dominant exporters, "in view of the sluggishness of world trade in most of these commodities during the last decade there were only limited opportunities for India to increase her export earnings. However, there is plenty of evidence that India failed to exploit fully even the available opportunities" (pp. 152-53). For commodities for which she is a marginal exporter—about 40 per cent of present exports—India's export record "has to be explained mostly in terms of domestic factors" (p. 153).

In Part II Singh uses his historical analysis to modify projections of world trade and consumption made by various international agencies. Assuming both that India will maintain her 1958-60 share of world exports of those

commodities for which she is a major exporter and that 1970 export prices will not be below their 1959-60 level, Singh projects total Indian exports at about Rs. 10,000 million in 1970-71, as compared to actual average annual exports of Rs. 6,315 million in 1959-61. About 60 per cent of the projected increase consists of increased exports of iron ore and "new manufactures." Singh feels the projection is "somewhat over-optimistic" (p. 228), for he argues both that import demand in other underdeveloped countries for simple manufactured goods may not grow rapidly and that the Third Five Year Plan did not provide for keeping Indian demand below output in order to provide a surplus for export of machinery, iron, and steel.

Data available in January 1965 indicate that India's 1964 exports were about Rs. 8,300 million; a continuation of the export trends of the last four years would bring India's 1970-71 exports close to the Planning Commission's target of Rs. 13,000 million-Rs. 14,000 million.

Part III (100 pages) discusses the prospects for increased Indian trade with Eastern Europe, the impact of a proposed Southeast Asia Common Market on Indian exports, the effect of the European Economic Community on Indian exports, and the devaluation of the rupee. Singh is skeptical that either more bilateral trade with Eastern Europe or the creation of a Southeast Asia Common Market is likely to be a panacea for India's export problems.

The appropriate exchange rate of the rupee is of more practical interest. He concludes that "a devaluation of the Indian rupee cannot be long delayed" (p. 342) if India wants both to prevent a further decline in her share of world exports of commodities for which she is now a major exporter and to increase export earnings of cotton textiles and of commodities for which she is now either a marginal exporter or a nonexporter. He feels devaluation should be accompanied by increased restraints on domestic consumption, increased efforts to raise agricultural output, establishment of buffer stocks to stabilize the prices of agricultural raw materials used in export industries, and stability of export quotas. The book has several major omissions. Singh devotes little attention to the question of why the Indian government adopted policies which restricted exports. He generally attributes past policies to "the fatalistic view" (p.v) that no Indian policies could much increase her exports. This may be too simple an explanation; export promotion may have conflicted, and may continue to conflict, with other important economic objectives. The book does not analyze any cases of rapid growth of Indian exports in the late 1950's, such as sugar and oilcakes.

Though somewhat repetitious, Singh's style is clear. He uses many statistics to support his verbal arguments, but he avoids formal statistical analysis and so weakens some of his major policy recommendations. For example, can one predict by how much the rupee should be devalued and, if not, should one rely on devaluation as the major means of subsidizing exports?

There is, however, much in this book that challenges prevalent views, especially within India, on the problems and prospects of Indian exports. The book will be useful to those concerned with influencing India's future export policies.

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The Middle East and the European Common Market. By ROUHOLLAH K. RAMAZANI. Charlottesville: University Press of Virginia, 1964. Pp. xxii, 152. $3.75.

This book by Professor Ramazani considers the effects of the policies of the European Common Market on the nine countries which the author includes in the Middle East. The countries are classified into two main groups: the non-Arab states of Iran, Turkey, and Israel; and the Arab states of Egypt, Iraq, Kuwait, Lebanon, Saudi Arabia, and Syria. The book has two parts; the first considers the effect the Common Market has had, and will have in the future, on Middle East exports; and the second examines and evaluates the region's largely political reactions to this economic impact.

Ramazani starts from an "overriding thesis" (pp. xii-xiv). Essentially it is this. The Common Market has been "least detrimental" to the Arab states in the region, but will confront the non-Arab states with serious and complicated adjustments in their trading and financing patterns. But these economic facts, which are spelled out in Part I, "do not go very far toward explaining Middle East attitudes" to the development of the Common Market. The reason for this is that the non-Arab countries which, according to the author, have experienced serious adverse effects nevertheless have reacted favorably and have attempted to accommodate their economies to the establishment of the Common Market. Paradoxically, Ramazani says, the Arab states which have suffered much less than Iran, Turkey, and Israel, and which may stand to gain in the future, "have tended toward outright opposition to the Common Market" (p. xii). Presumably, this observation provides the rationale for the author's advice that "the general reader . . . may move on from the Preface to Part II" (p. xiv), thus skipping his economic analysis.

If the author's major thesis is to stand up, he would have to show that Middle East exports to the EEC decreased from its inception in 1958 through 1961 (his cut-off date), and also that non-Arab exports suffered relative to those of the Arab countries. What are the facts? Since there has been no "reasoned and systematic" study of the impact of the Common Market on Middle East exports (p. 86), we turn to the 23 Appendix Tables. Unfortunately, the data provided are insufficient to determine what has happened to Middle East EEC-exports since 1958. We can use the author's figures for non-oil exports, and reverting to his source it is easy enough to obtain Saudi Arabia's EEC-exports for the missing year of 1958. But when we come to "other Arabian oil exports" in Table 7 we have difficulties.

Not only are data not given for 1958, but for the years 1952, 1954, and 1956 the data include Kuwait and "in some instances probably also Saudi Arabia" (p. 128)! According to the source, for these early years the data refer to total Middle East "sterling-area" oil exports less that of Iraq, i.e., Kuwait, the Neutral Zone, Bahrein, and Qatar. But without indication Ramazani's 1961 figure is given for only Kuwait, thus omitting $70 million of oil exports from Bahrein and Qatar for that year. Thus, the data in Table 7 are not comparable for the years they cover. We have, therefore, computed "Other Arabian Oil" exports from the source for 1958 and 1961 and include

TABLE 1—MIDDLE EAST EXPORTS TO THE COMMON MARKET, 1958, 1961
(millions of U.S. $)

	1958	1961	% Change
Total Middle East	*1,934.2*	*2,224.5*	*15*
Non-Arab States:	*364.2*	*583.2*	*60*
Iran	240.6	364.7	52
Turkey	94.2	152.9	62
Israel	29.4	65.6	123
Arab States:	*1,570.0*	*1,641.3*	*5*
Egypt	69.6	83.3	20
Iraq	380.1	421.4	11
Syria	64.7	127.9	98
Lebanon	60.6	57.2	−6
Saudi Arabia	319.9	263.8	−18
Other Arabian Oil	675.1	687.7	2

Source: UN, *Commodity Trade Statistics, 1958, 1961* (New York: 1959, 1962).

it in our Table 1, which shows Middle East exports to the EEC for 1958 and 1961.

Table 1 indicates that far from the Common Market injuring the region's total exports and affecting non-Arab exports more severely (pp. xii, xiii, 69, 85), we find the region's exports up by 15 per cent, with non-Arab exports growing much faster than those of the Arab states. Thus, the facts would seem to deny Ramazani's overriding thesis and hardly "document the description" (p. 119) as he had hoped.

Actually, Middle East countries achieved sizable gains in their EEC-exports between 1958 and 1961, excepting only Lebanon and the oil exports of the Arabian peninsula. For Lebanon, considering the relative unimportance of visible to invisible earnings, the small recorded reduction in their exports is not significant. Interestingly, though, in late 1962 Lebanon opened negotiations with the EEC, despite its being a member of the author's "Arab opposition group." Also the $56 million fall in Arabian peninsula oil exports may be only a statistical accident, since in 1961 the Netherlands recorded oil imports of $144 million from Syria and Lebanon, neither of which produces oil, but whose pipelines deliver oil from Iraq and the Arabian peninsula to the shores of the Mediterranean. Thus, actual Arabian oil exports to the EEC in 1961 must have been higher than the recorded country figures indicate.

But despite the erroneous deductions the author draws from his economic analysis in Part I, his contention that the reactions of the Middle East states to the Common Market have been based less on economic factors than on considerations more "political, ideological, geographical, historical and psychological" in nature (pp. 70, 97) seems well founded. These meta-economic considerations revolve around the traditional enmity of Turkey and Iran to Russia, the bitter antagonism between the Arabs and Israel, and the widespread Arab suspicion of Western neocolonialism, which apparently is how the Arabs, or at least the volatile Arab press, view the rise and development of the European Economic Community.

Ramazani's analysis, in Part II, of these factors is well done and interesting. Where we disagree is in his insistence that an analysis of the economic impact on the Middle East (which he erroneously sees as being detrimental) leads only to paradoxical conclusions, and therefore we are driven to political relationships to explain the differences in Middle East reactions to the Common Market.

What of the future? As Ramazani notes, for oil and cotton, which together in 1961 comprised 85 per cent of all Middle East exports to the Common Market, export prospects look bright. Apparently these products will be beneficiaries of the "growth effects" of the EEC, and not subject to "diversion effects," although such terms (as well as trade-creation and trade-diversion) are completely absent from the book. It would seem that the Common Market has been, and should continue to be, beneficial to the Middle East economies, and that in the future whatever political relationships develop will do so within the context of a complementary supplying area negotiating with a burgeoning industrial market. But this tentative appraisal must await the findings of a good economic study, which still needs doing.

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The Socialist Commonwealth of Nations—Organizations and Institutions. By Kazimierz Grzybowski. New Haven and London: Yale University Press, 1964. Pp. xvii, 300. $7.50.

Written by an expert on Soviet-bloc law, the purpose of this book is "to describe and analyze—with some, but not exclusive, preference to their constitutional instruments—the organization, aims, and activities of the institutions of the socialist commonwealth. . . ."

Different chapters deal with the legal unity of the bloc, the forms of economic relations, the Council of Mutual Economic Assistance (CMEA), specialized international bodies of the bloc, the Warsaw Treaty Organization, peaceful settlements of intra-bloc disputes, and the emerging concepts of socialist international law: Although his primary focus is on the European Soviet-bloc nations, the author also brings the Asian communist nations into the analysis as well.

Kazimierz Grzybowski believes that the formal institutional aspects of the Soviet bloc " represent only a small part of the reality that unites its members in an international complex." Nevertheless, a survey of these formal institutions is extremely necessary in an area of research in which hard facts are difficult to find and relevant materials are scattered in obscure sources in many languages. The author performs a valuable service by gathering these scraps of information from a large number of isolated sources and presenting them in a condensed form. Although the book has a number of shortcomings, it is a useful reference book for all Soviet-bloc specialists.

The best part of the book is Grzybowski's analysis of the way in which institutions have arisen to aid in the settling of economic disputes. Jacob Viner, in his celebrated 1944 article on state trading, stated the belief that economic conflicts could *not* be resolved in a state trading network except through force

or political pressures of the stronger nations on the weaker. Grzybowski shows how the socialist nations have tried to set up special courts with very definite rules and procedures to deal with some of these problems. He also tried to delineate some of the areas in which political solutions of such disputes are still necessary.

Moreover, the book contains some valuable information about the founding and organization of a series of lesser-known bloc institutions such as the Organization for Cooperation of Railroads, the Danube Commission, the Postal Organization, the various fishing commissions, the "Friendship" pipeline, the "Peace" power-grid, or the Joint Institute of Nuclear Research. Although the functions of many of these organizations are limited, the delineation and evaluation of their areas of activities are necessary for an over-all assessment of the economic interrelations of Soviet-bloc nations.

In spite of the great deal of useful information which the book contains, it has three serious flaws. First, the description of the institutions of the bloc omits a number of important aspects. Although considerable amount of space is devoted to the legal side of the specific trade agreements between foreign trade enterprises of different nations, no attention is paid to the much more interesting question of the legal status of the global one- or five-year trade-agreements which serve as the framework for the specific trade agreements. (In his *On Communism,* Imre Nagy suggests that Soviet reneging on such an agreement caused some damage to the Hungarian economy.) Such interstate agreements as the Pacts of Friendship and Mutual Aid or the Cultural Cooperation Agreements are hardly touched upon. The agreements on, and the institutions for, the bilateral exchange of technological and scientific information are not mentioned, although these exchanges have probably played an important role in the conservation of scarce scientific and technical personnel in different bloc countries.

The discussion of the Council for Mutual Economic Assistance also has important omissions. For instance, in his list of Standing Commissions of the Council (p. 77), the author omits reference to the Commissions for Construction, for Standardization, for Coordination of Scientific Research, and for Statistics, among others. The founding of the last three of these Commissions represents important developments in the widening sphere of CMEA activities and deserves attention. One of the two most important early activities of the Standing Commissions, the standardization work, is also completely omitted from the analysis. Interesting questions concerning the means CMEA uses to enforce its decisions are not discussed. It would have also been useful to the reader if the text of the only published CMEA charter could have been included in the Appendix.

Second, Grzybowski often steps out of his institutional and legal analysis and attempts economic and political analyses of certain questions as well, with questionable results. For instance, he spends a considerable amount of space discussing the new intra-bloc bank and the failure of the old currency clearing system, primarily from the viewpoint of some general notions about currency convertibility. Because he does not present a very clear or detailed account of the intra-bloc foreign trade price system or the methods by which

foreign trade is planned, his remarks are quite superficial. On the other hand, he could have given the reader considerably more insight into the problem by comparing the texts of the 1957 and 1961 clearing arrangements and arriving at the following important differences:[1]

a. In the 1957 agreement, the State Bank of the USSR (Gosbank) acted as the clearing-house agent. In the 1963 agreement, an independent organization (The International Bank of Economic Cooperation) was set up, where each member subscribes a specified amount of capital and is entitled to one vote on the Board of Directors.

b. In the earlier agreement, the multilateral clearing covered only that trade which was carried on over and above the annual bilateral agreements. Furthermore, specific permission of all involved countries was needed for any clearing to take place. In the new agreement, all trade (with certain exceptions) is to take place through the bank, and the multilateral balancing of accounts is to take place automatically.

c. The 1957 agreement placed strict (and very small) limits on possible debit balances and balances that could be carried over from one year to the next. The new agreement is much more flexible and gives the Bank Council authority to decide maximum debtor positions and balance carry-overs. These debit positions are to be covered by Bank loans.

d. In the 1957 agreement interest charges on balances were specifically stated. In the 1963 agreement the Bank Council has authority to set these rates, and commentators on the agreement have said that interest charges on Bank credits would vary considerably, depending on whether the credits were "trade creating" (e.g., to finance trade over and above the bilateral agreements) or "trade postponing" (to bridge over payment difficulties caused by avoidable delays or trade problems).

e. The 1963 agreement also contained a great many provisions for activities not covered in the 1957 agreement, e.g., developmental loans, special accounts for noncommercial payments, etc.

Such a contrast of provisions immediately suggests some of the factors behind the failure of the 1957 agreements and obviates the need for a general discussion on currency convertibility. Contrast of the 1963 agreement with the EPU agreement (and experience) might immediately suggest some of the difficulties which the IBEC will face in the future.

Similar to his discussion on multilateral clearing, Grzybowski tries to evaluate the economic successes and failures of CMEA without employing any very rigorous tools of economic analysis. With his skills and training, further legal analysis of the organization might have been more worthwhile to the reader.

A final weakness of the book is the author's almost total neglect of other

[1] An English text of the 1957 agreement may be found in: Laszlo Zsoldas, *The Economic Integration of Hungary into the Soviet Bloc* (Columbus: Bureau of Business Research, Ohio State University, 1963), Appendix A. A German and Russian text of the 1963 agreement may be found in *Gesetzblatt der Deutschen Demokratischen Republik* I, 2/1964. Although Grzybowski probably was not able to obtain the text of the 1963 agreement at the time he wrote the book, enough details of the agreement from the commentaries by various bloc economists were available for such a comparison to be made.

Western studies of various aspects of the Socialist Commonwealth. For instance, although his discussion of the Danube Commission is enlightening, he differs considerably with other Western authors in certain points of interpretation. Analysis of these differences would have left some readers in less of a state of confusion.

Despite these limitations, the book contains a wealth of information and ideas which make it a welcomed addition to the political-economic literature on the Soviet bloc.

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International Economics. By PETER B. KENEN. Foundations of Modern Economics Series. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963. Pp. vii, 120. $3.95; paper, $1.50.

This book is part of Prentice-Hall's "Foundations of Modern Economics" series; thus Peter Kenen was limited to a (roughly) 100-page treatment of his subject. That he succeeds in presenting a lively and reasonably thorough survey without exceeding this constraint by more than a mere 11 pages will not surprise those who are familiar with his *Giant Among Nations*. Kenen divides his work into six chapters, of which the middle four constitute the heart of the discussion, dealing respectively with trade theory, commercial policy, the balance of payments, and financial policy. Kenen develops a sort of modified Edgeworth Box and uses production-possibilities schedules in his discussion of trade theory; otherwise his analysis requires nothing more sophisticated than supply and demand diagrams, and of these it happily has a relatively generous number. Chapter 4, on the balance of payments, is the best in the book, in this reviewer's opinion; in rapid-fire order but in lucid and graceful style, Kenen develops the notion of a balance of payments, the accounting techniques involved, alternative exchange-rate systems and their concomitant adjustment processes, and winds up with the relationships of external adjustment to national income and growth; all this in 26 pages.

Upon finishing this well-written little volume, however, the reader realizes that, despite its author's competence and style, it is not very different in organization or content from the international trade sections of standard elementary textbooks. Kenen has 111 pages to deal with the subject; Samuelson has 82 pages, Bach 58, Harriss 72, Hamberg 94; McConnell with 32 pages, Wright with 30 and Reynolds with 35 offer shorter presentations. Kenen has what is essentially a four-chapter organization along with an introduction and summation; although they take up subjects in slightly different sequences, the same is true of Samuelson and Bach. Hamberg also includes a separate chapter on foreign aid and economic development. Wright and Harriss have three chapters apiece, McConnell two, and Reynolds only one. A comparative evaluation of the treatments of all these authors is not possible here; it may be useful, however, to notice at least some of the superficial resemblances and differences between Kenen's work and those of Samuelson and Hamberg, whose treatments are of approximately the same length. None of the three uses graphical devices more complicated than production-possibility

schedules; both Kenen and Samuelson make considerable use of charts and tables, while Hamberg does not. Neither Kenen nor Samuelson discusses the individual components of the balance of payments in as much detail as Hamberg, but Kenen's construction of the balance of payments is far superior on at least one score: by avoiding the ancient long-term–short-term capital distinction, lumping the latter together with gold, and calling this the "cash" account, he slaughters a terminological bugbear which had driven several generations of students into confusion. (To illustrate that, in economics as in so much else, there is little that is new under the sun, it might be observed that a quite similar division of the balance of payments was used by Fairchild, Furniss, and Buck in their 1939 edition; whether Kenen reinvented such a classification system or merely revived it, pedagogues of economics owe him a debt.) Samuelson's discussion of the foreign trade multiplier is (though in an appendix) by far the most thorough, and both Samuelson and Hamberg go deeper into foreign aid than does Kenen. In short, one pays one's money and takes one's choice; the products are differentiated, but for many readers not very greatly.

Such considerations provoke one to consider critically the whole *raison d'être* of the Prentice-Hall series; fairness requires that Kenen's book be examined in the light of the announced objectives of that series. In his editorial introduction, Otto Eckstein observes that "economics has grown so rapidly in recent years that no one book can present it authoritatively today." Hardly any economist will disagree with this respectable sentiment, which dates back at least as far as Howard Ellis' Introduction to *A Survey of Contemporary Economics* in 1948. Yet, while it will be readily admitted that no one 900-page book on economics can "present it authoritatively today," it is not so obvious that nine 100-page books can do much better. Indeed, while it is obvious that "the day of the exhaustive treatise by a single Jovian figure" may have passed, as Ellis suggested it might, it is not obvious that nine writers, though each be an outstanding authority in his specialty, will do a job superior to some single aspiring Jupiter trying to crash the gates of a vanished Olympus. That increasing specialization may lead to an incapacity to "conceive noble sentiments, judge of private duties, and assess the interests of (the) country," as Adam Smith warned us two centuries ago and Howard Ellis reminded us 16 years ago.

Eckstein is correct in asserting that, through this new approach, the nine individual books "by mirroring the actual state of knowledge and discussion . . . gain in interest, depth, and relevance (and) communicate some of the excitement of the current research in a developing field." Yet they may lose in uniformity of the level of treatment, as D. H. Robertson felt the Survey articles had done, or in development of interrelationships among specialties, as Jacob Viner felt the Survey articles had done, or in illustrating the versatility of analytical tools; all these factors may impair their utility as teaching devices.

Eckstein also points out that this format frees "the teacher to devise his own course curriculum, rather than to follow the format of the textbook. . . . Specific areas can be explored at greater length." It is doubtful that many

academicians have heretofore felt severely constrained by the organizational plan of the author of their textbook; the assignment sheet that calls for Chapter 7 to follow 11, with 23 in between, is all too familiar at most universities. Similarly, it is not necessarily the case that the exploration possible in 100 separately bound pages is likely to be greater than that possible in 100 pages bound up with 800 others in an ordinary text; in both cases, exploration of specific areas at greater length is surely going to involve outside reading assignments.

There is, however, one unquestionable advantage of having this series available, although the editor quite understandably does not touch upon it; the use of these separate and relatively inexpensive volumes may make possible some saving in textbook costs to the individual student. The purchase of a few "Foundations" volumes out of the set of nine may well let the pocketbook off more easily than would be the case if one fat text were required. The existence of the series, in other words, increases the alternatives open to that teacher who can conceive of himself as a minimizer of the costs of education, and that alone makes the series worthwhile, given the crushing costs of textbooks these days.

If, then, the purpose of Kenen's work is to present the most useful introduction to international economics that can be gotten into a relatively inexpensive paperback of 100 pages, he has accomplished his task very creditably. Whether his little book will find wide use would seem to depend, however, upon whether many teachers are sufficiently concerned about textbook costs that they will make the effort to organize their own courses around publications such as this or whether they will continue to rely upon the exhaustive treatise of some Titan or other. It is interesting in this respect to note that the best-selling treatise of them all was being written by Samuelson at the same time that Ellis was penning his observations on the passing of the day of the single Jovian figure.

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Business Finance; Investment and Security Markets; Insurance

Corporate Finance and Fixed Investment—An Econometric Study. By W. H. Locke Anderson. Boston: Division of Research, Graduate School of Business Administration, Harvard University, 1964. Pp. xiii, 130. $4.00.

This well-constructed study brings us closer to the resolution of an important issue in economic policy. As the author points out, his work does not fully solve the problem of the determinants of capital spending, but it has narrowed down the field of investigation; and its discussion of the succession of obstacles in the way of conclusive findings makes the book valuable as a case study for the future econometrician as well as for the consumer of the econometric product.

Professor Anderson's chief innovation is in including pre-existing liquidity position variables as well as the customary cash-flow variables in the investment model. He establishes that these variables do influence the changes dur-

ing the following quarter in manufacturers' aggregate debt and liquid asset holdings. This does show the variables to be relevant on the level of financial management; but that demonstration is no more than suggestive where *real* asset management is concerned, however. (For example, the strong effect of the long-term interest rate on long-term borrowing is found to reflect the tendency of the biggest manufacturers to borrow long term when money is cheap rather than when they need funds to finance investment; the strong negative influence of pre-existing long debt on the quarterly net rise in long debt probably is partly due to the fact that, when their balance sheets were looking too debt-ridden, many manufacturers could simply accelerate the conversion of their convertible long debt—a major part of their debt issues in the 1950's—into common stock.)

It apparently was expected to avoid the identification problem because the variables determining manufacturers' investment take effect with a substantial lag, but this escape is blocked by the presence of severe autocorrelation in the quarterly capital spending series: I_t is closely correlated with I_{t+2}. Anderson carefully points out the marked serial correlation resulting from this and from the need to express the independent variables as moving averages, as well as the weakened statistical reliability of his final results due to testing many different models and selecting the best one ("data mining"). Nevertheless, the confirmation from his balance-sheet liquidity adjustment tests and the plausibility of the results persuade him to accept the final result for purposes of discussion. In fact, the results for one variable, the ratio of long debt to total assets, are so consistent from one model to another and so consistently reliable by the t-test that this new determinant of manufacturers' aggregate capital spending should give permanent recognition to its discoverer by being labeled Anderson's Constant.

The model finally selected makes quarterly capital spending a function of the average values of the independent variables in the fourth through seventh preceding quarters; viz., the long-term debt/asset ratio, capacity utilization, the interest rate, and two vestiges of the short-term liquidity position variables: government securities held and accrued income taxes payable. (Others, which would seem essential for making sense of the last two, had to be dropped because of severe intercorrelation with other, more important variables or because—as with the scale factor, quarterly sales—their presence caused other independent variables to assume the wrong sign.) Trend and seasonal variables were also used. Gross retained profits are also used in the model, but the partisans of capacity utilization (the accelerator) will be gratified to learn that its coefficient was usually smaller than its standard error.

The influence of the cost (and/or availability) of money is found to be strong enough to have made monetary policy a useful anticyclical variable, if the lag of its effects had not been the long one noted above. That lag is roughly the same as that of de Leeuw (1962), part of whose lag formulation Anderson employs, but it is much longer than Meyer and Glauber's. Their study, published in 1964, is superior in allowing different lags among the variables and in having followed the business-cycle literature in providing for

a weaker interest-rate effect in times of low activity when all desired investment is easily self-financed; but it seems inferior to Anderson's in not having employed de Leeuw's lag pattern.

Anderson's long lag had to be derived indirectly. First, the best correlation between a few years of NICB data on big manufacturers' quarterly capital "appropriations" (decisions to spend) and the group's subsequent capital expenditure had the carrying-out of investment decisions occur in equal parts over the four quarters following the decisions. This "construction" period is not inconsistent with the findings of Thomas Mayer (1958)—although much shorter than Karaken and Solow's (1963) biased findings from the unsatisfactory machinery orders and production series—but it is dominated by the behavior of the larger, "heavier" companies, and is twice as long as that indicated by the Commerce-SEC evidence on the timing of manufacturers' planned capital spending, available for the depressed years 1958 and 1959.

The rest of the total lag—that from the independent variables to their effect on the investment decision—was finally set, by the criterion of best fit, at three quarters. Although such sluggish entrepreneurship seems intuitively suspect, it was confirmed by separate tests of 13 industry groups, only three of which were found to justify a shorter decision lag. However, the table presented seems to justify a mere one-period decision lag for six or seven of the thirteen.

With their models having a variable in one quarter which affects the investment of several quarters, econometric researchers usually are incorrect in their view that a long lag can preclude stabilizing monetary policy. But the lag question retains importance because common sense tells us that the models should have permitted decision lags to vary inversely with the amplitude of change in the independent variables, and policy variables will be the losers in an amplitude contest.

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Industrial Organization; Government and Business; Industry Studies

A Macroeconomic Theory of Workable Competition. By C. E. FERGUSON. Durham: Duke University Press, 1964. Pp. viii, 217. $7.50.

Dr. Ferguson has set before us a volume much wider in scope than its modest 217 pages might suggest. The task of assimilating it is not made easier by the fact that the title is an accurate guide to the contents only within the meaning of some rather special terms used by the author.

His essential theme can be summarized in these propositions. Welfare economics, despite the increasing rigor and elegance of its presentation in modern form, is basically unable to provide workable foundations for public policy toward industrial structure and competition. In particular, the "theory of second best," which finds its most important microeconomic application in the concept of workable competition, provides no substantive help. He therefore turns to an entirely different approach, based on the attainment of generally

acceptable macroeconomic objectives. Empirical investigation can provide the essential causal links between these accepted national targets and the instruments through which they are achieved, leading to public policies both simpler and more appropriate than those derived from concepts of market efficiency. And, more important, it is argued that attainment of these target objectives implies optimality in terms of a meaningful social welfare function.

The theoretical core of the argument is set forth in Part I. The first chapter deals in some detail with the origins and development of modern welfare economics. (In fact the thoroughness with which Ferguson traces out the increasingly lofty techniques might lead some readers to support Boulding's comment that welfare economics bids fair to become the phrenology of the social sciences.) This is followed by a useful review of the concepts of workable competition and an evaluation of their utility as norms and as guides to specific policies.

The conclusion that this approach is theoretically unsound and operationally cumbersome leads Ferguson to the view that the practical job of developing public policy toward industrial structures is best approached by "prescribing fixed values for some of the variables in the (social welfare) function and treating them as targets." Further simplification can be achieved by limiting the objectives to economic ones. These objectives—high and stable employment, reasonable stability of the price level, economic growth, and the absence of excessive market power—he regards as generally acceptable on a priori grounds. Moreover, they can be regarded as validated in some meaningful manner by their rather complete embodiment in statutes emerging from an essentially democratic legislature. These conclusions, coupled with a finding (or, more accurately, a statement) that these objectives are always mutually consistent, leads to the conclusion that successful public policies directed toward democratically sanctioned objectives constitute an acceptable approximation to maximization of a social welfare function (with due recognition of the position of economic objectives in a fuller hierarchy of social values).

Given the targets, the basic task is to establish, through empirical analysis, causal relationships linking "instruments" and targets; once these are established, policy design follows almost automatically. So does the definition of workable competition: "an economic system is workably competitive if there is no feasible change in industrial and union structure that would make target attainment more likely, given the probability limits imposed upon our knowledge by its origin in empirical research."

In Part II Ferguson undertakes empirical tests of alternative hypotheses relating business size to stability of employment, stability of the general level of prices, and economic growth. The section also includes explicit discussion of the impact of bottlenecks on firm behavior and of the influence of government policies toward business firms and labor unions that bear on market power. From these, he derives specific government policies required to induce firm and union behavior leading toward attainment of his four targets.

I suspect that other readers will share this reviewer's perplexity over omis-

sions and commisions in what is clearly a scholarly work reflecting long and detailed consideration of the literature in the several fields considered. Perhaps the most obvious omission is the virtual absence of any reference to efficiency in the allocative sense. This is to be expected, of course, in view of the ruthless disposition of microeconomic theory in Part I. Still, to one raised in the tradition that economics is always concerned with alternative ways of achieving given objectives, it would appear that even the author's broad macroeconomic objectives are surely attainable in a variety of ways, the essential tests of which must rest in part on straightforward, microeconomic evaluation of alternative costs.

The argument that ballot-box votes are somehow a better test of fundamental social choices than dollar votes, clearly implicit in the justification of the author's macroeconomic targets, has been treated so roughly in other places that it hardly seems to stand the weight placed on it on this exposition.

After the searching analysis of the shortcomings of a microeconomic approach to welfare maximization through analysis of the behavior of firm and industry, it is surprising that the fundamental concepts of his macroeconomic alternative are accepted so casually—and on the basis of unequivocal support of positions that are anything but settled in the current literature. A few points might be noted by way of illustration. On page 68 for example, Ferguson states "the level of employment varies directly with the level of gross national product and, consequently, with the levels of its components, especially consumption and private domestic investment." While this statement is quite unexceptionable, the accompanying footnote reads: "We omit government expenditure. At least in the short run a sufficiently high volume of government expenditure would probably create full employment. But such a policy would also violate the stable price objective." If this is macroeconomic orthodoxy, then heterdoxy is surely on the rise.

Five paragraphs suffice to demonstrate to the author's satisfaction that "in the special case given by our economic objectives, there seem to be direct functional relationships among the variables of the social welfare function. Growth is necessary to provide a climate in which high employment and price level stability may be achieved. Similarly, high employment and price level stability are, at least in the long run, preconditions for perpetual growth. Hence, a policy measure that leads to a higher level of attainment for anyone of the targets is likely to affect the other targets in exactly the same way." If these statements are accepted, one could not question Ferguson's conclusion that the implications of the theory of second best would not complicate his analysis substantially. I simply feel that it would take a great deal more than five paragraphs to convince most economists that the conclusion is valid, even in a broad probabilistic sense.

The reader will find a careful discussion of statistical techniques employed in Part II, and these need not be reviewed in detail. Ferguson does find support for the position that business and union structure are related to the price-stability objective, but argues that neither business size nor—surprisingly—the level of industrial concentration, has determinable systematic, and stable relationships with the other macroeconomic targets selected (high and

stable employment, economic growth, and absence of excessive market power).

The policy conclusions that emerge are predictable. The author argues for a vigorous antitrust policy, but with greatest emphasis on the traditional area of "the prevention and elimination of monopolies, conspiracies, unfair competitive practices, etc." He specifically rejects the possibility of curing "non-demand-pull" inflations by controlling market power as long as labor unions are specifically exempt from antitrust legislation, and finds no solid support for the argument that control of firm size would improve economic performance in any of his four target areas. In a more positive vein, the proposals include familiar and generally acceptable changes leading to improvement of the competitive environment, together with others considerably more contentious. Among the latter are the elimination of such built-in inflationary mechanisms as further increases in the legal minimum wage, automatic cost-increasing provisions in labor-management contracts, and wage and employment guarantees.

The heavy stress laid upon the impact of business and union policies on inflation not traceable to excess demand calls for a national wage policy limiting wage increases to the level of national productivity gains (whatever that might be). This latter proposal is less drastic in a legal sense than those involving abridgment of contractual rights now sanctioned in collective bargaining, and sounds simpler; I suspect, however, that its ramifications are vastly more difficult to trace out.

To this reviewer, the book repaid the considerable effort required to pursue its arguments carefully. I am afraid, however, that the benefits arose more from the excellent critical summaries of the literature dealing with specific topics, welfare economics, workable competition, alternative theories of inflation, and matters of statistical technique, than from the author's own positive thesis and the support provided for it.

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Microeconomics and the Space Economy—the Effectiveness of an Oligopolistic Market Economy. By M. L. GREENHUT. Chicago: Scott, Foresman & Company. 1963, Pp. 357. $7.75.

This book is unfortunately and badly misnamed. The use of the term "Space Economy" in the title and the fact that the author is best known for his previous work in the area of economic-location theory and practice have led most economists to assume that this book deals with the location of economic activity. The subheading hints that a quite different subject is involved but it too is misleading.

What is actually presented in this book is a new theory of imperfect competition, monopolistic competition, or oligopolistic competition—whichever term one chooses to utilize. It is the opinion of this reviewer that this book contains new insights not matched since the contributions of Edward H. Chamberlin and Joan Robinson in this area of economic theory. Dr. Greenhut has introduced the fundamental factors of space and uncertainty into his

ieory to produce a new model to which he has not given a specific name, but hich I have chosen to call the spatial oligopoly model.

What is the nature of this new theory? (1) Greenhut notes that pure competition theory assumes, among other things, infinitely great demand for the idividual firm, an unadvanced technocracy, a relatively small number of andard products for consumers, and many buyers and sellers at a point; ut, today, advanced technocracy, indivisibilities and uncertainties, diverse roducts, and *distance* account for a system of markets in each of which iere exists a limited number of firms belonging to a given industry. The ecoomic landscape consists of manufacturing firms with one or more plants, holesalers, and retailers who are spatially (and otherwise) differentiated. He hows that on any level the spatial competition that develops takes oligopolisic form. The inevitable "oligopolistic" result is established by several models lealing with the relations between firms over space. By these models, Greenut (a) establishes the present-day real-world impossibility of pure competiion, (b) points out the improbability of Chamberlin's type of monopolistic ompetition, (c) identifies the conditions under which firms agglomerate or lisperse, and (d) proves the emergence of oligopoly in an economy marked y noticeable transport burdens and distances.

(2) Greenhut formally establishes the idea that "organized" oligopoly ver space (e.g., the basing point system) is imposed by large firms on small irms. He shows the inefficiency of "organized" oligopoly both from the tandpoint of locational efficiency and productive efficiency. (3) He examines "unorganized" oligopoly and derives the conditions (e.g., free entry, no collusion, etc.) under which firms in space will locate and produce efficiently. (4) He identifies the basic properties of an efficient spatial oligopoly and compares them with the welfare standards used in pure competition theory; he establishes the existence of these properties in a manner similar to the proofs which have become fundamental parts of pure competition theory.

The models, demonstrations, and mathematics used to establish (2), (3), and (4) require several chapters in the book and cannot be explained readily in a brief review. However, Greenhut accomplishes his proof by demonstrating the hand-in-glove relationship between uncertainty, indivisibility, and oligopoly—by noting that uncertainty and indivisibility warrant a return, call it profits, and that the long-run profits from uncertainty and indivisibility are counterpart to the classical returns of long-run rents for risk and for differential skills. In this way, he establishes the normative properties of oligopoly in space and shows their points of identity (as well as differences) with pure competition. He explains this result as follows:

> Thus, we found that when the spatial oligopolistic market is unorganized and entry is free, an irrepressible force blankets the market in much the manner of Adam Smith's invisible hand and presses profits to the level where they are perfectly commensurate with the indivisibilities and uncertainties of industry. . . . The fact is, however, that the difference between these two markets is only one of semantics. That is, the oligopolist's positive economic profits (surplus differentials or whatever they may be called) turn out to be as natural and desirable as the pure com-

petitor's zero economic profits, provided they are recognized as the return for undertaking economic activity in a market subject to indivisibilities and uncertainties (p. 266).

Greenhut further observes that the normal long-run profits of his model and of the U.S. economy may constitute an inducement to private investment sufficient to maintain full employment. He concludes the book by pointing out certain problems in economic theory (e.g., antitrust and labor union matters) which will be resolved differently with the use of his spatial oligopoly theory as compared with conventional theory. Whatever the reader's reaction to this new theory, he will find here a well-written, scholarly work which uncovers as much content of classical economic theory as it does the theory of spatial competition.

What are the deficiencies of Greenhut's model? First, reasonable freedom of entry may not only not exist but be impossible to obtain. The model explains such phenomena as the development of suburban retail shopping centers and the multiple location of gasoline service stations, as no previous theory can. On the other hand, there appears to be a tendency to overinvestment and excessive allocation of resources in some markets which is not reflected or even acknowledged in the analysis.

Despite these and other possible shortcomings that may be discovered in this new model, the book is one that cannot be ignored by the nation's academic economists who teach graduate courses in economic theory. Eventually, many of the innovations contained in this book may find their way into undergraduate textbooks.

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Les formes modernes de la concurrence. By A. PIATIER, W. KRELLE, J. AUSTRUY, P. MORAN, M. PEYRARD, J. F. BESSON, G. RULLIERE, J. OUSSET, AND P. KENDE. Paris: Gauthier-Villars, 1964. Pp. vi, 344. F 50.

Modern Forms of Competition consists of nine essays on oligopoly, market power, and related questions. The co-ordinator of the group, André Piatier, describes the background and objectives of the collection in his introductory essay, which focuses on the reasons for loss of belief in competitive models.

Two chapters are devoted to theoretical analysis. Wilhelm Krelle provides a well-organized discussion of bilateral monopoly and of alternative oligopoly solutions, with a helpful summary relating the results to other studies in this field. Pierre Moran concentrates on interpretation of cost curves, paying particular attention to alternatives facing the firm in considering movement into new markets.

Jacques Austruy is concerned with public policy toward oligopoly, concluding in general that cooperation among firms should be encouraged rather than attacked. The greater the degree of cooperation, the greater the reduction of uncertainty, and the more extended the planning horizons of producers. Extended planning periods in turn maximize technical progress. Agreements may make it possible for firms to retard innovation, but no such practice should be

judged hastily: "... it may mean a passage less abrupt, hence more economical, from one form of life to another" (p. 110). Profit is viewed as an obligation of the present, "to assure for the world of tomorrow à life that we judge better" (p. 111). Still, oligopolists may need to be checked if they become so interested in economic progress that they cause trouble for small firms. "Oligopolists are dedicated, in a certain vision of the world, to the exploration of the future and the organization of the present. And that is what justifies them. But that organization of the present ought not to imply, when it is not necessary, the destruction of old values and of preceding structures" (p. 116).

Austruy's essay is full of interesting argument, but it is not notable for concern with analytical or empirical complications which might have led toward deepened understanding at the cost of lessening the simplicity of the conclusions.

Max Peyrard summarizes a good deal of material from Bain, within an original organization of questions concerning definition of optimum scale. Did no one in the group see the contrast between Austruy's one-dimensional statement of the superiorities of the large firm (p. 99), and Peyrard's careful presentation of the real complexities of the question?

Jean-François Besson discusses relationships between the Coal-Steel Authority and the steel producers, stressing the problems of coping with multi-industry financial groups. This essay is chiefly concerned with legalistic questions, giving useful information on case decisions concerning permissible techniques of market organization, but not much on behavior.

Jean Ousset and Gilbert Rullière examine French agriculture. Ousset does an intriguing job of explaining methods of control in markets for sugar, Roquefort cheese, flour milling, and wine. These cases are full of new information about real markets and varied experiments in managing them. The difficulty is that they are all so brief that the significance of the facts is left unexplored. This material would make the basis for a splendid book if it were organized to examine implications for efficiency and public policy.

It is especially tantalizing that the completely theoretical discussion of bilateral monopoly by Krelle never refers to Ousset's detailed study of bilateral monopoly in the supply of inputs for and the sale of Roquefort cheese, nor does Ousset make any reference to Krelle's analytical framework. An unusual opportunity for exchange between the theorist and the empiricist is completely missed. In any event, Ousset's chapter is bound to stimulate the reader's appetite.

Rullière combines a rather prosaic discussion of the applicability of oligopoly models to agricultural markets with a brief but exciting portrayal of the rapid rate at which innovations in technique and organization have begun to change French agriculture. "In the space of a few years, the progress of techniques in agricultural production has accelerated to a point such that bottlenecks braking intensive production on a large scale have been progressively eliminated" (p. 243). One can well believe it and conclude that the postwar renovation of the French economy is now transforming even this most conservative of all sectors.

Pierre Kende discusses the meaning of economic power and argues that its omnipresence makes market solutions normally inferior to those that may be achieved by public intervention. "The analysis of the acts of intervention possible for legislative and planning powers would go beyond the scope of this essay" (p. 330).

The title of the book promises too much. "Modern" turns out to refer to little more than departures from perfect competition. There is no connection with the impressive postwar French studies of applied price and allocation theory, such as those of Boiteux and Massé. Apart from Ousset, there is little concern with systematic study of actual markets and no original quantitative research. Anyone looking for new information on behavior in French or other European industries in response to such changes as the introduction of the Common Market, or to economic planning in France, or to newly established and sustained aggregative growth will be disappointed. The world in which these economists live is far livelier, far more richly varied, than that conveyed by these essays.

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American Industry: Structure, Conduct, Performance. By RICHARD CAVES. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1964. Pp. viii, 120. $3.95; paper, $1.50.

This is an excellent little book. In six brief chapters extending through 111 pages of text, it summarizes in clear and cogent English the most important elements in the problem of industrial organization and public policy, and it does so without resorting to mere description. Richard Caves, who regards industrial organization as a form of applied price theory, is concerned with two sets of related problems: (1) the effects of organization on the allocation of scarce resources, on the efficiency with which business firms utilize resources in supplying consumer wants, on economic growth and stability, on income and employment; and (2) the evaluation of public policies that industrial organization has evoked.

In Chapter 1, "The Business Sector and its Organization" (14 pages), besides setting forth the aims and methods of a study of industrial organization, the author presents data indicating the extent of seller concentration in the American economy—a major characteristic of market structure. In Chapter 2 (20 pages) he examines other important elements of market structure—chiefly product differentiation and barriers to entry—and indicates the circumstances under which they arise and their economic significance. Having sketched the major elements of market structure, he considers in Chapter 3 (17 pages) their significance to the way firms behave: how market structure affects a firm's pricing policies, output, product characteristics, selling and research expenditures—in short, market conduct.

Before turning to the more significant question, how the structure of industry affects the performance of the economy, he reviews, against the background of his analysis of market structure and market conduct, U.S. policy

towards competition. In 14 pages he outlines the relevant provisions of legislation designed to promote competition and outlaw monopoly—the Sherman, Clayton, and the Federal Trade Commission Acts—and shows how the Courts have interpreted the legislative design. With greater brevity (6 pages) he discusses "natural" monopolies and the regulations to which the state and federal governments have subjected them.

Having dissected with professional skill and admirable dispatch public policies designed to promote competition or regulate monopoly, he subjects to similar brief but discerning scrutiny public policy designed to restrict competition. In six pages he shows how, why, and with what effect the government has attempted to bring prosperity to American farmers. He devotes four and one-half pages to the public effort to protect the little business man in the retail sector of the U.S. economy through chain store taxation and the Robinson-Patman Act. In briefer compass he sketches the federal patent program and the effect it has had on innovation, and expresses doubt as to whether it has generated enough progress to compensate for the monopoly it fosters and protects. He concludes that the measures to restrict competition have some justification in the conditions that would prevail in their absence, but raises grave doubts, which many of his professional colleagues no doubt share, of the efficacy of the policies adopted.

In a final chapter of 17 pages he comes to the *raison d'être* of what has gone before. Given the structure of the U.S. economy and the policies that have evolved for its control, he asks how well has it done, how has its actual performance compared with its potential? He concludes that price rigidities and pricing policies in concentrated industries have probably aggravated the problem of maintaining stability in the general price level, that "the link between income distribution and concentration in the economy is both weak and complex," that some level of monopoly is necessary to promote research and innovation, but whether the existing structure provides too little or too much is debatable. On the influence of structure on the economical allocation of resources he speaks with greater confidence. High barriers to entry tend to produce a high profit rate, a sign that the industry contains too few of the factors of production. But industrial performance is to be judged not only by the allocation of resources among industries, but within each industry. Although he finds that the disadvantages of small scale are frequently not crippling, he concludes that 10 to 30 per cent of the capacity of many industries on which data are available "seems to lie in plants too small to exploit the available scale economies."

In a final section (2 pages) evaluating public policy, he expresses the judgment that everyone wants to keep the two basic antitrust laws, the Sherman and Clayton Acts. But Caves is unwilling to commit himself unequivocally either with those who favor further restrictions on market power (in the direction of the Alcoa decision) or with those who would revive the "rule of reason." A "prudent man," of which Caves is a good example, "will not think that it (the evidence) supplies a clear-cut answer." With commendable caution he concludes, however, that the evidence seems to favor a further restriction on market power.

Economists are unlikely to take serious exception to Caves's analysis. The book's chief virtue is also its chief defect. To cover the more important aspects of industrial organization and public policy in such brief compass and do it well requires intimate acquaintance with the literature, familiarity with the tools of economic analysis and skill in their use, and great facility in the art of plain talk. Caves's book reflects all of these. One who undertakes such a task is unlikely to plow much new ground, or to plow deep, or to uncover new truths in the plowing. And he is likely to leave some previously cultivated terrain unharvested. Although he discusses mergers briefly, Caves has nothing to say about the three great merger movements as such—the Great Combination Movement at the close of the nineteenth century, the more expansive movement of the 1920's, and the current movement whose scope and import are not yet clear—but all of which, regardless of the forces that actuated them, have greatly changed the structure of U.S. industry and, for better or worse, its conduct and performance. Each of these movements followed legislation designed to prevent it, and together they have complicated antitrust policy greatly. They merit consideration in a book of this sort. But one can't do everything in 111 pages.

I am not sure for whom the book is designed, but it should be useful as supplementary reading in the introductory principles course or as required reading for all undergraduate majors in economics. Economists generally and nonspecialists interested in public policy could also read it with profit.

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Concentration of Economic Power in India—A Statistical Study of Concentration and Diffusion of Economic Power in India. By S. R. Mohnot. Allahabad: Chaitanya Publishing House, 1962. Pp. xvi, 275. Rs. 20.

This is a solid contribution to our knowledge of economic structures in the world's greatest underdeveloped country; and those interested either in concentration or in Indian economic life will find it profitable to overlook the literary imperfections and the meager footnote citations which make sources hard to check. And if it is mildly amusing that an author strains a bit to avoid value judgments, it is reassuring that in fact he does keep them distinct from analysis, as they should be.

Economic power is defined as ability to achieve or encompass material welfare, the author thus avoiding the usual hazy confusion, so rife in his country, ours, and others, of "power" with a lot of verbal agglomerations which mean simply *absolute size.* Concentration in the manufacturing sector is examined in terms of capacity (measured by value added), market power (measured by percentage of sales), and employment. Mohnot is clear that concentration, without more, is not to be equated to monopoly power or market control, though, like the rest of us, he is not able to get very far from the former to the latter. Frequency distributions by productive capacity for 1946-48, and by employment for 1946 and 1954, suggest a tendency for both the largest and smallest establishments to grow at the expense of the middle-sized (pp. 52-56); though this reader suspects that the latter part of the trend is spu-

rious, due largely to the imperfections of enumeration, of which more shortly. Another approach to capacity is that of concentration by firms in 35 industries, roughly comparable in scope with our four-digit groups. This coverage is of course much too small, but nothing better can be done; the author believes that most industries with very high concentration of rated capacity are "infant" industries. Over the period 1950-61, there was a tendency toward lower concentration among the 35 industries.

Both frequency distributions show very great inequality by the Lorenz measure, though the reviewer considers this of much less interest than the simple fact that in some industries the one or two biggest firms account for the whole industry. But this anticipates perhaps the author's second line of inquiry: shares of sales as a direct measure of market power. In my opinion, share of sales and of value added are complementary, for surely a firm with a high per cent of sales but with a low per cent of value added in its own and adjacent product classes has much less power over price than a firm with high percentages in both—because in the former case the chance of entry is many times greater. In manufacturing, inequality in product market shares increased between 1946 and 1954, but concentration decreased (p. 91). It follows that the number of establishments and firms must have increased considerably, and in fact they did (pp. 92-98). But the number of units, a doubtful enough statistic even in well-developed economies, becomes of no interest in India, where enumeration must be even more inaccurate. Finally, there is employment, where there was a slight increase during 1929-49, both in all manufacturing as well as seven key industries (pp. 117-24).

The writer then turns to such matters as "control" over finance and capital, income inequality, and institutions which counteract or offset the power of large producing units. Here the interest is descriptive, not analytical, since the control of finance is sketchy at best and has little relation to market concentration except as indicating possible barriers to entry, though even this connection is more assumed than proved. Income distribution has no necessary connection with concentration or with monopoly power, nor is any demonstrated here. As for trade associations and cartels, their effectiveness remains to be judged in the particular market in the particular country; we learn little by merely hearing about them. Consumer cooperatives, on the contrary, can and should be analyzed as knowledgeable buyers who can induce or "coerce" competitive behavior among sellers; "countervailing" verbalisms get in the way of this task. As for labor unions and the state, their effect goes in one or the other direction in any given market. Hence the more macroscopic part of Mohnot's study is rather diffuse. But the work does serve as a useful beginning to more basic inquiries: to what extent does concentration exceed what is necessary for efficiency at any given time; are markets growing enough to allow the entry of efficient new firms; what ought policy to aim at in this area? If the reviewer may be allowed a very general comment: an underdeveloped economy is *ipso facto* a monopolized economy, where a working competitive market is the exception, not the rule. But to say that one should get rid of the monopoly elements is not helpful because the only way to do so is by widening the markets—i.e., development and increasing national in-

come, which everybody knows already. But development policy needs always to consider the effects on competition and monopoly of any project or plan, and to do so one needs more information of the kind that Mohnot's work begins to develop.

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Land Economics; Agricultural Economics; Economic Geography; Housing

The Federal Bulldozer—A Critical Analysis of Urban Renewal, 1949-1962. By MARTIN ANDERSON. Cambridge: The M.I.T. Press, 1964. Pp. xiv, 272. $5.95.

In 1949 the federal government launched a program to help cities remove existing slum and blight and to prevent the creation of additional blight, as part of a larger effort aimed at achieving the goal of a decent home and suitable living environment for every American. The objectives of the program have been broadened over the years—for example, to include the strengthening of downtown business areas—and its scope extended. In 1962 there were 1,210 projects in some stage of the urban renewal process, and they were distributed among 636 cities throughout the United States.

This book attempts an evaluation of the program as it evolved up to 1962. It criticizes all aspects of urban renewal and argues that it is not capable of achieving the goals put forth by Congress. It presents the following case against the national program. (1) It has destroyed more housing than it has provided. (2) It has displaced a great many people. The greatest proportion of those forced to move have been members of minority groups. (3) Most of the new housing constructed has been for higher-income families. (4) It "simply shifts slums and thus encourages the spread of slum and blight." (5) It plays an insignificant role in the U.S. economy. (6) It has not increased the tax receipts of local communities as it was supposed to. (7) It has generated relatively little private investment; most of the money comes from the government. (8) Urban renewal takes a very long time, with all sorts of unfortunate consequences. Given these shortcomings, Martin Anderson argues, the entire program should be abolished.

While the study is represented as an objective evaluation of urban renewal, it has much more the character of a presentation of the prosecutor's case, even to a summation at the end knocking down the arguments for ("beliefs") with the arguments against ("facts"). It may be that at one stage Anderson went through a careful and judicious weighing of the evidence, but if that is so, the reader is not taken through the same process, but is provided with the final case against the program only.

From the journalistic-style title of the book to the summation chapter, the clear purpose is to convince the reader to accept the author's conclusion that the program should be repealed. One problem with this argumentative approach is that, in the effort to pile up as strong a case as possible, a good bit of the argument inevitably becomes contrived. Thus there is a chapter whose

purpose it is to prove, on the basis of the number of housing units built under urban renewal as a proportion of total construction, that the program has had little impact on the national economy. But surely this is not a logical objective for a program concerned with slum clearance. If the federal government wanted to encourage a greater volume of construction it would hardly do so under the obvious constraints of redevelopment, but could subsidize construction in open outlying areas. On the other side, on an issue which is of direct relevance to an evaluation of urban renewal—its economic effect on specific localities where major programs have been launched—the book has nothing to say. (One would certainly want to know, for example, what the impact was of the redevelopment effort which initiated the so-called "Pittsburgh Renaissance.")

Similarly, the book takes the federal program severely to task because of the difficulties it imposes on relocated families, mainly because some have to pay higher rents in their new quarters. Unbelievably, it does not ever mention the fact that this is the *only* major program that requires that displaced families must be offered standard housing at prices or rents they can afford in localities convenient to their places of work and which requires financial assistance to displaced families as well. Other public construction programs, and particularly highways, force thousands of families to move without any assistance whatsoever, and of course this is true for private "renewal" as well. Certainly, in fairness, it should be pointed out that, while improvements in relocation practices are undoubtedly much needed, the urban renewal program has introduced a significant humanitarian feature to ease the burden of urban change among families least able to bear it, a feature that might well be extended to other activities that displace poor families.

There is also a question of relevance. The study simply does not get to the heart of the matter. The problems of slum and blight can only be understood against the background of the forces of urban change which bring into being the blighting factors, including the rate of migration to the city from poverty-ridden rural places, the movement of wealthier families to the suburbs, the economic, social, and locational impact of racial discrimination, the economics of housing construction, and the like. The question of the number of housing units built as against the number torn down—which is the issue that concerns Anderson most—is clearly of secondary importance. The appropriateness of the objectives and of the methods employed, in the face of the powerful forces to be dealt with, is at the heart of the problem.

Here, then, is the greatest weakness of the study. There is absolutely no analysis of the nature and causes of blight. For example, there is no reference to the important neighborhood-effect externalities in land-use, such as the issue of property undermaintenance under atomistic behavior, the "Prisoner's Dilemma" of Davis and Whinston. Nor is there any discussion of the question of the economic and social viability of central cities under the impact of ghetto living and suburbanization, with isolation often so strong that intergroup contact virtually ceases and with a flight of the wealthier families, so that the tax base is divorced from service needs. Rather the entire basis for evaluation is the counting of certain end-result numbers.

Unfortunately, even on this secondary issue of numbers, the significant questions are not treated. His case against the urban renewal program rests mainly on the fact that, according to his estimates, the total amount of time involved in a typical urban renewal project is roughly 12 years. Since the study is concerned with the period from 1949 to 1962, and since programs normally take some time to get started, it would not seem to be surprising that by 1962 more houses had been removed than built. (By 1962, some 1,200 projects had been started and only 86 completed.) And then it should not be surprising that great profits had not yet been made by developers and that the cities had not yet enjoyed an increase in tax receipts. (Yet, given Anderson's approach, the simple arithmetic of taxes turns out to be a chapter entitled "The Tax Increase Myth.") Is a 12-year period for completion a fatal weakness? Apart from the fact that the time involved has been reduced year by year (currently the figure is somewhere between four and eight years) given the life of a city, and given the enormous stakes involved, even 12 years would not by itself seem to be an adequate basis for condemning a major program, any more than a forest rehabilitation program would be condemned because it took many years for the trees to reach maturity.

The book is on much firmer ground when it raises the issue of the class character of the urban renewal program. Do its broad and long-term objectives really justify the fact that it has destroyed homes of the poor and built new homes for the higher-income families, or the fact that its impact has been particularly great on minority group families (to the point where the program is sometimes referred to as a "Negro clearance" program). Granted that urban renewal does not have a primary responsibility for adding to the stock of housing for the poorer families, but that this is the responsibility of other public agencies as well as of private enterprise, the question may well be raised as to whether the net impact of the housing and urban development programs taken as a whole is not the matter of chief concern. Would it not be appropriate to require that the federal government as well as the cities accept the responsibility for the over-all impact on the various income classes? And, similarly, should not a greater proportion of the public funds made available for urban renewal be channeled to improving the housing and neighborhoods of the poorer families? Moreover, since it is obvious that the objective of eliminating slum and blight cannot possibly be achieved without very greatly reducing poverty itself, should not new methods be devised whereby the urban renewal program could be integrated with the community development parts of the antipoverty programs, (for example, by helping greatly to improve the public facilities and services available in poorer neighborhoods)?

Certainly, there are enough significant issues about urban renewal to suggest the value of a thoroughgoing and objective evaluation of the program. And the problem of relating the program's objectives to the complicated forces of change at work in our urban communities is difficult enough to challenge any scholar interested in urban affairs. Anderson's book, unfortunately, falls short of the challenge.

Harvey S. Perloff

Resources for the Future, Inc.

The Economics of Regional Water Quality Management. By A. V. KNEESE. Baltimore: The Johns Hopkins Press for Resources for the Future, 1964. Pp. xii. 215. $5.00.

This book embodies the results of one of many investigations into resource economics sponsored by one of our large and well-financed economic research institutions. The author has already tested his skill in this field with an earlier monograph on *Water Pollution* published in 1962. This earlier study is now extended to problems of treatment and administration of water management systems.

Part I attempts to delineate for lay readers essential technical facts about water quality and pollution and control devices. Part II and Chapter 6 of Part III are a somewhat formalistic exposition of the theory of resource allocation applied to waste-disposal problems. Economists will be pleased (or distressed) to see numerous textbook diagrams of rising marginal costs representing incremental costs of waste elimination or abatement and marginal revenues representing costs for water purification by users. The intent of the analysis was to exhibit the layout of resource allocation for industrial water uses in a basin where all users are consolidated under single management so that "external" costs and benefits are "internalized." This internalization would involve as variables location of plant facilities in the basin, use of waste abatement or treatment procedures at the outfall or intake as would be most advisable. In the grand equilibrium the marginal cost of the optimum combination of waste abatement including process and product adjustments at the outfall would equal the marginal reduction in the cost of the optimum combination of damages to users and treatment (Figure 7, p. 130). This optimization would be affected by the size of water flow which varies both seasonally and over the years. A tolerable outfall of pollutants may become lethal at low waters. Hence optimization would need to include devices for waterflow basin management including waste lagoons, storage reservoirs, or aerating devises which step up oxygen levels (see Figure 12-16).

Since industrial use of water now outranks both domestic and agricultural, this design of analysis is fitting. Kneese emphasizes again and again that water-quality management should not be geared to monistic standards of water purity for their own sake or for avoiding pollution but should be determined in the light of the costs of abatement and charges for water purification.

All this substantially leaves out public health requirements and aesthetic considerations for humans, their domestic animals, and fish populations. These were considered crucial by the Water Pollution Control Federation which in October 1963 officially informed a Congressional Committee that public health standards should govern pollution control and that the U.S. Public Health Service was best fitted to administer pollution control. One of the highly successful water-management programs in the Ruhr spells out a fish-pollution standard in terms of the amount of clean water required to dilute effluent in order to avoid harm to the fish (p. 173). Kneese agrees that these requirements and considerations are important (pp. 72ff.). Sewage treatment and purification expenses for fresh-water domestic use of water

may be assimilated into the systems of basin-wide cost-equilibration either by direct costs and charges or quality standards imposed as "constraints upon the cost-minimization objective" (p. 142) or zoning arrangements which would rule out certain classes of water uses to provide in certain areas or stretches of a stream for high-grade recreational and fish life.

Optimum control of water quality could be attained by subsidies (for pollution abatement) or by charges or by a scheme of regulation with permitted uses and procedures. The case against subsidies is clear-cut (p. 196). As between effluent regulation and a tailored array of charges adjusted to measure the amount of external damage actually imposed by different pollutors at different times and locations, Kneese strongly favors the charge philosophy, which is used by many municipalities as a way of apportioning charges for common treatment facilities. Charges permit each pollutor to adjust by withholding wastes in a lagoon or by altering production processes or output design up to the point these adjustments cost less than the charge (pp. 79ff.). Charges could be fixed with less information than quality standards and would yield revenues which could be used to build desirable basin-wide treatment or flow-regulating facilities or rebated to waste-dischargers as a group (pp. 59ff.).

In two interesting chapters Kneese examines programs for controlling salinity in the Ohio River Basin (Ch. 5) and for administering waterflow systems in the eight regional agencies (Genossenschaften) in the Ruhr industrial area (Ch. 7). These agencies, whose members are drawn from local governments and large plants, were given almost complete multipurpose authority over water use in entire watersheds. This chapter should be required reading for persons bearing responsibility for design or operation of regional waterflow systems in the United States and elsewhere.

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Labor Economics

Toward a Wage Structure Theory. By LOUIS R. SALKEVER. New York: Humanities Press, 1964. Pp. vi, 161. $5.00.

Attempts to explain the unequal rewards of labor have provided one of the oldest subjects of economic speculation. Such current controversies as those over job evaluation, low-wage competition, racial discrimination, and the so-called "War on Poverty" suggest the present and probable future importance of this still-beclouded subject. It remains one of the most underdeveloped of all areas of economic reasoning.

Subsequent to Adam Smith's special-purpose theory of wage differences, the nineteenth-century economists became largely preoccupied with the great debate over the alleged exploitation of labor *as a whole* by the landlords (Ricardo) and/or the capitalists (Marx). Thus their reasoning as to income distribution came to be cast into the mold of a functional distribution (of any given national income under equilibrium conditions) as between the labor and

nonlabor production inputs. National income statistics have now made it evident, however, that the property share is both too small and too stable to account for much of the sharp and changing actual differences in individual incomes—contrasts which provide the prime and enduring source of social tensions (and perhaps also of economic and social dynamics). Thus Professor Salkever's comprehensive, critical, and brilliant assessment of the history and contemporary state of wage structure theory is both timely and prophetic.

In eight compact chapters, Salkever traces, documents, and evaluates the history of economic thought related to "the differential rewards for man's labor"—from the Aristotelian precursors of marginal analysis to the groupings of such contemporary authorities as Dunlop and Reynolds for what the latter has defined as a needed "generalized statement, bearing some semblance of reality, of how money wage rates are determined and how changes in these rates . . ." are actually brought about. In his concluding chapter, Salkever first classifies the historical approaches to the problem as having been based on the acceptance of arbitrary power or of pure chance or of equilibrium reasoning. Elements for more dynamic future models are then proposed, involving *inter alia* distinctions between forces of social inertia, forces responsible for the emergence of wage differentials, and forces inducing changes in them.

Salkever is at his best as a perceptive, trenchant critic, reminiscent of a Veblen. Thus "the social romanticism of an all-pervasive marginalism" (p. 141) is brought to earth with eight succinct reasons for its "limited relevance to [either] the formation or change of occupational wage differentials" (pp. 86-87). Again, "Empirical research . . . can cast doubt on prevalent notions but it cannot substitute its data for theory . . ." (p. 106). Bargaining theory might throw useful light on the evolution and change in wage structures, if it could only "free itself from the nihilism of some of its proponents . . . and the logical positivism of other[s] . . ." but, for stated reasons, "attempts to construct elegant models of bargaining may be premature . . ." (p. 121). Neoclassicism "made substantial contributions to analytical procedure . . . [but] added little in the way of newly identified factors important to [the explanation of] . . . wage differences . . ." (p. 127). The games approach "has not yielded significant theorems of wage structure analysis . . . because its simplicity is illusory . . ." (p. 133). In spite of all this, Salkever believes that "the past is more than a bucket of ashes" and thus he sifts something of significance for a "new theoretical system" from each of the approaches whose pretensions he has so devastatingly demolished.

Perhaps the most original element offered for the suggested reconstruction is Salkever's interpretation of "the nature of the learning curve" (pp.139-43), a concept offered somewhat apologetically (note 4, p.143) and one which is obviously limited, like too much of wage structure theory, to the occupational type of wage differences. The entire essay suffers from overcompression, and it is to be hoped that Salkever will provide a much more elaborated treatment, especially of his concluding chapter, so that he may progress further toward his desired "comprehensive and logical analysis of the factors which

appear under particular and defined circumstances to affect wage structure" (p. 154).

N. ARNOLD TOLLES

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Labour in the Tropical Territories of the Commonwealth. By B. C. ROBERTS. Durham: Duke University Press, 1964. Pp. xviii, 411. $6.50.

In this volume Ben Roberts of the London School of Economics has traced the growth of trade unionism (Part I), the trend of labor legislation and administration (Parts II and III), and the development of industrial relations (Part IV) in approximately 32 tropical territories of the Commonwealth in Africa, Asia, and the islands of the Pacific and Atlantic. With such a large number of countries to contend with, and given the length of the period surveyed, it is little wonder the story often becomes confused. There is little evaluation of the events surrounding the rise in the importance of unions, or reference to the economic structure of the countries involved, and one unfamiliar with the situation is apt to receive a somewhat distorted picture of the importance of trade unions. Especially is this the case with respect to the extent that collective bargaining plays an active role in wage determination.

Trade unionism in the colonies developed from a multiplicity of influences. On the one hand, unions were one of the few media through which the indigenous populations carried on campaigns for political independence. Consequently, they were politically oriented from the beginning, and little thought was given to developing them to function as economic bargaining units. Another major influence was the mercantilistic practice of the colonizing powers, with the Colonial Office promoting trade unionism, minimum wage legislation, and employment centers after the pattern in the mother country. In part, this paternalistic attitude reflected policy at home, for after 1920 there was strong pressure to extend to the colonies the same social practices organized labor had won at home. In addition, the Colonial Office very early recognized the growing trade union movement as a force for independence, and decided to regulate it in order to keep it under surveillance. As a consequence, a vast administrative machinery was developed for the registration of trade unions, the supervision of their finances, and the fostering of a belief that they were dependent upon the government for their existence.

There were few trade union movements in the territories which were dependent upon the Colonial government for their existence as a political force for promoting independence. In countries like Nigeria, Ghana, the Rhodesias, Kenya, Tanganyika, Trinidad, and Jamaica, the struggle for independence was in large measure an outgrowth of the trade union movement. However, although the Colonial government was not behind the political motives of trade unions, it did develop and promote an administrative machinery which fostered them as economic administrative units. For almost all the territories the British passed legislation setting up the machinery for collective bargaining, minimum wage legislation, and the whole gamut of welfare policies that have become part of the social structure in advanced countries. Roberts makes

the mistake of viewing this vast administrative machinery as the reality of labor relations and collective bargaining in these countries.

It is extremely doubtful that, as Roberts claims, "today the conditions of employment on estates and plantations sometimes compare reasonably with those found in the best of modern industrial plants or public services" (p. 365); or, that "signs point to the possibility of establishing a viable system of industrial relations that will bear comparison with those existing in the more advanced countries" (p. 403). While these two rather typical statements are vague enough, for "sometime" it might be "possible" for trade unionism in Uganda or Tanganyika to be comparable to the United States or England, at the same time they reflect the author's persistent habit of looking upon legislation, and at reports of union officials and government agencies, as a description of reality. To be sure, the official reports of the Nigerian Ministry of Labour indicate there were about 260,000 trade union members in 1960, but only about 15 per cent of these "members" pay any dues, the leadership is largely political, and only a few hundred work under anything approaching collective-bargaining contracts. Moreover, given the volume of unemployment, the possibility that trade unions can function as economic institutions is practically zero.

Unions, at least in Africa, will continue for some time to be political institutions, and while certainly they will influence events, they will not meet employers at the bargaining table and have a direct voice in the determination of the conditions of employment. Such matters are going to be determined by the governments, not by individual employers. There are few unions in Africa today that could function as economic units. Leaving aside monopoly nuances, the economic base for collective bargaining is grounded in the productivity advances of the workers involved; in Africa these are not significant, to say the least. It is a mistake, in a volume devoted to labor in underdeveloped countries, to ignore the relation of the wage sector to the rest of the economy. Roberts points out there are 700,000 wage earners in Nigeria, a sizable number, but what impresses me is that these 700,000 workers account for about 1 per cent of the population.

Roberts contends that employers and unions in the territories should settle their disputes by "independent collective bargaining." What he does not point out is that, with only limited gains in productivity available, independent collective bargaining may not be a desirable objective. Inflationary and disruptive results will follow from it, especially when it produces an acceleration in consumption which these economies cannot support. In most of the emerging nations the trade unions have played a very important political role, helping to advance the cause of independence. It is naïve, however, to believe they now function as effective economic institutions or that they have ever done so. What Roberts, the ILO, the BTUC, and the AFL-CIO do not seem to recognize is that in most underdeveloped countries there is not a sufficient economic base to support independent collective bargaining at the present time. Apparently, however, the leaders in these countries do recognize that this is the case.

John P. Henderson

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Technology and Woman's Work. By ELIZABETH FAULKNER BAKER. New York and London: Columbia University Press, 1964. Pp. xiii, 460. $8.50.

This study reviews in detail the evolution of woman's work since the eighteenth century in the United States.

The volume is divided into five parts and has an extensive bibliography. Part I begins with Alexander Hamilton's classic report on manufactures in 1791 and traces the development of women's work through the nineteenth century. Part II covers the twentieth-century changes in factory, office, and shop employment. Part III deals with changes in women's professions and professional opportunities. Part IV discusses white-collar unionism. Part V explores the U.S. economy's adjustment to women's work.

Although the study duplicates some information available in other relatively recent works, it brings together in coherent form much far-ranging material. The author with painstaking care draws on historical records, contemporary sources, and detailed statistics to show industry by industry and occupation by occupation how women's employment opportunities over the years have been affected by changing technology. Her presentation carefully relates the impact of technology to the relevant institutional environment, and it acknowledges the importance historically of war-induced labor shortages, education, and the plain willingness of women to work for less as critical factors in enlarging women's employment and career horizons. What emerges is a detailed socioeconomic chronicle of the important progress of American women toward the day when "sex-typing" of occupations shall have reached the irreducible minimum.

The study's great value is not so much in what it makes possible in the way of new generalizations. It is, rather, in the great detail which it provides that the work excels and makes a contribution. There are some errors, as for example when James B. Carey is identified as president of the UE instead of the IUE (long-standing rival labor organizations), but in general the errors do not mar the work. Some of the interesting matters treated include: How did the development of the principle of interchangeable parts and the arrival of electrical energy as a power source affect women at work? Here may be found the answers, industry by industry, in rich detail. Likewise, the impacts of the typewriter, the telephone, the radio tube, and the evolution of mass merchandising in providing light and attractive employment for women are explored in depth. What have been the salient features of the growth of teaching and nursing—the two professions whose membership has been predominantly female? The author traces them from their beginnings. What has been the relationship of working women in manufacturing to organized labor and collective bargaining? The author deals with this by industry in historical context, but she also unfolds the developments among white-collar and professional women employees, including, for example, a glimpse of the struggle between the American Federation of Teachers and the National Education Association, as well as a description of the on-going interest of the American Nurses Association and its district affiliates in economic security and collective bargaining. The discussion about organization of the white-collar worker may be somewhat oversimplified, but it is not without merit. What role has

protective labor legislation played in women's working lives? Legislative developments from the early nineteenth century state enactments to the Equal Pay Act of 1963 are discussed.

The work presents a very fair and accurate portrayal of the extent to which over the years the weight of tradition and prejudice have worked against the consideration of women on their merits for the more desirable and important occupations. The author acknowledges that there are certain inherent risks associated with the hiring of women for high-level positions, and they surely need no elaboration here. But the author points out that many women have risen to high places, and "women have far more in common with men than society has yet been willing to accept . . ." (p. 441). She concludes, "(S)urely in this age of revolutionizing technology more and more education is the most promising route" (p. 442) to overcoming the cultural lags which still keep women from receiving fuller opportunity and recognition in the learned professions, science, engineering, and administration and which impede a "Declaration of Independence" in the home and the market place. Without any intention of disagreeing with this, perhaps it might be added in closing that the evidence she has so masterfully presented as to the impact of changing technology on woman's work may justify the hypothesis that nothing is more likely to contribute to the future improvement of woman's occupational and cultural opportunities than the continuing advancement of technology itself.

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Population; Welfare Programs; Consumer Economics

The Economic Value of Education. By T. W. SCHULTZ. New York and London: Columbia University Press, 1963. Pp. xii, 89. $3.00.

Economic Aspects of Education: Three Essays. By W. G. BOWEN. Research Report No. 104. Princeton: Industrial Relations Section, Department of Economics, Princeton University, 1964. Pp. 128. $3.75; paper, $3.00.

External Benefits of Public Education: An Economic Analysis. By B. A. WEISBROD. Research Report No. 105. Princeton: Industrial Relations Section, Department of Economics, Princeton University, 1964. Pp. 143. $3.75; paper, $3.00.

The little volume, *The Economic Value of Education,* is a monograph commissioned by the Ford Foundation for the purpose of reviewing the rapidly growing work on the economics of education. Predictably, the job was entrusted to T. W. Schultz, and the result is a compact, yet lucid, exposition of a complex subject, and a most useful, extensive pre-1963 bibliography.

As an economic quantity, education appears in three economic relations: (1) as an output in the production function of the education industry (e.g., of schools); (2) as an input in individual, firm, and aggregate production functions; and (3) as an input to individual utility and social welfare functions. Schultz directs his attention primarily to major aspects of the second and third relations. These he treats in a framework of investment analysis, in

which costs are related to returns. In the discussion of concepts of costs, returns, and rates of return, and in the reporting on empirical work, Schultz strongly distinguishes between private and social accounting and between consumption and production components of the returns to educational investment.

Schultz is inclined to believe that private rates of return exceed social rates, because of public subsidies to education. This is not logically necessary, since taxes and positive external effects may produce the reverse. If true, however, a tendency toward private equilibrium would imply a tendency to overinvest from a social point of view. The empirical evidence, at least for the United States, shows no such tendency. The challenge for research, with strong implications for policy, lies in the exploration of forces which thwart this "incentive to overinvest."

Schultz sees the major promises of the developing economics of education in (1) its relevance to the normative issues of optimum allocation of investments about which political decisions are made continuously, (2) the insights it can provide into labor force behavior and the wage structure, and (3) the insights into processes of economic growth. He is particularly sanguine about the latter when he says: "Economists who are analyzing the investment attributes of education are laying the foundations for an economic growth policy which assigns a major role to schooling and to the advance in knowledge that is won by those in the educational establishment" (p. 19). This statement comes dangerously close to a preconceived conclusion. However, in the suggested inventory of unsettled issues Schultz rightly leaves the question open: Are the improvements in skill and knowledge of the labor force a prerequisite or a consequence of economic growth?

The above, incidentally, is not quite the same as the question about the extent to which education is an investment or consumption good. On this issue Schultz conjectures that the omission of consumption components in estimated returns to education is likely to impart a sizable downward bias to the calculated rates of return. In consequence, the use of calculated rates would bias allocative decisions toward insufficient investment in education. But this conclusion is not necessarily correct: the consumption components may be sizable and positive on the average, but this is less likely to be the case at the margin, so long as the nonpecuniary elements are valued differently by different people.

Among many other topics that await exploration, Schultz notes the importance of studying the allocative efficiency of the "schooling industry." He also remarks, in passing, on the multiproduct nature of this industry which combines the transmission of knowledge, the creation of new knowledge, and the search for talent. These are issues of utmost importance, though seemingly less accessible to economists.

In the first of his three essays, W. G. Bowen covers partly the same ground as Schultz in surveying the analytical problems encountered in the attempts to assess the economic contribution of education. Unencumbered by bibliographical, empirical, or definitional details, he presents a well-organized statement of advantages and disadvantages of various approaches to the problem,

with major emphasis on the investment approach. Bowen's essay makes for profitable reading, in many respects complementing Schultz's discussion.

Of the approaches alternative to the investment analysis Bowen mentions correlation techniques in which some indexes of economic activity (output) are related to education in simple or partial correlation with other factors. A particularly important example is the aggregate production function in which education could be specified along with other traditional and not-so-traditional factors as one of the determinants of output. Denison's work in this area is an illustrative starting point. Perhaps overly impressed with the difficulties with which this road is beset, Bowen fails to point out the major promise of a framework which, in principle, permits the calculation of a social rate of return, regardless of market organization and of external effects. Bowen lists and criticizes another approach, the "forecasting of manpower needs." Despite the notorious unreliability of such forecasting, this approach is popular since it supplies apparently specific guides to policy. As Bowen points out, once questions are raised about criteria of "needs" and the relevance of considering costs that have to be incurred to fill these "needs," it is realized that apparent numerical precision is no substitute for analytical validity.

The second of Bowen's essays deals with comparative advantages and disadvantages in methods of university financing in the United States and in Britain. The third essay is an empirical analysis of the effects of unequal pressures of nonacademic employment opportunities on academic salaries in different subject fields in Britain, in the presence of nominally uniform salary scales. The effects are predictable: uniformity persists in name only.

In the *External Benefits of Public Education,* B. A. Weisbrod catalogues a large set of benefits accruing to persons other than the student and his family. These are listed mainly in Chapter 3, whose content already appeared in Weisbrod's paper on "Education and Investment in Human Capital" in the October 1962 *Supplement to the Journal of Political Economy*. It is not clear why all the externalities listed are benefits, and none are losses. This bias is probably shared by most educators, though a smaller number would go as far as to say that "it is possible to associate virtually every desirable aspect of life with education" (p. 45).

If all (or most?) of the external effects are on the plus side, the calculated social rates of return are too low. More than that, Weisbrod argues that private returns to education have also been underestimated in the usual computations. These take account of the additional earnings resulting from completion of a given level of education, but they disregard the value of the "option" to obtain still further education. The value of the option is the probability of its being exercised multiplied by the expected gain, if exercised. The gain is the excess of the return on the additional education over the return obtainable on the best comparable alternative investment. This reasoning yields an illustrative calculation, in which it is implicitly assumed that the probability of benefiting from the additional gain is the same for those who stopped at a particular level of education as for those who go on. There is no doubt that for many "dropouts" the potential gain from continuing is higher than for many of those who went on. But, on the average, surely the opposite

is true. There is evidence that a positive selection (though an imperfect one) by "ability" does take place. Therefore, the calculated rates (unadjusted for "ability") may, on balance, have an upward rather than downward bias.

Aside from Chapter 3, most of the study is devoted to an analysis of benefits not captured by public investors, such as local communities which bear the costs of public education. In view of these local public investments in education, migration flows result in ("external") gains and losses to the particular communities. This is the interregional "brain-drain," complete with "spill-outs, spill-ins, and spill-overs." Despite the dripping terminology, Weisbrod is not to be accused of raising the banner of neo-mercantilism with the battle cry against the exportation of precious gray matter. He realizes that this is too narrow a perspective for a normative analysis of (human) capital movements. Rather, his purpose is to explain the secularly rising percentage of public education expenditures coming from higher than local levels of government.

The issues Weisbrod raises are interesting. His contribution and the two other studies here discussed, while not breaking any particularly new ground, are stimulating and thought-provoking.

Education is not a free good, and knowledge is power. These are platitudes, not revelations. Yet the implication that the economic aspects of education can and should be subjected to analysis was not given full attention until recently. Why the rapid growth of interest now, after a long history of neglect? After all, labor as human capital is surely not a more palatable concept than labor as a commodity. Certainly the law, while condemning both, leaves no doubt as to which is the greater evil. I suggest that the question be given priority in Schultz's inventory of unexplored issues.

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The Economics of Health and Medical Care—Proceedings of the Conference on the Economics of Health and Medical Care, May 10-12, 1962. (Sponsored by the Bureau of Public Health Economics and Department of Economics, University of Michigan.) Ann Arbor: University of Michigan, 1964. Pp. viii, 321.

The purpose of the book and the conference was, presumably, not only to stimulate additional economic studies in the fields of health and medical care but to explore the possibility of developing a systematic subspecialty in medical economics. The rationale for such a development, according to Selma Mushkin, conference chairman, lies primarily in the economist's new-found interest in the relation of health expenditures to economic development and in the increasing importance of the public sector of the medical economy with its greater challenge to rational planning. The purpose is to be heartily applauded. The results are mixed.

On the positive side is the quality of some of the individual contributions and the broad range of interests represented. Starting with an introductory paper ("Why Health Economics?"), the volume is divided into sections on

"Organization and Financing of Health Services," "Demand, Costs, and Prices of Health Care," "The Microeconomics of Health Care," "Investment in Health," and a concluding "Agenda for Research."

Individual papers range in subject matter from Duncan MacIntyre's pragmatic defense of the trend to experience rating in health-insurance pricing to an undocumented assertion of the advantages of national compulsory health insurance. In approach, they vary from extreme preoccupation with technical problems of measurement, e.g., the Mushkin-Weisbrod attempt to ascertain total health expenditures per capita over the working life of the 1960 work force, to Eli Ginzberg's conclusion, "The best mix for medical economics would include 1 part of economic concepts to 10 parts of institutional knowledge to 20 parts of social judgment."

If this suggests a *smörgasbord* rather than a well-ordered table d'hôte, it does not follow that the result is either unpalatable or lacking in nutrition. On the contrary, there are some findings of considerable significance to health-care planners.

One of the most dramatic is Nora Piore's conclusion that public funds are now paying for over one-third of the health care received by New York City residents. Convinced that such expenditures will inevitably increase, Mrs. Piore makes a plea for more systematic meshing of public services with total community medical-care arrangements, and especially for an intensive effort in one area where the cities have relative freedom of action—the structure of the public medical-care establishment.

Perhaps the most intractable element in the medical costs of the nation at large, as well as of New York City, is hospital care—the subject of Herbert Klarman's paper. Using trend data for New York City, he examines the three most common explanations for the unabating rise in patient-day costs: (1) the correction of lag in wages and working conditions, (2) the advance in medical science, and (3) lag in productivity gains. While acknowledging some validity in the first two hypotheses, he says they are minor factors and that the chief responsibility lies with the third. The implications are extremely serious. For, as the author points out, with hospitals lagging in productivity gains behind other industries but more or less competitive with them for employees, every productivity gain in the economy at large will adversely affect hospital costs.

This reviewer takes exception to only one major point in this interesting paper: the apparent implication that hospitals not only *have not* but *cannot* increase their productivity. To my knowledge, no really adequate test of this hypothesis has ever been made by U.S. voluntary hospitals, and I do not believe it can be made so long as authority in these hospitals is so fragmented between the three major power groups—medical staff, board, and administration.

Despite the excellence of a number of the papers, the over-all impression of the book is disappointing. The whole seems less than the sum of the parts. Perhaps this is partly because it appears unconscious of, or deliberately to ignore, the many previous assays at medical economics. It is inaccurate to

suggest that interest in economic studies of health and medical programs stems primarily from current interest in economic development. Interest in health economics long antedates interest in economic development.

It is impossible to understand the problem of medical economics as an academic specialty without taking into account two factors: (1) The long-standing hostile professional environment, including the opposition of the AMA not only to "lay interference" in medical and health matters but even to the very concepts of planning, resource allocation, or even rigorous benefit-cost analysis, and the effect of such opposition in discouraging young economists concerned with professional career and status. (2) As these external deterrents have gradually been modified and the role of the non-MD in medical care has increased, partly by default, the interest of the economics profession in policy issues of all kinds has declined. This well-known phenomenon appears to apply with special force to the health and medical fields, where institutional factors are so overriding and, in many respects, unique, where data are so scattered and lacking in comparability, and where the investment of time and energy required for mastery often appears disproportionately great.

The results of this continuing divorce between one of the most challenging public policy areas of our day and the academic discipline which, to this reviewer, has potentially most to offer with respect to rational policy-determination, are unfortunate. Most dramatically, vast new public and private medical-care programs are being designed and put into effect almost without benefit of assistance from professional economists. As the opposite side of the same coin, the Michigan conference, the most systematically planned and financed meeting devoted exclusively to the economics of medical care in the past decade, included not a single paper on the major relevant issue of our time—provision of care for the aged—and gave it only the most perfunctory mention in the lengthy recommended agenda for future research.

Clearly there are no Commons, no Millises, no Slichters, to attract and inspire young economists to grapple with a new social and academic frontier as these men did in the field of labor economics at its inception. The new hero is Technical Proficiency, symbolized by the computer. The result may well be a flow of increasingly competent studies, a growing number of well-trained young economists prepared to conduct such studies and to provide other useful but limited services to the policy-makers, whoever they may be. But this is far from saying that we are likely to witness the birth of an exciting new field called Medical Economics.

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Electronic Computation of Human Diets. By VICTOR E. SMITH. East Lansing: Bureau of Business and Economic Research, Graduate School of Business Administration, Michigan State University, 1964. Pp. xvii, 234. $8.00.

This book reports a significant piece of research. Professor Smith has clearly carried his investigations much further than previous works in this field.

The reported research is consistently workmanlike and, as a result, is a valuable contribution to the literature which will be of interest to scholars in several fields.

Smith uses linear programming as his major analytic technique. He first develops a simple model in which only nutritional constraints must be met. Next, he attempts to expand his viewpoint by injecting palatability constraints into the analysis. In this reviewer's opinion this section contains Smith's most important contributions. In attempting to handle the palatability problem, he has attacked the major limiting feature of previous works. That he does not treat the subject in a completely satisfactory manner is more a testimony to the difficulty of the problem than an indication of inadequate attention on the part of the researcher. The remaining sections of the book are devoted to a series of analyses made possible by Smith's linear-programming approach. He discusses, for example, alternative sources and costs of individual nutrients, the costs to a consumer of particular habits and preferences, and the impact of seasonal adjustments on minimum-cost diets. In a concluding section Smith discusses the uses to which this type of research can be put.

The question of how to minimize cost, subject not only to meeting specific nutritional requirements, but also to meeting standards of palatability has been discussed in the literature, but to date it has not been treated adequately. Smith, in his most important section, considers the subject in detail. In his literature review he discusses several novel approaches, including an attempt to estimate the disutility of consuming excess amounts of particular foods and J. A. C. Brown's attempt at developing a model which would produce a diet corresponding to actual British working diets. Smith's approach to the problem is admittedly pragmatic. He argues that a pragmatic approach is the only feasible one for the near future.

It is difficult to disagree with this point of view. It is reasonable, however, to consider the implications of Smith's approach. Smith injects palatability into his model by adding constraints which are designed to force the diet into a pattern that corresponds more or less to conventional consumption habits. He adds maximum limits to prevent satiation, minimum requirements to insure that common food preferences are not ignored, and complementary restrictions in recognition of the fact that some foods are more palatable in combination than when eaten alone. These are, I suspect, all important aspects of palatability, and their addition does affect his results significantly. Smith points out, for example, that flour rapidly loses it superiority as an economical food when complementary goods must be purchased in order to use the flour in palatable ways.

It is disturbing, however, that in using maximum and minimum constraints based on observed consumption patterns one is restricting alternatives drastically. Given a change in price, or the introduction of new commodities, for example, the mix of items a family purchases may change significantly. Smith recognizes this possibility. He does not, however, recognize the inconsistency of this approach to one of his major claims for his model, namely, that it can provide insights into *unique combinations* of foodstuffs and therefore should

be useful in pointing out novel and cheaper menus to consumers. Making the point directly: if we constrain a model on the basis of current consumption habits we restrict severely the possibility of finding novel and palatable diets. In using this approach we must be assuming that the consumers are now doing a fairly good job of making these decisions. If this is true, then one wonders why we need a relatively complicated computational technique which may do just a little bit better than habit and the shoppers' informal calculus. Alternatively, we are, as I suspect is the case, eliminating a number of potentially palatable menus from consideration. It must be concluded that this area is still an important piece of unfinished business. Using past consumption patterns as a method of introducing palatability considerations into the analysis is simply too restrictive.

Several years ago this reviewer had an opportunity to comment on an early report by Smith on this work. At that time I criticized him for not devoting enough attention to the potential uses of this technique. It is satisfying to be able to report that in his book Smith has a detailed discussion of these potential uses.

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Labor Economics

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Population; Welfare Programs; Consumer Economics

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NOTES

Opposition to the Quality Stabilization Bill

In 1963 bills "to promote quality and price stabilization" were introduced in both the House and the Senate. Both bills authorized a manufacturer of a product with a brand or trade name to establish the price or price range at which such a product could be sold by wholesalers or retailers, provided only that similar products were being marketed. The manufacturer was authorized to prevent his products from being sold below the price he had established by (1) withdrawing the retailer's right to sell his product for a period up to one year; (2) by obtaining an injunction against the retailer; and (3) by suing the retailer for damages. Suit against the retailer was authorized in the Federal Courts of any district where the merchandise in question was offered for sale. The only defense open to the retailer was to establish that the manufacturer or wholesaler involved had been delinquent in enforcing his rights against other violators, or had sold the same type of goods to others for more favorable prices.

On December 30, 1963 a request by six economists (Roy Blough, Arthur R. Burns, Charles H. Hession, Charles Hoffman, Henry H. Villard, and Harold L. Wattell) for signatures in opposition to the then proposed "Quality Stabilization Bill" (H.R. 3669) was mailed to approximately 1,200 economists. In addition, economists attending the 1963 convention of the American Economic Association were (rather erratically) given an opportunity of indicating their opposition. To the mailing only one reply was received expressing approval of the bill and one letter was returned without comment. In all (including the response both to the mailing and the solicitation at the convention) more than 570 economists indicated their opposition to the bill, including 2 former chairmen of the Council of Economic Advisers, 80 deans, 102 heads of departments, and at least 255 full professors (in not all cases was rank indicated).

For the record, the mailing was financed by a group of small business chains loosely formed into an *ad hoc* committee.

At the same time statements signed by 159 lawyers and by 64 distinguished Americans were also obtained. These three statements were circulated among the members of the Senate Commerce Committee and the House Rules Committee. The Senate Commerce Committee voted down the bill. The House Rules Committee did not act upon it at all. In the opinion of the public relations firm which had been retained by the *ad hoc* committee, up until the submission of the three statements the House Rules Committee was expected to vote in favor of the bill and the relevant Subcommittee of the Senate Commerce Committee was also believed to favor it, so that the firm is convinced that the three statements were responsible for killing the bill.

Henry H. Villard

New Journal

In 1965 a new journal *Ekonomicko-matematický obzor* (*Review of Econometrics*) will be published by the Czechoslovak Academy of Sciences (Economic Commission and Laboratory of Econometrics). The journal will carry original papers by Czechoslovak and foreign authors, reports on the practical application of econometric methods in a planned economy, book reviews, and notes. Major papers will be published in Russian, German, French (with English summaries), or English. The first number will be issued in March, 1965. Annual subscription is $6.00. Write to the Editor, Ekonomicky Ustav, Ceskoslovenska Akademie Ved, Tr. politických vesnu 7, Prague, Czechoslovakia.

AER Manuscripts

The following manuscripts have been accepted for publication in subsequent issues of the *American Economic Review:*

A. Ando and F. Modigliani, "The Relative Stability of Monetary Velocity and the Investment Multiplier"

M. DePrano and T. Mayer, "Tests of the Relative Importance of Autonomous Expenditures and Money"

M. Friedman and D. Meiselman, Reply (to previous two articles)

T. Dernberg and K. Strand, "Hidden Unemployment 1953-1962: A Quantitative Analysis by Age and Sex"

P. Diamond, "National Debt in a Neoclassical Growth Model"

R. Fels, "The U.S. Downturn of 1948"

B. Fleisher, "The Effect of Income on Delinquency"

G. Garb "The Machlup-Samuelson Deadlock"

H. Gilman, "Economic Discrimination and Unemployment"

P. Horvitz, "A Note on Textbook Pricing"

Ronald Jones, "Neutral Technological Change and the Isoquant Map"

H. Miller, "A Note on the Estimation of Lifetime Income"

M. Paglin, "Surplus Labor and Development: Facts and Theories"

E. Phelps, "Second Essay on the Golden Rule of Accumulation"

F. M. Scherer, "Firm Size, Market Structure, Opportunity, and the Output of Patented Inventions"

Announcements

The National Institute of Social and Behavioral Science, in collaboration with the Section on Social and Economic Sciences of the American Association for the Advancement of Science, will hold sessions for contributed papers at the annual meeting of the AAAS in Berkeley, California, December 26-31, 1965.

Association members interested in presenting a paper at these sessions should forward titles and abstracts of some 300 words not later than September 1 to Donald P. Ray, Director, National Institute of Social and Behavioral Science, 863 Benjamin Franklin Station, Washington, D.C. 20044.

Papers should be based on research current or recently completed by the author Subjects may concern any aspect of national or international economic problems. Topics of a multidisciplinary nature are also of interest.

Selected materials of the sessions will be published by the National Institute. The annual meeting of the AAAS is the largest professional meeting in the world embracing all branches of science, and the meeting receives extensive coverage by communications media through the participation of science writers, including social science writers. The American Economic Association is an affiliate member of the Section on Social and Economic Sciences of the AAAS.

A request has been received from the Cebu Graduate School, University of the Philippines, asking members of the American Economic Association to assist in building up its library resources by sending, as gifts, old issues of economic journals and theoretical economic books to this address: George Guy, U.P. Cebu Graduate School, Lahug, Cebu City, Philippines.

The New England Council announces its 1965 Publications Prize Program for the selection and publication of outstanding research of economic development relevant to New England. Dissertations and manuscripts dealing with the process of regional economic change in general and New England in particular are eligible. For more detailed information write to Rudolph W. Hardy, Director of Economic Research, The New England Council, 1032 Statler Office Building, Boston, Mass. 02116.

Deaths

Quincy Adams, The American University, October 8, 1964.

Milton S. Briggs, May 1964.

Wayne F. Caskey, August 29, 1964.

Walter Wilson Jennings, University of Kentucky.

Malcolm Keir, Dartmouth College, December 18, 1964.

Albert J. Raebeck, Brooklyn College, November 27, 1964.

Ray Addison Sigsbee, March 18, 1964.

Walter F. Willcox, Cornell University, October 30, 1964.

Retirements

Charles Weisheit, Economics Department, St. Olaf College.

Forrest A. Young, chairman, department of economics and business administration, Macalester College.

Visiting Foreign Scholars

Frank Hahn, Churchill College, Cambridge University: visiting Hill Foundation Professor, department of economics, University of Minnesota, April, 1965.

Promotions

Morton S. Baratz: professor of economics, Bryn Mawr College.

Thomas Bruce Birkenhead: assistant professor, department of economics, Brooklyn College.

Stanley I. Buchin: associate professor of business administration, Harvard Graduate School of Business Administration.

Joseph Craycraft: associate professor of economics, College of Business Administration, University of Cincinnati.

Richard Day: associate professor, department of economics, University of Wisconsin.

Manoranjan Dutta: associate professor of economics, Rutgers—The State University.

Denis A. Flagg: professor economic, San Diego State College.

David Freytag: assistant professor of economics, College of Business Administration, University of Cincinnati.

Stanley L. Friedlander: assistant professor, department of economics, City University of New York.

Marshall Goldman: associate professor of economics, Wellesley College.

Edward Greenberg: associate professor of economics, Washington University, St. Louis.

Charles H. Hession: professor, department of economics, Brooklyn College.

Edmund R. Hill: associate professor of economics and business administration, Gettysburg College, September 1, 1965.

Raymond W. Hooker: associate professor of economics, University of Wyoming.

Erich Isaac: associate professor, department of economics, City University of New York.

Peter d'A. Jones: associate professor of economic history, Smith College.

Edward D. Kalachek: associate professor of economics, Washington University, St. Louis.

J. William Leasure: associate professor of economics, San Diego State College.

John V. Lintner: George Gund Professor of Economics and Business Administration, Harvard Graduate School of Business Administration, April 1, 1964.

Howard McBride: associate professor of economics, College of Business Administration, University of Cincinnati.

Basil J. Moore: associate professor of economics, Wesleyan University.

Richard F. Muth: professor of urban economics, Graduate School of Business, University of Chicago, June 1, 1965.

Walter C. Neale: professor of economics, University of Texas.

Carl A. Nordstrom: associate professor, department of economics, Brooklyn College.

Richard W. Poole: professor of economics, Oklahoma State University.

Robert Riley: assistant professor of economics, College of Business Administration, University of Cincinnati.

William E. Saupe: assistant professor of economics, Iowa State University.

Eric Schenker: professor of economics, University of Wisconsin-Milwaukee, September, 1965.

Arthur Schleifer, Jr.: associate professor of business administration, Harvard Graduate School of Business Administration, July 1, 1965.

Hans O. Schmitt: associate professor, department of economics, University of Wisconsin.

Morris Silver: assistant professor, department of economics, City University of New York.

J. Marvin Skadberg: assistant professor of economics, Iowa State University.

George J. Staller: associate professor of economics, Cornell University, July 1, 1965.

Pan A Yotopolous: associate professor of economics, University of Wisconsin-Milwaukee, September, 1965.

Stanley L. Warner: associate professor of economics and business Claremont Graduate School and University Center.

Waldemar Zagars: professor of economics and business administration, Gettysburg College, September 1, 1965.

Administrative Appointments

Ralph Andreano: director of undergraduate programs, associate professor of economics, University of Wisconsin.

Henry W. Briefs: associate professor and chairman, department of economics, Georgetown University.

Richard Goode, Brookings Institution: director, fiscal affairs department, International Monetary Fund.

Richard M. Gordon: associate professor, department of business management and director, Institute of Management, Old Dominion College.

Roy J. Hensley: acting head, department of finance, College of Business Administration, Lehigh University.

Charles L. Leven: professor of economics, Washington University, St. Louis; chairman, Faculty for Urban Studies and Regional Science; director, Institute for Urban and Regional Studies.

Geoffrey H. Moore: director of research, National Bureau of Economic Research, June 1, 1965.

Roger F. Murray: vice president and economist, Teachers Insurance and Annuity Association of America.

William F. Railing: professor and chairman, department of economics and business administration, Gettysburg College, September 1, 1965.

Gaston V. Rimlinger: professor, chairman, department of economics, Rice University, academic year 1964-65.

Dwight E. Robinson: chairman, department of general business, College of Business Administration, University of Washington.

Leonard G. Schifrin: associate professor of economics and head, department of economics, College of William and Mary, September 1965.

George H. Sorter: director of doctoral programs, Graduate School of Business, University of Chicago, July 1, 1965.

Jerome Strong: chairman, economics department, Hartwick College.

Eugene L. Swearingen: vice president for development, Oklahoma State University.

Phillip S. Thomas: chairman, department of economics, Kalamazoo College.

H. Edward Wrapp: associate dean for management programs, Graduate School of Business, University of Chicago.

Appointments

John Q. Adams, III: instructor of economics, University of Maryland.

Robert F. Adams: research associate, Bureau of Business and Economic Research, and assistant professor of economics, University of Maryland, August 16, 1965.

Hugh G. J. Aitken: professor of economics, Amherst College.

William P. Albrecht: assistant professor of economics, University of Iowa.

Bruce T. Allen: instructor, department of economics, Michigan State University, September 1, 1965.

Philip D. Arben: manager of corporate evaluations, Armour and Company.

Robert P. Armstrong: assistant professor of economics, Williams College.

Peter Asch: assistant professor of economics, Rutgers—The State University.

Richard D. Auster: assistant professor of economics, City University of New York.

Richard M. Bailey: assistant professor, School of Business Administration, University of California, Berkeley.

Leo I. Bakony, Southern Methodist University: staff, Economic Council of Canada, July 1, 1965.

Walter B. Bennett: assistant professor, department of business management, Old Dominion College.

Herbert M. Bernstein: associate professor of economics, Washington and Jefferson College, September 1965.

S. A. Billon: associate professor, School of Business and Economics, University of Delaware.

John M. Brazzel: assistant professor, department of economics, University of Missouri.

Anne Mayhew Brown: assistant professor of economics and education, University of Illinois.

William R. Bryan: Federal Reserve Bank of St. Louis and visiting professor of economics, Washington University, St. Louis.

Elwyn M. Bull: economics and costing division, Research Analysis Corporation.

Anthony S. Campagna: instructor in economics, Rutgers—The State University.

Hollis Chenery: department of economics, Harvard University, February 1, 1965.

Miltiades Chacholiades, M.I.T.: assistant professor of economics, Graduate School of Business Administration, New York University, September 1965.

Veerappan Chetty: instructor in economics, University of Wisconsin-Milwaukee, February 1965.

James W. Christian: assistant professor of economics, Iowa State University.

Geoffrey Churchill, University of North Carolina: assistant professor of managerial controls, School of Business, The University of Kansas.

Joel Clark, Old Dominion College: economist, USAID, Laos.

Steven Cohen: assistant professor, department of economics, Brandeis University.

William E. Cole: assistant professor of economics, University of Tennessee.

Michael D. Copeland: assistant professor, department of economics, City University of New York.

George Daly: assistant professor of economics, Miami University, Ohio, September 1965.

Coldwell Daniel, III: professor of economics, University of Houston.

Irene Davidson: instructor, department of business management, Old Dominion College.

William S. Davis, III: lecturer in economics, Rutgers—The State University.

Stanley L. Dolins: assistant professor of economics, U.S. Air Force Academy.

Henry J. Donnelly, Jr.: professor, School of Business and Economics, University of Delaware.

J. Malcolm Dowling: assistant professor of economics, University of Colorado, fall 1965.

Daniel M. Driscoll, Jr.: lecturer in economics, Rutgers—The State University.

Richard B. Du Boff: assistant professor of economics, Bryn Mawr College.

Zabioullah Eltezam, University of Houston: Royal Afghan Ministry of Commerce

Edward C. Ettin, Duke University: Board of Governors of the Federal Reserve.

Harold W. Fox: instructor in economics, Rutgers—The State University.

Fred R. Glahe: assistant professor of economics, University of Colorado, fall 1965.

John J. Griffin, Jr., New York University: instructor in economics, Marist College.

Clifton M. Grubbs, University of Colorado: associate professor of economics, University of Texas.

Geoffrey B. Hainsworth: assistant professor of economics, Williams College.

Joseph E. Haring: Richard W. Millar Associate Professor of Economics and Finance, Occidental College.

Floyd K. Harmston: professor, department of economics, University of Missouri.

Samuel L. Hayes, III: assistant professor of finance, Graduate School of Business, Columbia University.

John Herzog, University of Wisconsin: associate professor of business economics, Claremont Graduate School and University Center.

W. Whitney Hicks: assistant professor, department of economics, University of Missouri.

David Hitchin: assistant professor of international business, School of Business, The University of Kansas.

Cornelius Hofman, Idaho State University: associate professor of economics, Utah State University, academic year 1965-66.

David S. Huang, University of Texas: associate professor of economics, Southern Methodist University.

Joseph W. Hunt, Jr.: instructor, School of Business and Economics, University of Delaware.

Alice Hanson Jones: lecturer in economics, Washington University, St. Louis.

Max B. Jones: associate professor, department of business management, Old Dominion College.

Peter Kilby: assistant professor of economics, Wesleyan University.

Jan Kmenta: associate professor, department of economics, Michigan State University, September 1, 1965.

Iwan Koropeckyj: assistant professor of economics, School of Business Administration, Temple University.

Helen M. Kramer: instructor, department of economics, Brooklyn College, September 1965.

Henry M. Levin: instructor of economics, Rutgers—The State University.

Carl E. Liedholm: instructor, department of economics, Michigan State University, and economic adviser, Economic Development Institute, University of Nigeria, July 1, 1965.

Peter J. Lloyd: assistant professor, department of economics, Michigan State University, September 1, 1965.

Millard F. Long: associate professor of business economics, Graduate School of Business, University of Chicago, July 1, 1965.

William A. McCleary, III: assistant professor of economics, Williams College.

Kenneth McLennan: lecturer in economics, Rutgers—The State University.

Charles B. McLure, Jr.: assistant professor, department of economics and business administration, Rice University, academic year 1965-66.

Paul J. McNulty: assistant professor of business, Graduate School of Business, Columbia University.

Michael S. Malone: assistant professor of economics, Washington and Jefferson College, September 1965.

Carmen F. Menezes: assistant professor, department of economics, University of Missouri.

Paul B. Miller: assistant professor, department of economics, Texas A & M University, academic year 1965-66.

Hyman Minsky, University of California, Berkeley: professor of economics, Washington University, St. Louis.

James C. Moore: assistant professor, department of economics, University of Missouri.

Rodney Morrison: instructor in economics, Wellesley College, 1965-67.

Austin S. Murphy: managing director, Savings Banks Association of New York State.

Jack W. Murray: assistant professor of economics, Trinity University, San Antonio, Texas.

Wallace E. Oates: assistant professor of economics, department of economics, Princeton University, September 1965-June 1968.

Edward B. Oppermann: assistant professor of economics, U.S. Air Force Academy.

Robert Ortner: associate economist, The Bank of New York.

Kristian S. Palda, State University of New York, Buffalo: professor of business economics, Claremont Graduate School and University Center.

Allan V. Palmer: associate professor, department of business management, Old Dominion College.

Belinda Kemp Pearson: lecturer in economics, Wellesley College, 1965-66.

Morris Perlman: visiting assistant professor, Cornell University, academic year 1965-66.

Ernest Davis Phelps: associate professor of marketing, Nichols College.

Joseph Pichler: assistant professor of behavioral science, School of Business, University of Kansas.

Hugh Pinchen: assistant professor of economics, Colgate University.

Thomas F. Pogue: assistant professor of economics, University of Iowa.

William Pollak: assistant professor of economics, Grinnell College, September 1, 1965.

Barry W. Poulson: assistant professor of economics, University of Colorado, fall 1965.

S. Benjamin Prasad: associate professor of management, College of Business Administration, University of Nevada.

Richard H. Puckett: instructor, department of economics, The American University.

John Trout Rader, University of Illinois: associate professor of economics, Washington University, St. Louis.

Henry B. Reiling: assistant professor of business law, Graduate School of Business, Columbia University.

Frank Reilly: assistant professor of finance, School of Business, University of Kansas.

Raymond C. Robak: assistant professor of economics, Southwestern University, Georgetown, Texas.

Jerald F. Robinson: assistant professor of economics and specialist in labor education, University of Missouri.

Henry Rosovsky, University of California, Berkeley: department of economics, Harvard University.

Neville L. Rucker: assistant professor, department of economics, Old Dominion College.

Reynold M. Sachs: assistant professor of business, Graduate School of Business, Columbia University.

Charles M. Sackrey: assistant professor of economics, Ithaca College.

Edward Saraydar: assistant professor of economics, California State College, Long Beach.

Ryuzo Sato: visiting professor of economics, Brown University, academic year 1965-66.

Rainer Schickele: associate, Agricultural Development Council.

Charles Schotta, Jr.: assistant professor, department of economics, University of California, Davis.

Robert A. Schwartz, Columbia University: assistant professor of economics, Graduate School of Business Administration, New York University, September 1965.

Lawrence Sherr: assistant professor of statistics, School of Business, University of Kansas.

Calvin D. Siebert: assistant professor of economics, University of Iowa.

Theodore R. Snyder, Jr.: Intelligence Staff, U.S. Taiwan Defense Command, September 1964.

Thomas Sowell: assistant professor of economics, Cornell University, academic year 1965-66.

Raymond S. Strangways: assistant professor, department of economics, Old Dominion College.

Edward L. Summers: instructor in accounting, department of economics and business administration, Rice University, academic year 1964-65.

Leopold A. Sveikauskas: lecturer in economics, Rutgers—The State University.

Thomas R. Swartz: assistant professor, department of economics, University of Notre Dame, 1965.

Michael K. Taussig: assistant professor, department of economics, City University of New York.

Russell Taussig: visiting associate professor of business, Columbia University, 1965-66.

Richard B. Tennant: professor of business economics, Graduate School of Business, Columbia University.

Ronald L. Tinnermeier: assistant professor of economics, Iowa State University, and with AID Iowa-Peru project on agarian reform and economic development, March 1965 for two years.

George I. Treyz: assistant professor of economics, Bryn Mawr and Haverford Colleges.

Adolf Vandendorpe: lecturer, department of economics, Boston College.

Emiel C. H. Veendorp: assistant professor of mathematical economics, department of economics, Tulane University, fall 1965.

Jose J. Villamil: lecturer in economics, Rutgers—The State University.

Robert C. Vogel: assistant professor of economics, Wesleyan University.

Charles Waldauer: lecturer in economics, Rutgers—The State University.

Henry Y. Wan, Jr.: associate professor, department of economics, University of California, Davis.

Frederick E. Webster, Jr., Graduate School of Business, Columbia University: Amos Tuck School of Business Administration, Dartmouth College.

Gus Weiss, Jr.: instructor in economics, Graduate School of Business Administration, New York University, September 1965.

William E. Whitesell: assistant professor of economics, Franklin and Marshall College.

Charles K. Wilber: assistant professor, department of economics, The American University.

Maurice Wilkinson: assistant professor of business, Graduate School of Business, Columbia University.

Bruno S. Wojtun: visiting associate professor of economics, Susquehana University, September 1965.

Uwejan Woltemade: assistant professor of economics, Ohio Wesleyan University.

Theodore O. Yntema: professorial lecturer, Graduate School of Business, University of Chicago.

Leaves for Special Appointments

William C. Bagley, Rutgers—The State University: visiting professor, University of Puerto Rico, Rio Piedras, P. R., academic year 1964-65.

M. L. Black, Jr., Duke University: assignment with AID through the National Farmers Union to make a survey of accounting requirements for cooperatives in Kenya, February 26-April 3, 1965.

Harry G. Brainard, Michigan State University: economics consultant, Naval War College, July 1-December 1965.

Henry J. Bruton, Williams College: visiting professor at Instituto de Economia, University of Chile.

Lehman B. Fletcher, Iowa State University: AID Iowa-Peru project on agrarian reform and economic development, March 1, 1965-May 31, 1965.

Seymour Friedland, Claremont Graduate School: Joel Dean Associates, New York City, for two years.

Arthur S. Goldberger, University of Wisconsin: visiting professor, University of California, Berkeley, Athens Project 1964-65; Center of Planning and Economic Research, Athens.

David G. Greene, Rutgers—The State University: Rutgers Professor at San Andres University, La Paz, Bolivia.

David L. Huff, University of California, Los Angeles: visiting professor and acting director, Center for Regional Studies, School of Business, The University of Kansas.

Albert H. Imlah, Tufts University: visiting professor, department of economics, Harvard University, fall 1965-66.

Homer Jones: Federal Reserve Bank of St. Louis and visiting professor of economics, Washington University, St. Louis.

Lawrence D. Jones, Wesleyan University: University of California, Berkeley, 1965-66.

Joseph Kershaw, Williams College: assistant director, Office of Economic Opportunity, Executive Office of the President.

Kenneth K. Kurihara, Rutgers—The State University: Fulbright Professor, Tokyo Metropolitan University, January 1, 1965-June 30, 1965.

Gordon A. Marker, Rutgers—The State University: MIT-Harvard Joint Center for Urban Studies project in Venezuela, academic year 1964-65.

William C. Merrill, Iowa State University: AID Iowa-Peru project on agrarian reform and economic development, June 1, 1965-December 1, 1966.

Michael Michaely, Hebrew University, Jerusalem: research fellow, National Bureau of Economic Research, August 1965.

Guy H. Orcutt, University of Wisconsin: visiting professor, department of economics, Harvard University, 1965-66.

Erwin S. Penn, U.S. Department of Labor: U.N. adviser on industrial development, U.N. Technical Assistance Operations Bureau, Saudi Arabia.

Rudolph Gerhard Penner, University of Rochester: visiting assistant professor, department of economics, Princeton University, 1965-66.

Ray C. Roberts, Old Dominion College: visiting associate professor in economics, Duke University, 1964-65.

Tibor Scitovsky, University of California, Berkeley: visiting professor, department of economics, Harvard University, 1965-66.

Leonard S. Silk, *Business Week:* Ford Foundation Distinguished Visiting Research Professor, Graduate School of Industrial Administration, Carnegie Institute of Technology, fall semester 1965.

Gerald Sirkin, City University of New York: visiting professor of economics, University of the Philippines, January 1965-August 1965.

Charles J. Stokes, University of Bridgeport: Fulbright Professor of Economic Development, National Major University, San Marcos, Lima, Peru, academic year 1964.

Alfred P. Thorne: visiting professor, Rutgers—The State University, spring term.

Hylke Van de Wetering, Iowa State University: AID Iowa-Peru project on agrarian reform and economic development, March 1965 for 18 months .

Marshall D. Wattles, University of Oregon: special assignment with Division of Social Science, UNESCO, Paris, 1964.

Resignations

Curtis A. Cramer, University of Houston.

Solomon Fabricant, New York University, will resign as Director of Research, National Bureau of Economic Research, June 1, 1965. He will continue as a member of the National Bureau's research staff.

William H. Miernyk, University of Colorado, August 31, 1965.

Robert F. Shannon, Washington and Jefferson College.

William M. Wendt, The American University.

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THE RELATIVE STABILITY OF MONETARY VELOCITY AND THE INVESTMENT MULTIPLIER

By ALBERT ANDO AND FRANCO MODIGLIANI*

In their contribution to the staff studies for the Commission on Money and Credit [9], Milton Friedman and David Meiselman (hereafter referred to as FM) report that, according to their tests, "Except for the early years of the Great Depression, money is more closely related to consumption than is autonomous expenditures," and that "The results are strikingly one-sided" [9, pp. 165–66]. The purpose of this paper is (i) to call attention to a number of basic shortcomings in their procedure which make the results of their elaborate battery of tests essentially worthless; (ii) to show that the elimination of the shortcomings, insofar as feasible within the framework of their analysis, changes drastically their conclusions; and above all, (iii) to make it clear why the Friedman and Meiselman game of testing a one-equation one-variable model in search of the highest correlation, fascinating as it might be, cannot be expected to throw any light on such basic issues as how our economic system works, or how it can be more effectively stabilized.

In order to establish these propositions, we first show in Section I that because of serious distortions in their formulation of the "income-expenditure" model and/or biases in their statistical procedure, the results reported by FM contain very little, if any, information about the empirical usefulness of that model. No really adequate test of this model

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As this paper was about to be completed, the authors became aware of the paper by Professor Donald Hester of Yale University which deals with similar problems with somewhat different emphasis. They have had the benefit of discussing with him their mutual problems. See Donald Hester [12].

appears feasible within the constraint of a single equation with a single independent variable arbitrarily imposed by FM. It is shown, however, that insofar as the implications of the model concerning the relation between consumption and autonomous expenditure can be forced into such an artificial straitjacket, these implications receive very strong empirical support, even though the forcing is at the cost of a variety of misspecifications which tend to bias the results against the model. Even after an additional variable is introduced which eliminates, at least roughly, the possible influence of common trends, the impact effect of autonomous expenditures on current expenditure is shown to remain quite substantial.

In Section II, using the models proposed elsewhere by Friedman [7] [10], we suggest that the very high correlation between the stock of money and money income or consumption expenditure is to some extent spurious, although, again, after an additional variable is introduced to minimize the possible bias resulting from common trends, the net contribution of the money stock to the explanation of variations in current income or consumption remains significant.

On the basis of these results, we conclude that it is nearly impossible to answer the question posed by FM as to which of the two models of income determination set up by them as contenders should win the prize for the highest correlation. In the concluding section, we go on to develop the reasons why an answer to this question, even if it could be obtained, would be of very little value for any conceivable purpose.

In order to conserve space, in what follows, various components of the national income account and other relevant variables will be frequently referred to by symbols whose definitions are provided in Table 1.a.

I. *The Stability of the Investment Multiplier*

According to FM the essence of the "income-expenditure theory" can be embodied in the equation

$$Y = \alpha + K'A \tag{1}$$

where α and K' are constants, $K'>1$ [9, p. 170], and Y and A must satisfy the condition [9, p. 175]

$$Y = C + A. \tag{2}$$

From (1) and (2) they derive

$$C = \alpha + KA, \qquad K = K' - 1, \tag{3}$$

the equation on which all of their tests are based, with C defined as

measured consumers' expenditure and A the sum of measured net investment, net exports, and government deficit (cf. Table 1.b, equation (b.1)).

TABLE 1—DEFINITION OF SYMBOLS AND ACCOUNTING RELATIONS

a. Definitions of Symbols (all variables measured in billions of current dollars unless otherwise specified):

A: Autonomous expenditure; also autonomous expenditure as defined by FM (see equation b.1 below)
C: Consumption expenditure
$C^i \equiv C + Z^i$: Induced expenditure
E: Exports
G: Total government purchases of goods and services
H: Statistical discrepancy
I: Imports
$K \equiv K_1 + K_2$: Net private domestic investment
K_1: Net investment in plant and equipment, and in residential houses
K_2: Net changes in inventory
M^*: Maximum currency plus demand deposits that can be created, given the supply of reserves by the Federal Reserve System excluding borrowed reserves. Computed as follows:

$$M^* = \frac{L - B}{L - E} M,$$

where L denotes currency in circulation plus member banks deposits minus reserves against time deposits minus reserves against U.S. Government deposits (when required); B denotes member banks borrowings from the Federal Reserve; and E, member banks excess reserves.
M^D: Demand for money
M^f: Currency outside banks plus demand deposits adjusted, plus time deposits in commercial banks
M: Supply of money (currency plus demand deposit adjusted)
N: Net national product
N_p^*: Permanent net national product
P: Population
π_p: Permanent price level
Q: Subsidies less current surplus of government enterprises
R: Corporate retained earnings after taxes, plus inventory valuation adjustment
S: Personal saving $\equiv Y^d - C$
$T \equiv T_b + T_c + T_p + T_s - T_g - T_i - Q$
T_b: Indirect business tax $\equiv T_b^1 + T_b^2$
$T_b^1 \equiv T_b - T_b^2$
T_b^2: Property tax portion of indirect business taxes
T_c: Corporate profit tax accruals
T_f: Foreign transfer payment by government
T_g: Government transfer payment $\equiv T_g^1 + T_g^2$
T_g^1: Unemployment insurance benefits
$T_g^2 \equiv T_g - T_g^1$
T_i: Net interest paid by government
T_p: Personal tax and nontax payment
T_s: Contribution to social insurance
W: Excess of wage accruals over disbursement; may be ignored because it is negligibly small
$X \equiv Y^d - N \equiv X^a + X^i$
X^a: autonomous portion of X
X^i: induced portion of X
Y: Some measure of income
Y^d: Disposable personal income

TABLE 1—*Continued*

$Z \equiv N - C \equiv Z^a + Z^i$
Z^a: autonomous portion of Z
Z^i: induced portion of Z

b. Definitional and Accounting Identities:

(b.1) $S \equiv K + (G-T) + (E-I) - R - (H+W+T_f) \equiv A - R - (H+W+T_f)$
(b.2) $S \equiv (N-C) + (Y^d - N) \equiv Z + X \equiv Z^a + Z^i + X^a + X^i$
(b.3) $Z \equiv K_1 + K_2 + G + E - I$
(b.4) $X \equiv -R - T_b - T_c - T_p - T_s + T_i + T_g + Q - H - W$

For the purpose of the empirical tests reported in this paper, the following definitions were used:

(b.5) $X^a \equiv T_b^2 + T_i + T_g^2 + Q - H - W$
(b.6) $X^i \equiv -R - T_b^1 - T_c - T_p - T_s + T_g^1$
(b.7) $Z^a \equiv K_1 + G + E$
(b.8) $Z^i \equiv K_2 - I$

Our contention that their results are irrelevant is based on three considerations.

(i) *Misspecification of the consumption function.* The income-expenditure model, as generally understood for at least two decades, does not imply equation (3). To establish this proposition, let us start with the conventional, elementary form of the consumption function given by

$$C = c_0 + c_1 Y^d + \epsilon, \tag{4}$$

where c_0 and c_1 are constants, and ϵ is a random-error term. Taking into account the identity

$$Y^d \equiv C + S, \tag{2'}$$

substituting (2′) into (4) and solving for C, we obtain

$$C = \frac{c_0}{1 - c_1} + \frac{c_1}{1 - c_1} S + \frac{1}{1 - c_1} \epsilon. \tag{3'}$$

Equation (3′) is of the form (3) and C, Y, and S satisfy the condition (2′), which is similar to (2). But, the independent variable in (3′) is not A but S, which differs from A by corporate retained earnings adjusted for inventory valuation (R), the statistical discrepancy (H), excess of wage accruals over disbursement (W), and government foreign-transfer payment (T_f) (cf. equation (b.1)). Equation (3) above used by FM in which the independent variable is A is valid only if Y^d is replaced by $Y^d + R + H + W + T_f$ in the consumption function (4). But this surely involves a grievous misspecification of the consumption function, for we are not aware of any author having suggested that current consumption is a linear function of current disposable income, plus corporate savings,

plus statistical discrepancy, plus excess of wage accrual over wage disbursement, plus net government foreign transfer payments.[1]

(ii) *Treatment of war years.* Even if one were to disregard the above misspecification, it is apparent from (3′) that equation (3) could be expected to hold only if the parameters c_0 and c_1 of the consumption function can be supposed stable. But these parameters could hardly be supposed stable over a period including the war years 1942–46, especially when one recalls that C is defined to include purchases of consumers' durables. During this period consumers may have been persuaded to consume abnormally small proportions of their income for patriotic reasons, and/or they may have changed their consumption habits in response to rationing and to unavailability of some goods.[2] Hence any test including these years is worthless unless it has been shown that the results are largely invariant whether these years are included or omitted. In fact, the omission of these years makes an overwhelming difference, as is readily apparent from an inspection of FM's own scatter diagrams [9, pp. 191, 198] and from the results exhibited in Table 2 below. These results speak for themselves. Yet in their basic Table II-1 [8, p. 190], out of the six tests covering the period since 1929, three include the war years and, not surprisingly, they are the most damaging to the income-expenditure model. Yet, in their contribution, FM not even once mentioned the possibility that any of their results might be distorted by the inclusion of the war years!

(iii) *Inclusion of induced components in the "independent" variable and the resulting least-squares bias.* Whether the "independent" variable of equation (3) is S, as called for by the standard consumption function, or the variable A, arbitrarily picked by FM, one cannot get a reliable estimate of the parameters of this equation by the method of least squares applied by FM—except in the limiting and empirically irrelevant case in which the consumption function (4) holds with no error. This is because both S and A cannot be regarded as autonomous in the sense that they are uncorrelated with the residual error of the consumption function (4).[3]

[1] As far as we can see, this last item—net government foreign-transfer payment—was overlooked by FM.

While in the traditional formulation, consumption is supposed to depend on disposable income, it has been argued more recently that the argument of the consumption function should be (long-term, expected, or "permanent") total accrued income, including corporate earnings, whether distributed or not. But the extreme short-run variability of undistributed corporate income makes it a very poor proxy for the long-run expected income of corporations, and therefore it is unlikely that consumption would significantly respond to it from year to year.

[2] Other reasons for omitting war years are given in footnotes 6 and 10 below.

[3] Although FM make extensive use of the terms "autonomous" and "induced" in their paper, they have failed to communicate clearly, at least to us, just what is the relevant criterion for distinction. As will soon become apparent, for the purpose of their test the relevant

This point and its implications can readily be established with the help of the accounting identities set forth in Table 1.b. It is seen from equation (b.1), that aside from the minor reconciliation items $H+W+T_f$, personal saving S can be expressed as

$$S \simeq \text{Private Domestic Investment} + \text{Government Expenditure} + \text{Exports} - [\text{Net Taxes} + \text{Imports} + \text{Corporate Saving}].$$

TABLE 2—EFFECTS OF INCLUDING THE YEARS 1942–46 ON REGRESSION AND CORRELATION COEFFICIENTS

Using C as the Dependent Variable[a]

Row No.	Period	Coefficient of			R^2	S^2
		Constant	A	M'		
(2.1)	1929–58 inclusive	71 (17)	3.94 (.79)		.49[b]	3284
(2.2)	1929–58 inclusive	−1.2 (7.0)		1.35 (.06)	.94[b]	386
(2.3)	1929–58 excluding 1942, 43, 44, 45, and 46	50.6 (7.2)	6.70 (.39)		.92	601
(2.4)	1929–58 excluding 1942, 43, 44, 45 and 46	2.0 (5.0)		1.37 (.04)	.98	174

[a] Variance of C, 1929–58 inclusive = 6440. Variance of C, 1929–58 excluding 42–46 = 7515.
[b] See [9, p. 190, Table II.1]. Slight differences are due to revisions of national income statistics by the Department of Commerce.

The three components in the square brackets could not possibly be regarded as autonomous in the sense defined above. The movements of each of these three components are closely related to that of consumption (either directly as in the case of imports or through income as in the case of taxes), which in turn is clearly correlated with the error term ϵ of the consumption function. Since S thus includes terms correlated with ϵ, it will in general be itself correlated with ϵ. It is well known that under these conditions direct regression of C on S will yield biased estimates of the coefficients as well as of the variance of the error term ϵ. It can also be verified from the definition (b.1) in Table 1.b that this same conclusion applies if S were replaced by A, as was done by FM.

criterion of classification is whether or not a variable is supposed to be correlated with the error term of the test equation to be estimated. Accordingly, in what follows, we shall call "autonomous" those variables that are expected to be uncorrelated with the error term of the test equation under consideration, and call "induced" all other variables. Autonomous variables in this sense are not necessarily "exogenous" in the usual sense of being determined entirely outside the economic system and therefore uncorrelated with the error term of any structural equation. Thus exogenous variables are autonomous, but not all autonomous variables are necessarily exogenous.

The bias in the coefficient will be upward if the correlation of S (or A) and ϵ is positive, and downward in the opposite case. In our case there can be little doubt that this correlation will be negative and that the bias is therefore definitely downward. This is because the expression in brackets, which is positively correlated with ϵ, appears in S with a negative sign. In other words fluctuations in ϵ will tend to be accompanied by fluctuations in the same direction in imports, net taxes, and corporate retained earnings, and hence by fluctuations in S in the opposite direction.[4] Indeed, one could very well imagine circumstances in which the downward bias resulting from this negative association between S and ϵ might be strong enough so that the regression coefficient of S (or A) on C and hence also the correlation coefficient might be zero or even negative (See also [3, Appendix B].)

This conclusion may usefully be illustrated by a simple algebraic example. Consider an economy in which there are no corporations and no foreign trade, while net investment, K, and government expenditure, G, are entirely autonomous and net tax collection, T, is closely related to before-tax income. Under these conditions we have

$$S = A = K + (G - T) \tag{5'}$$

and

$$T = t_0' + t_1'(Y^d + T) + \eta' \tag{6'}$$

where the t's are constants and η' is a random-error term, implying

$$T = t_0 + t_1 Y^d + \eta, \qquad 0 < t_1 < 1. \tag{6}$$

For the sake of simplicity, let us also suppose that ϵ and η are uncorrelated.

The four equations (5'), (6), (2'), and (4) form a system in the four endogenous variables C, Y^d, S, and T and one exogenous variable $G+K$. If we solve this system for C and Y^d, we find

$$C = \frac{c_1}{1 - c_1 + t_1}(G + K) + \frac{1 + t_1}{1 - c_1 + t_1}\epsilon - \frac{c_1}{1 - c_1 + t_1}\eta + \text{constant}, \tag{7a}$$

$$Y^d = \frac{G + K + \epsilon - \eta}{1 - c_1 + t_1} + \text{constant}. \tag{7b}$$

[4] There is a strong temptation to argue that the downward bias in the regression of C on S arises directly from the positive association between consumption and the sum of imports, net taxes, and corporate saving which enter into S with a negative sign. However, this correlation can create a bias only through the mechanism of the resulting negative correlation between S and ϵ pointed out in the text. This can be readily seen by considering the limiting case in which the residual error in the consumption function is zero. In this case the correlation between C and S must be unity, no matter what might be the correlation between C and some components of S.

From these equations it is apparent that C and Y^d will be positively correlated with autonomous expenditure, as one would expect under the income-expenditure model. The size of the correlation would depend on the variance of $G+K$ relative to that of the two error terms (and not just ϵ). But what does the model imply about the correlation between consumption (or income) and the government deficit $D=G-T$? Close consideration will indicate that this correlation is likely to be very much smaller, and might even be zero or negative. This is because the relation between a change in D and the simultaneous change in C depends entirely on the source of the change in D. If D changes because of a change in G, then the association will be positive; but the association will be negative if the change in D occurs owing to changes in either K or ϵ with G (and η) constant. To illustrate, a fall in K will reduce income causing a *decrease* in consumption and also in taxes and hence an *increase* in D; similarly a fall in the error term ϵ will *decrease* consumption and hence income and taxes leading again to an *increase* in the deficit. Analogous considerations apply to the relation between S and C. If the change in C arises from a change in $K+G$ then S and C will tend to vary in the same direction. But a change in C may also arise from a change in ϵ; this will change Y^d in the same direction but to a smaller extent because of the tax bite, and hence change S in the *opposite* direction.[5] Thus, while the model definitely implies a positive correlation between Y or C and *autonomous* expenditure, it has no definite implication about the correlation between C and S. It might even be negative, depending on the variance of $K+G$ relative to that of ϵ (and η).[6]

On the basis of (i) to (iii) above we submit that the laborious battery of tests presented by FM is basically irrelevant for the purpose of assessing the empirical usefulness of the income-expenditure framework as generally understood.

[5] From (7a) and (7b) one finds

$$\frac{\partial S}{\partial \epsilon} = \frac{\partial Y^d}{\partial \epsilon} - \frac{\partial C}{\partial \epsilon} = \frac{1}{1 - c_1 + t_1} - \frac{(1 + t_1)}{1 - c_1 + t_1} = \frac{-t_1}{1 - c_1 + t_1} < 0.$$

[6] The condition for the correlation between C and S to be negative is

$$\frac{c_1(1 - t_1)}{(1 + t_1)t_1 \dfrac{\text{var}(\eta)}{\text{var}(\epsilon)} + c_1(1 - c_1)} < \frac{\text{var}(\epsilon)}{\text{var}(K + G)}.$$

If we remember that t_1 is the marginal variation of total taxes with respect to the variation in disposable personal income, and therefore a fairly large number, say .3 or .4, the possibility that the above condition is satisfied cannot be ruled out.

It should be noted that the correlation between S (or A) and ϵ will be particularly strong and negative when the economy is operating near or at full employment. This is because, given income, an accidental increase in ϵ must be offset by the corresponding decrease in S, and the condition of *given* income is more likely to be binding in the full-employment situation, even in terms of current prices.

It does not follow from the above considerations that the model has no testable empirical implications. On the contrary, as is apparent from the above example, the model does imply a strong positive association between income, or even consumption, and autonomous components of the income account, but this relation differs from (3) in a number of important respects. For one thing, because only a part of S can be taken as autonomous expenditure, it follows that the sum of C and the autonomous expenditure will not add up to income, at least as long as income is defined as the argument of the consumption function (a condition which is in turn necessary to derive (3) from the consumption function).

Having removed this irrelevant constraint, let us inquire what are the conditions under which the rudimentary consumption function (4) will imply that consumption can be expressed as a linear function of the autonomous expenditure (and no other variable). For this purpose, it is unfortunately necessary to dwell on the detailed structure of the national income accounting, and recognize that S is the sum of two parts, the difference between NNP and C, denoted by Z, and the difference between personal disposable income and NNP, denoted by X:

$$S \equiv Y^d - C \equiv (N - C) + (Y^d - N) \equiv Z + X.$$

As can be seen from equations (b.3) and (b.4) in Table 1.b, Z and X can be expressed as the sums of a number of entries in the national income accounts, some of which can be supposed as autonomous and others clearly induced. Let Z^a and X^a denote the autonomous parts and Z^i and X^i the induced portions of Z and X, respectively. We can thus rewrite equation (2′) as

$$Y^d \equiv C + Z^a + X^a + Z^i + X^i. \tag{8}$$

The question we wish to answer then becomes: under what conditions can C be expressed as a linear function of autonomous items Z^a and X^a? It can be verified that the condition in question is that the induced components Z^i and X^i can be expressed as the following linear functions (one or more coefficients, of course, could be zero):[7]

$$Z^i = z_y Y^d + z_c C + z_x X^a + z_z Z^a + z_i X^i + z_0 + \eta_z \tag{9a}$$

$$X^i = x_y Y^d + x_c C + x_x X^a + x_z Z^a + x_i Z^i + x_0 + \eta_x. \tag{9b}$$

Under these conditions (and only under these conditions) equations

[7] If more detailed components of Z^i and X^i are expressed separately as linear functions of more detailed components of Z, X, C, and Y^d, similar considerations will also apply.

(8), (9a), (9b), and (4) can be solved to yield an expression for C linear in Z^a and X^a,

$$C = \alpha_0 + \alpha_z Z^a + \alpha_x X^a + \xi, \quad (10)$$

where ξ is a linear combination of ϵ, η_x, and η_z. If we make the further assumptions that $z_x = z_z$ and $x_x = x_z$, then the coefficients α_z and α_x will be equal and (10) reduces to

$$C = \alpha_0' + \alpha_a'(Z^a + X^a) + \xi'. \quad (10a)$$

We have thus finally arrived at an equation which, like FM's equation (3), relates consumption to a single independent variable. It differs from equations (3) and (3′) in two important respects aside from the difference in the argument: (i) its coefficients, unlike those of (3) and (3′), depend not only on those of the consumption function (4), but also on those of equations (9a) and (9b); (ii) similarly the error term depends not only on that of (4), but also on η_z and η_x.

Equation (10a) is directly testable as soon as one specifies which components of Z and X can be regarded as "reasonably" autonomous. We have actually carried out such a test for the period 1929–58 excluding war years with Z^a and X^a defined by equations (b.5) and (b.7) in Table 1.b. Our choice of the autonomous components is no doubt debatable. Its main justification is that we have endeavored to stick as close as possible to the definition of A adopted by FM, eliminating only those components whose induced nature seemed beyond reasonable doubt.[8]

Before we comment on our results we must call attention to a large number of serious misspecifications that are involved in deriving the one variable equation (10a) from the income-expenditure model.

[8] With respect to those components of X which are related to government expenditure and receipts, our choice was largely dictated and supported by A. Ando, E. C. Brown, and E. Adams [2]. While X^a and Z^a, as defined, appear to be reasonably autonomous in this context, when the relations developed so far are embedded into a more general Keynesian framework, allowing both for real and for monetary forces, it will turn out that Z^a will tend to be correlated with ϵ indirectly through the money market and the interest mechanism—unless investment is completely insensitive to the rate of interest. The discussion of this section, therefore, must be read with caution. See also the Appendix to this paper for the consequences of recognizing this indirect correlation. X^a, however, remains autonomous even in the general Keynesian framework, since it is largely exogenous.

While we confine ourselves to reporting results using the specific definition of autonomous expenditure given by equations (b.5) and (b.7) for the entire period 1929–58, some sporadic experiments we have made suggest that alternative "reasonable" choices of the autonomous components or of subperiods would not change the results appreciably. In particular, if we substitute GNP for NNP in our formulation, but consider capital consumption allowances to be exogenous, then capital consumption allowances will be included in Z^a with a positive sign, and in X^a with the negative sign, leaving $Z^a + X^a$ unchanged. Even in equation (10), this change in the definition does not alter our empirical results in any significant way. For further evidence on these points, see also Hester [12].

(a) The consumption function (4) is so crude that for at least a decade hardly anyone, least of all Mr. Friedman, would regard it as an acceptable approximation—except possibly for elementary classroom exercises.

(b) Equations (9a) and (9b) must be regarded as equally crude approximations. As is documented by countless empirical studies, many of the components of Z^i and X^i depend on other variables besides those included: e.g., inventory investment depends not only on current income, but also on initial inventories and lagged income, and possibly also on the availability of credit and terms thereof; imports depend on relative prices; corporate profits on some measure of the rate of utilization of capacity and its rate of change, and so on. It is precisely the endeavor to take into account these more complex interrelations that has led to the formulation of more or less extensive models such as those of Klein [14], of Duesenberry, Eckstein, and Fromm [6], of Suits [18], and of Liu [15], to mention but a few.[9] Dropping variables known to be relevant, as we have done in order to arrive at equation (10a), can only be at the cost of increasing the variance of the error terms η_z and η_x, and hence finally of increasing the variance of ξ and decreasing the correlation between C and (Z^a+X^a).

(c) The coefficients of equations (9), especially (9b), and hence those of (10) will depend on forms and rates of taxation. Treating these coefficients as constant over a period of substantial tax change again produces an upward bias in the variance of the error term ξ.[10]

(d) The assumption leading from (10) to (10a) is also unwarranted. It can be shown that α_z may be expected to be smaller than α_x. This is because the impact of Z^a on personal income tends to be lower than that of X^a as a result of leakages related to imports, to corporate retention and to taxes. Thus, the restrictions that these two coefficients be equal again has the effect of biasing down the extent to which the variance of C appears to be explained by the model.

In spite of all these biases, it is apparent from Table 3a, row (3.1a) that hypothesis (10a) fits the data reasonably well in terms of the correlation coefficient as well as the size of the regression coefficient. Comparison with (2.3) of Table 2 shows that replacing FM's variable A with the more relevant variable X^a+Z^a, and thereby at least reducing the downward bias implicit in their procedure, reduces the unexplained variance from roughly 600 to 69, a reduction of nearly 90 per cent. Note also, by comparing (3.1a) with (2.4), that the variance left unexplained by

[9] Even though each of these models may be criticized on a number of grounds, they do endeavor to deal with the problem of correctly specifying the implications of the income-expenditure approach, as presently understood.

[10] This is incidentally another reason why war years had better be omitted or analyzed separately.

(10a) is less than half as large as that left unexplained by FM's money equation for the same period.

Some of the misspecifications listed under (a) to (d) above can be removed or made less serious, though at the cost of violating FM's arbi-

TABLE 3—EFFECTS OF THE FAILURE TO DISTINGUISH EXOGENOUS AND ENDOGENOUS COMPONENTS OF A. REGRESSION, MULTIPLE, AND PARTIAL CORRELATION COEFFICIENTS, 1929–58, EXCLUDING 1942–46[a]

a. Using C as the Dependent Variable[b]

Row No.	Coefficient of					R^2	S_e^2
	Constant	Z^a+X^a	Z^a	X^a	C_{t-1}		
(3.1a)	42.1 (2.5)	1.60 (.03)				.992	69
(3.2a)	49.3 (2.6)		1.34 (.07) [.947]	3.87 (.56) [.686]		.995	41
(3.3a)	14.8 (4.3)	.64 (.14) [.486]			.64 (.09) [.677]	.997	23
(3.4a)	24.1 (4.6)		.68 (.12) [.615]	2.10 (.46) [.498]	.52 (.09) [.626]	.998	16

b. Using C' as the Dependent Variable[c]

Row No.	Coefficient of					R^2	S_e^2
	Constant	Z^a+X^a	Z^a	X^a	C_{t-1}		
(3.1b)	40.4 (2.5)	1.49 (.03)				.991	66
(3.2b)	47.8 (2.5)		1.23 (.06) [.944]	3.83 (.54) [.706]		.995	37
(3.3b)	20.3 (5.8)	.79 (.19) [.436]			.47 (.12) [.385]	.994	43
(3.4b)	33.1 (6.2)		.84 (.16) [.570]	2.79 (.62) [.489]	.30 (.12) [.239]	.996	29

[a] Figures in parentheses are standard errors of respective coefficients, and figures in brackets are partial correlation coefficients squared.

[b] Variance of $C=7515$.

[c] Variance of $C'=6584$.

trary restriction to a single independent variable. In particular we can remove the misspecification pointed out under (d) by regressing C on Z^a and X^a rather than on their sum. The outcome, reported in Table 3a, row (3.2a), shows that the coefficients of both variables are highly significant, that the coefficient of X^a is larger than that of Z^a as expected (though the coefficient of X^a may be on the high side), and that removal of this misspecification reduces the unexplained variance by another 40 per cent.

In the last two rows of the table we present the results of a set of tests which consists in adding the variable C_{t-1}. These two tests can be justified and supported by two rather different sets of considerations. First, they can be regarded as the outcome of replacing the naïve consumption function (4) with the less naïve but still quite simple formulation

$$C_t = c_0 + c_1 Y_t^d + c_2 C_{t-1}. \tag{4'}$$

In this version C_{t-1} performs a role similar to that of such variables as the highest previous income, permanent income, or net worth, which have been advocated and successfully tested by students of the consumption function over the last 15 years. It can be readily verified that replacing (4) by (4′) leads to equations of the form (10) or (10a), but including the additional variable C_{t-1}. Second, the introduction of C_{t-1} reduces the danger that the high correlation reported in Tables 2 and 3 may be just the spurious results of common trends in all the variables. The partial correlation of the autonomous expenditures, given C_{t-1}, provides a less questionable, and probably overconservative,[11] estimate of the net contributions of these variables to variations in consumption.

It is seen from an inspection of (3.3a) and (3.4a) that the introduction of C_{t-1} reduces further the unexplained variance by some $\frac{1}{3}$ to $\frac{1}{2}$, but that otherwise the results confirm those of equations (3.1a) and (3.2a). In particular the partial correlations of the autonomous expenditures remain uniformly quite substantial.[12]

[11] See footnote 12 below.

[12] The regression coefficients of Z^a and X^a in (3.4a) or of (Z^a+X^a) in (3.3a) provide estimates only of the immediate or impact "multiplier" effect of a change in these variables on consumption. The long-run effect of autonomous expenditures on consumption (and hence finally on income) may be considered as being appreciably larger, at least if one accepts the interpretation of the consumption function (4′) as reflecting a gradual adjustment of consumption to changes in income (whatever might be the "true" mechanism behind this gradual adjustment). Under this "distributed lag" model, the long-run multiplier on consumption of a permanent change in (Z^a+X^a) implied by (3.3a) is given by the ratio of the coefficient of (Z^a+X^a) to one minus the coefficient of C_{t-1}. In the present instance this long-run multiplier is thus given by $.64/(1-.64)=1.78$, a figure very similar to the direct estimate obtained from (3.1a). Similarly from (3.4a) we find the long-run effect of Z^a and X^a to be respectively $.68/(1-.52)=1.42$ and $2.10/(1-.52)=4.37$. These figures are only moderately larger than the direct estimates obtained in (3.2a).

In Table 3.b we report the results of one final battery of tests which were inspired by a criticism kindly offered by Friedman to an earlier draft of this paper. He has suggested that the tests we have reported so far do not quite come to grips with the basic issue with which FM are concerned. This issue is which of the two alternative models does a better job of accounting for the behavior of a broad measure of income such as NNP, taking as a datum certain autonomous variables, to wit, autonomous expenditure in the income-expenditure model, and the money supply in the "rival" model. The only reason for the choice of C rather than NNP as the dependent variable in the FM tests was to avoid a bias in the correlation coefficient coming from the inclusion of A in NNP. But, in our tests, we have no reason to use consumption as the dependent variable since the sum of C and Z^a or Z^a+X^a does not add up to NNP. Instead, the relation between NNP and C is given by

$$\text{NNP} = C + Z = C + Z^i + Z^a.$$

Now our procedure tells us only how well one can account for C, given the value Z^a (and X^a), but not how well one can account for NNP because it leaves one part of NNP, namely Z^i, still unaccounted for. Hence Friedman has understandably suggested that, to be faithful to the spirit of the FM tests, the dependent variable in our tests should not be C, but rather the entire induced component, i.e., $C+Z^i$.

Fortunately this suggestion can be carrried out with very little difficulty. The four-equation system (4), (8), (9a), and (9b) can be solved simultaneously to give solutions for the four endogenous variables, C, Y^d, Z^i, and X^i, as linear functions of Z^a and X^a. From these solutions, the entire induced component, $C+Z^i$, can itself be expressed as a linear function of Z^a and X^a. Hence to carry out this last test all we need to do is to replace C with a new dependent variable $C^f \equiv C+Z^i$ and correlate C^f with Z^a and X^a (or just with their sum Z^a+X^a if we impose the misspecification $z_x=z_z$ and $x_x=x_z$). Note that since we have defined Z^i as inventory investment minus imports (cf. Table 1.b, equation (b.8)), our new dependent variable is roughly equal to the value of domestically produced consumption $(C-I)$ plus the change in inventories, a very sensible approximation to the induced part of NNP.[13]

It is apparent from Table 3.b that the results of this latest test do not change in any way our previous conclusions, especially if one uses as a criterion of performance *the variance of the residual error*. Note that, because $C^f+Z^a \equiv$NNP and Z^a is assumed given, this variance can now also be interpreted as the variance of the error in *predicting NNP* given

[13] This interpretation of C^f is only approximate since I does include imports going into fixed investment, government expenditure, and re-exports. Ideally one would want to subtract this part of imports from Z^i and add it back to Z^a, though in practice this suggestion would be extremely hard to carry out.

the autonomous components Z^a and X^a. When the dependent variable is the sum Z^a+X^a, the residual variance of (3.1b) is even smaller than in (3.1a). When Z^a and X^a are introduced separately, as they should be, the coefficient of each remains highly significant and of a very reasonable order of magnitude, while the residual variance is halved, implying that, given the autonomous components, the error of prediction of NNP has a standard deviation of only about $6 billion. Finally, the residual variances in (3.1b) and (3.2b) are respectively one-third and one-fifth of the residual variance of the money equation (2.4) (though a comparison of these variances is no longer the relevant one, as we shall point out in the next section). With the addition of C_{t-1} in (3.3b) and (3.4b), the coefficient of Z^a and X^a remains highly significant and sensible, and the residual variance is reduced further, though it remains somewhat higher than in (3.3a) and (3.4a).[14]

We have thus demonstrated that the relatively low correlation coefficients between C and A obtained by FM are due to several very serious misrepresentations of the Keynesian theory and its observable implications: inclusion of the Second World War years; the use of induced components in their independent variable; neglect of the necessity of separating various components of the exogenous variable; and the oversimplification of the consumption function. We have shown that autonomous expenditures, provided they are reasonably defined, can account for the variations in NNP up to a rather small error when some of the oversimplifications and misspecifications are removed, although it is impossible to remove many of them due to the straitjacket imposed by FM rules of the game. We will now turn to a review of their test of the money velocity.

II. *Stability of the Relation Between Money Supply and Income or Consumption*

The correlation between C and the stock of money reported by FM is uniformly so high that one cannot fail to be impressed, especially if one holds, as we do, that money is an important factor contributing to the determination of the level of income. Yet, there are also ample grounds for holding that the causal links from the money supply—or more precisely from the proximate determinants of the money supply under the control of the monetary authority—to money income are quite complex and sometimes tortuous. One is therefore inclined to

[14] It should be noted that, since $C'+Z^a=$ NNP, from Table 3.b, we can derive the estimate of the net national product multipliers for autonomous expenditures. In the case of Z^a the multiplier is one plus its coefficient in row (3.2b), or approximately 2.2. For X^a, it is directly its coefficient in row (3.2b), or approximately 3.8. In row (3.4b), because the remaining variable is C_{t-1} rather than C'_{t-1}, we can obtain only rough approximations for the long-run multipliers, 2.2 for Z^a, 4.0 for X^a.

suspect that their high correlation may be partly spurious in the sense of overstating the strength and tightness of the causal mechanism from the money supply to the level of income or consumption. We will consider here two possible sources of bias in their test procedure, one suggested by Friedman's own theory of income determination, and the other related to the existence of feedbacks from income to the stock of money.

The core of Friedman's monetary model of income determination is the quantity theory of money. His well-known and elegant formulation of this theory [8, pp. 4–17] is so general that few would quarrel with it except in matters of detail. But for purposes of empirical applications some further specifications are needed, which Friedman has supplied in later articles [7, esp. pp. 335–38 and 350] [10, esp. pp. 59–63]. In these contributions his formulation of the demand for money has been narrowed down to

$$\frac{M^D}{P\pi_p} = \gamma\left(\frac{N_p^*}{P\pi_p}\right)^\delta + \eta^* \tag{11}$$

where δ and γ are constants, and η^* is a random-error term.

In order to use Friedman's theory in interpreting the results of FM, we must establish the relation between (11) and the demand for money implicit in the FM tests, namely

$$M^D = g_1 N + g_0 + \epsilon,\text{[15]} \tag{12}$$

where g_0 and g_1 are constants and ϵ is a random-error term.

If δ is unity, it is obvious that (11) reduces to

$$M^D = \gamma_1 N_p^* + \gamma_0 + \eta \tag{13}$$

where $\gamma_1 = \gamma$, $\gamma_0 = 0$, $\eta = \eta^* P\pi_p$.

Friedman, relying on a set of time-series data covering a span of about a century, has contended that δ is distinctly larger than unity and close to 1.8. He has rationalized this finding on the ground that money is a luxury good. Both his finding and his rationalization are, however, open to considerable question. At the factual level, it may be pointed out that, using a definition of money either *less* inclusive (excluding savings deposits in commercial banks) or *more* inclusive (including all forms of savings deposits and not merely in commercial banks) and following his procedure from the turn of the century to date, one gets a value of δ much closer to unity than to 1.8.[16] At the theoretical level, the inventory model of the transactions demand for money suggests that, for a given pattern of payment habits and a given rate of

[15] The further problem created by the substitution of C for N may be postponed momentarily.

[16] The following table gives the average of ratios of the more commonly used definitions of

interest, δ would tend to be somewhat less than unity. It is quite conceivable that the mechanism to which Friedman appeals and that stressed by the inventory theoretical approach may be simultaneously at work, making the value of δ not much different from unity (once proper adjustment is made for the effects of variations in interest rates). However, it must be admitted that there is no reason for δ to be exactly unity, though it may be close to it. To the extent that δ is not unity, (13) must be regarded as an approximation obtained by first expanding the right-hand side of (11) by the Taylor's series around the mean of $N_p^*/P\pi_p$, and then disregarding all but the constant and the first term of the expansion.[17]

N_p^* is not an observable magnitude. Following Friedman, however, we may approximate N_p^* by an exponentially weighted moving average of past values of N, or

$$N_{pt}^* \propto N_{pt} \equiv \beta(1-\rho)\sum_{\tau=0}^{\infty}\rho^{\tau}N_{t-\tau} \tag{14}$$

where β is an adjustment factor for the time trend in N, a number slightly greater than unity.[18] Friedman thinks that ρ is about .7. When (14) is substituted into (13) we have

$$M_t^D = \gamma_1\beta(1-\rho)\sum_{\tau=0}^{\infty}\rho^{\tau}N_{t-\tau} + \gamma_0 + \eta. \tag{15}$$

If we further assume that

$$M_t^D = M_t \tag{16}$$

where M is the supply of money, and that M is completely autonomous,

money to GNP for three periods. Data are taken from the U.S. Bureau of the Census, *Historical Statistics of the United States, 1960,* and various issues of the *Federal Reserve Bulletin.*

	Currency Plus Demand Deposits Adjusted	Currency Plus Demand Deposits Adjusted Plus Savings Deposits in Commercial Banks	Currency Plus Demand Deposits Adjusted Plus Savings Deposits in Commercial Banks Plus Savings Deposits in Mutual Savings Banks
1895–1905	.315	.366	.466
1950–1955	.348	.469	.535
1956–1963	.280	.428	.499

[17] Note that, if δ is not unity, (13) should contain a term involving the price level, as would be true for (4) if the consumption function is properly stated in real terms. We ignore this consideration as is done by FM in most of their tests.

[18] Strictly speaking, this calculation should be performed separately for NNP in real terms and for prices, and N_{pt} should be obtained as the product. However, since Friedman applies the same weights to both series, equation (14) should provide a close approximation.

then (16) can be substituted into (15), which can then be solved for N_t in terms of M and $N_{t-\tau}$, $\tau = 1, 2, \cdots$. We thus obtain:

$$
\begin{aligned}
N_t &= \frac{1}{\gamma_1\beta(1-\rho)} M_t - \frac{\rho}{\beta(1-\rho)} \beta(1-\rho) \sum_{\tau=1}^{\infty} \rho^{\tau-1} N_{t-(\tau-1)} \\
&\quad - \frac{\gamma_0}{\gamma_1\beta(1-\rho)} - \frac{\eta}{\gamma_1\beta(1-\rho)} \qquad (17) \\
&= \frac{1}{\gamma_1\beta(1-\rho)} M_t - \frac{\rho}{\beta(1-\rho)} N_{p,t-1} - \frac{\gamma_0}{\gamma_1\beta(1-\rho)} - \frac{\eta}{\gamma_1\beta(1-\rho)}
\end{aligned}
$$

where M_t and $N_{p,t-1}$ are both predetermined variables. This implication is the one suggested by Friedman himself in his earlier article [7, p. 350].

It appears from (17) that in order to make their test at least roughly consistent with Friedman's own model of how money affects income through the demand for money, FM should have added to their test equation the variable $N_{p,t-1}$ and judged the importance of money from the over-all fit of this equation and the partial correlation of M. This same conclusion can be reached by a rather different route by others who, like ourselves, agree as to the importance of money but have serious reservations about the specific and rather simple-minded mechanism proposed by Friedman. One may suspect that, since both N and M are dominated by marked time trends, there exists a very real possibility that the high correlation between them may be partly the spurious result of the common trend. The introduction of another trend-dominated variable like $N_{p,t-1}$ would tend to reduce this danger. Clearly that part of the movement of N_t which can be accounted for by a lagged variable like $N_{p,t-1}$ cannot be properly attributed to current variations in the money supply, and therefore a more reliable, though conceivably overconservative, measure of the impact effect of M will be provided by its partial correlation with N, given $N_{p,t-1}$.[19] In short the addition of $N_{p,t-1}$ would seem an effective testing device from more than one point of view. In terms of the Friedman model the coefficient of this variable should be negative, and for this reason as well as because it eliminates a misspecification, the addition of this variable should tend to increase the multiple correlation as well as the partial correlation of the variable M. Those who suspect some spuriousness would also expect the multiple correlation to increase, but would expect the coefficient of $N_{p,t-1}$ to be positive, and the partial correlation of M to decrease and to provide a more realistic measure of the initial impact of M on income.

[19] See footnote 21 below.

The first two rows of Table 4 present the result of this test.[20] Row (4.1) gives the residual variance of NNP given money supply alone as 438, and comparison of this row with the first two rows of Table 3.b reveals that this residual variance is seven to ten times larger than the residual variance of NNP given autonomous expenditure. Introduction of the additional variable $N_{p,t-1}$ does reduce the residual variance by nearly two-thirds, as can be seen from row (4.2), though it remains substantial, and is several times larger than any of the errors of Table 3.b. However, the coefficient of $N_{p,t-1}$ is *positive*, contrary to the implication of Friedman's theory, and the partial correlation between N_t and M^f, given $N_{p,t-1}$, is dramatically smaller than the simple correlation, though it is by no means negligible.

Test (4.2a) shows that very similar results are obtained if we use consumption rather than our variable $N_{p,t}$ as a measure of permanent income. This substitution is supported by Friedman's own theory that consumption tends to be proportional to permanent income and also makes the results more directly comparable with those of Table 3. On the whole, the results of (4.2) to (4.2a) are consistent with the view that FM's high correlations are somewhat misleading and that money affects income through a mechanism which is quite different from the simple one envisaged by Friedman and which leaves room for "slippage."[21]

As mentioned earlier, a second likely source of bias in FM's tests, or even in that of equation (17), is that, under the institutional arrangements prevailing during the period covered by the tests, M was at least partly induced, and in consequence positively correlated with the error

[20] The series for money used in the calculation is that given in [9, p. 262, Appendix II-B2], except for M^*, defined below.

[21] The regression coefficients of equation (4.2) might also be interpreted as reflecting a rather complex process of gradual adjustment of N_t to the money supply. To see this, suppose that $N_{p,t}$ adjusts but gradually to the money supply so that we may write

$$N_{p,t} - N_{p,t-1} = g(hM_t - N_{p,t-1}) \tag{i}$$

where g is the speed of adjustment and $h(\simeq 1/\gamma_1)$ is the equilibrium value of velocity of money in terms of permanent income. Using definition (14), we can rewrite (i) as

$$\beta(1-\rho)N_t + \beta(1-\rho)\rho N_{p,t-1} = g(hM_t - N_{p,t-1}) + N_{p,t-1}$$

$$N_t = \frac{gh}{\beta(1-\rho)}M_t + \left[\frac{1-g}{\beta(1-\rho)} - \rho\right]N_{p,t-1}. \tag{ii}$$

The difference between (ii) and (17) is that, according to (ii), for reasonable values of g and ρ, the coefficient of $N_{p,t-1}$ is likely to be positive, while (17) predicts it to be negative. Also, the value of the coefficient of M_t implied by (ii) is g times that predicted by (17). In other words, the impact effect implied by (ii) is g times that predicted by (17). It should be recognized, however, that while this is a possible and reasonable interpretation of equation (4.2), it implies a theory of the relation between money and income quite different from the simple-minded demand equation of Friedman embodied in (11).

TABLE 4—SPURIOUSNESS IN CORRELATIONS BETWEEN MONEY STOCK AND INCOME OR CONSUMPTION, REGRESSION COEFFICIENTS, MULTIPLE AND PARTIAL CORRELATION COEFFICIENTS,[a] 1929–58, EXCLUDING YEARS 1942–46

a. Using N_t as the Dependent Variable[b]

Row No.	Coefficient of				C_{t-1}	R^2	S_e^2
	Constant	M^f	M^*	$N_{p,t-1}$			
(4.1)	−15.8 (8.0)	2.05 (.07)				.974	438
(4.2)	−15.4 (4.8)	.91 (.18) [.527]		.69 (.11) [.651]		.991	160
(4.2a)	−22.1 (4.7)	.63 (.21) [.29]			1.11 (.16) [.67]	.992	147
(4.3)	−13.6 (12.8)		2.65 (.14)			.939	1,102
(4.4)	−16.5 (4.6)		.85 (.15) [.58]	.85 (.07) [.88]		.992	139
(4.4a)	−24.0 (4.3)		.61 (.16) [.41]		1.25 (.09) [.89]	.993	122

b. Using C_t as the Dependent Variable[c]

Row No.	Coefficient of				R^2	S_e^2
	Constant	M^f	M^*	C_{t-1}		
(4.5)	−2.4 (2.0)	.37 (.09) [.445]		.79 (.07) [.841]	.997	25
(4.6)	−4.0 (8.8)		1.78 (.10)		.935	524
(4.7)	−3.4 (1.9)		.31 (.07) [.49]	.89 (.04) [.957]	.997	23

[a] Figures in parentheses are standard errors of respective coefficients, and figures in brackets are partial correlation coefficients squared.
[b] Variance of $N_t = 16765$.
[c] Variance of $C_t = 7515$.

term of (13) or (17).[22] We should like to suggest a test which should at least reduce the danger of this bias. It consists in replacing M with M^*, defined as the estimated maximum amount of money (in the conventional definition) that could be created by the banking system on the basis of the reserves supplied by the money authority (except in response to commercial bank borrowings), account being taken of reserve requirements and currency-holding habits [5] [19]. There should be little question but that the authority is, by and large, in a position to control M^* autonomously. It cannot however directly set M, and there is a good deal of evidence that the movements of M, given M^*, have tended to be partly caused by movements of income through the mechanism of movements in interest rates (modified by the rediscount rate) inducing changes in bank borrowings and excess reserves [5] [11] [19]. To be sure even replacing M with M^* may not completely dispose of the bias problem. The money authority could—and to some extent probably did—follow policies under which M^* would be partly induced by changes in aggregate demand, e.g., a policy of stabilizing interest rates or maintaining free reserves at some stated level. Nonetheless it seems highly likely that M^* is more nearly autonomous than M and that the substitution is therefore a step in the right direction.

The results of this substitution of M^* for M^f are shown in the last three rows of Table 4.a. When $N_{p,t-1}$ is not present, the error variance is quite large, many times larger than it is when the independent variable is M^f, tending to confirm the suspicion that some of the high correlation between N and M^f is due to the induced nature of M^f. When $N_{p,t-1}$ or C_{t-1} are present, however, the difference between the partial correlations is not large enough to warrant any definite conclusion.[23]

It is of some interest to note that whether one uses M^* or the more dubious variable M, the variance of the error of prediction of NNP, though modest, is still three to four times larger than that resulting

[22] This possibility and its implications are set forth in general terms by Donald Hester [12] and explained more fully in the Appendix.

[23] To the extent that M^* is scaled to the conventional definition of M rather than M^f, the substitution of M^* for M^f introduces a misspecification to Friedman's strictest formulation. It should be noted, however, that the substitution of the conventional M for M^f leads to *higher* multiple and partial correlations. In particular, if in (4.2a) M^f is replaced by M, the partial correlation square is .36 instead of .29, and the S_e^2 is reduced to 132. This fact probably accounts in large measure for the surprising finding that, on the whole, M^* appears to perform somewhat better than M^f. The fact that M^* performs even better than M as is apparent from the statistics cited above, may be partly a statistical freak, since if one runs the same correlations separately for the period 1929–41 and 1947–58, one finds that M (though not M^f) performs consistently better than M^*, the error variance being 40 per cent lower for the first period and 15 per cent for the second. Another contributing factor may be that M^* has been computed on the basis of the latest available estimates of M, which differ a little from the estimates used in our tests.

from using Z^a and X^a, and four to five times larger than that resulting from using Z^a, X^a, and C_{t-1} (cf. row (3.4b)).[24]

III. *Some Implications and Conclusions*

We have thus shown that the "strikingly one-sided" results of FM are largely accounted for by their strikingly one-sided procedure. Once we rely on a less partisan approach, the income-expenditure model can readily meet the challenge on FM's own chosen ground, namely, the size of the correlation coefficient. Indeed, from our tests, this model comes out somewhat ahead of the "rival," though we do not doubt that with some ingenuity FM would be able to better the score for their favorite champion.

We should like to make it clear, however, that we regard the game of who can produce the highest correlation as a very sterile one—except possibly for its entertainment value. Even in terms of the issue posed by FM—which of the two "rival" models is more successful in accounting for the movement of NNP, given the relevant autonomous variables—the relevant measure of "success" is the variance of the residual error and not the correlation coefficient, which depends on the ratio of this variance to another variance, and which can be radically changed by a mere transformation of variables in many cases.[25] We have shown that in terms of the residual variance the income-expenditure model appears distinctly more successful than its rival. But regardless of which model

[24] In most of FM tests in which the independent variable is M, the dependent variable is C, not N. FM justify this choice on the ground that, to avoid upward bias in the correlation coefficient, the appropriate dependent variable in the "income-expenditure" model is C, and that they wish to compare the two models "running over the same mile." As we have seen, the only meaningful measure for performance of these models is the residual variance of NNP given the autonomous variables. This variance can be obtained by using indifferently N or C^f as the dependent variable in the income-expenditure model (since the resulting residual variance will be identical) and by using N as the dependent variable in the money model. Hence the relevant test consists in comparing the residual variance in Table 4.a and Table 3.b. Nonetheless, for the sake of completeness, we give in Table 4.b the results of money regression using C as the dependent variable, although these regressions do not yield the residual variance of NNP given M^f or M^* and are therefore hard to interpret.

The residual variance of Table 4.b could at best be interpreted as a measure of the accuracy of FM's prediction for C plus autonomous expenditure from M^* or M^f on the assumption that autonomous expenditure is already known without error. But note that, under this interpretation, FM must admit that C plus autonomous expenditures, which is a broad measure of the level of employment, depends not only on M but also on autonomous expenditures. This would imply a multiplier of at least one for autonomous expenditures—a conclusion which FM would presumably find distasteful. See also the Appendix at the end of this paper.

[25] For instance in our Tables 3 and 4, for each of the equations containing the dependent variable lagged, we can drastically reduce the correlation coefficient by changing the dependent variable to a first difference, without thereby changing the error variance or the estimates of the regression coefficients. In addition, the recent systematic experiments of Ames and Reiter confirm what econometricians have known for some time, namely that obtaining high correlation is not much of a trick when dealing with U.S. time-series data [1].

is more successful even in terms of the residual variance, of what possible value would the contest be? Conceivably it might be relevant if the issue before us were which of the two *mutually exclusive*, *small* sets of variables does a better job of prediction. But this is most definitely not *the* issue, as must be obvious from the fact that both M and Z^a and X^a are contemporaneous with the variable to be predicted, and not known in advance. None of the experiments performed in FM's or in our paper throws any light on how well, or even how, the relevant "independent" variables could be predicted in advance.

But can it not be maintained that these tests do shed a good deal of light on the issue as to how income or consumption can be most effectively controlled? Even on this score the answer must be largely negative. In the first place, it is readily apparent that none of the hypotheses tested should seriously be regarded as a behavioral or structural relation. If the "independent variables" used in each equation were truly exogenous, then these equations could be regarded as grossly misspecified "reduced forms." But even this interpretation is open to question since our independent variables are at best "autonomous" in the sense of not being correlated with the error term of the consumption function,[26] and this is quite different from being exogenous to the economic system. Hence, the covariation observed over a period in which our "independent variables" were not used for control purposes can throw but little direct light on what might happen if we endeavored to manipulate them to stabilize the economy. Clearly, such an endeavor would change significantly the underlying structure. This fundamental problem exists in addition to the already formidable issue of how the "independent" variables used in our tests could be effectively and reliably controlled.

Second, and more important, there is absolutely no justification for treating autonomous expenditure and money supply as mutually exclusive stabilization devices. Indeed there is no justification for FM's posing the problem as one of choosing between a Keynesian multiplier mechanism and a monetary mechanism of income determination. It is well known that, if broadly understood as a theory of the demand for money, the quantity theory, far from being inconsistent, is actually an important part of the mechanism in the Keynesian framework as generally understood.[27] Indeed, some of the best-known contributions to the literature of the last 25 years, beginning with Hicks's classical essay [13], have been concerned with clarifying the relation between the multiplier mechanism and the quantity of money mechanisms in

[26] More generally with the error term of the test equation.

[27] See, for instance, Modigliani [16], particularly Section 6.

the process of income determination, and the outcome of these endeavors has found its way into the standard economic textbooks.[28]

As indicated in the Appendix, the results of our tests are fully consistent with the Keynesian view that both mechanisms play a role. It is shown there that the Keynesian model and the quantity of money models do not have observationally distinguishable implications in terms of our test procedures, except possibly if either model is formulated in such an extreme form as to assert that "money matters not at all" or "money only" matters. But the tests carried out in the Appendix indicate that the evidence provides no support for either of these extreme views.

We conclude, therefore, that if we are concerned with advancing knowledge of how to reduce economic instability, whether by rules, built-in stabilizers, and/or by *ad hoc* measures, there is little point in pursuing the game of testing one-equation-one-independent-variable models in search for the highest correlation, fascinating as the game might be. We need instead to buckle down to the unended and unending labor of learning more about the structure of our economy. This applies in particular to the task of charting the complex and still ill-understood channels through which money and the tools of monetary policy affect economic activity. We trust that Friedman and Meiselman will share this point of view and will continue to apply their talents and intimate knowledge of matters monetary in the pursuit of this challenging task.

APPENDIX[29]

The purpose of this Appendix is to relate explicitly the two types of tests reported in Tables 3 and 4 to the general Keynesian system, and to show that the high multiple correlation coefficients obtained, together with the appropriate signs of the regression coefficients, are perfectly consistent with that model. A useful by-product of this exercise is to lay bare the extremely low "discriminating power" of the single-equation tests performed by FM and by ourselves in the text of this paper for the purpose of assessing the relative importance of real versus monetary forces in the process of income determination. The outcome of these tests is shown to be consistent with, and hence incapable of discriminating among, the entire spectrum of views on this question, including at one extreme the view that money "matters not at all," and at the other end the view that "money only matters." Following the terminology of [16], we shall refer to these two views, respectively, as the "Effective Demand Only (EDO)" and the "Money Only (MO)"

[28] See e.g., the chapter, "Synthesis of Monetary and Income Analysis," in Samuelson's *Economics* [17], beginning with the fourth edition, and the less formally articulated treatment in earlier editions.

[29] We wish to express our thanks to Professor Friedman for his criticism of an earlier draft of this Appendix, in which he rejected as unwarranted certain implications we had drawn from the extreme quantity theory, and which led to a complete restructuring and, we hope, improvement in the argument.

models. It will be shown in particular that the only tests that have some (though still very limited) discriminatory power are those relying on the Central-Bank-controlled M^*. The outcome of these tests and of some other empirical investigations will be shown to provide fairly strong (though not rigorous) ground for rejecting both the EDO and the MO hypotheses. However, the outcome of one further variation of our tests suggests the pre-eminence of commodity market forces over monetary market forces both in the generation and in the control of income fluctuations.

We take as a point of departure Model II of Modigliani [16, p. 80, Table I] with a number of modifications in order to allow for: (i) the existence of fiscal parameters which enter our variables Z^a and X^a; (ii) the need to display the essence of the mechanism controlling the money supply; (iii) the need to modify consumption and investment functions formulated in real terms so that they will naturally lead to relations expressed in current prices, used prevailingly in the FM tests and exclusively in our own; (iv) the need to use linear approximations to the extent necessary to come out with linear test equations; (v) the presence of random-error terms in behavior equations. Once these modifications are carried out, it turns out that the resulting model contains the following determinate subsystem of nine equations in nine endogenous variables C, Y^d, M, Z^a, X, Z^i, C^f, N, and r, the rate of interest, and the predetermined variables X^a, M^*, and C_{t-1}[30] (all variables except r are defined in Table 1 of the text):

(A.1) $C = C(Y^d, r, C_{t-1}) + \epsilon_c^*$ (The consumption function)

(A.2) $Z^a = f(r, C_{t-1}) + \epsilon_z^*$ (The investment function)

(A.3) $M = L(N, r, C_{t-1}) + \eta_d^*$ (The demand for money equation)

(A.4) $M = B(r, M^*) + \eta_s^*$ (The money supply equation)

(A.5) $C^f \equiv C + Z^i$

(A.6) $N \equiv C^f + Z^a$

plus equations (8), (9a), and (9b) of the text.[31]

[30] Even though in our tests of Tables 3 and 4 the variables Z^a and M were assumed to be "reasonably" autonomous, they must be recognized as endogenous to the general Keynesian system, since they are both supposed responsive to the endogenous variable r (cf. equations (A.2) and (A.4)). In the case of Z^a the dependence on r reflects the responsiveness of some of its components, notably private capital formation and possibly parts of government expenditure, such as state and local capital outlays, to the cost (and availability) of funds. Note that the dependence of Z^a on r is sufficient to make Z^a endogenous even though we retain the assumption in (A.2) that Z^a is not *directly* responsive to simultaneous income or consumption. In the case of M the dependence on r reflects the money supply mechanism. Cf. also footnote 34 below.

As was stressed in the text, the essential characteristic of the predetermined variables is that they are independent of the residual errors of the structural equations.

[31] In order for the subsequent developments to be directly valid, the functions c, f, L and B must be homogeneous of degree one in the flow variables and the stock of money, which incidentally is precisely the assumption implied in Modigliani's 1944 Model (reproduced in

The correspondence between these equations and those of Model II is reasonably obvious. C_{t-1} has been added to (A.1) and to (A.3) as a proxy for net worth or permanent income. (A.4) has been added to the original model to account for the main determinants of the money supply in the context of the U.S. monetary institutions, namely, the Central-Bank-determined M^* (as defined in Table I and footnote 16) and the rate of interest. The remaining equations of Model II will then determine the price level, the wage rate, and employment.

If we eliminate from the above system five variables in which we are not interested per se (Z^i, X^i, N, Y^a, C) and approximate all equations linearly, we are left with the following system of four equations in the four remaining endogenous variables, C^f, Z^a, M, and r:

$$\text{(B.1)} \qquad C^f - c_z Z^a + c_r r = c_c C_{t-1} + c_x X^a + \epsilon_c$$

$$\text{(B.2)} \qquad Z^a + f_r r = f_c C_{t-1} + \epsilon_z$$

$$\text{(B.3)} \qquad -l_n C^f - l_n Z^a + l_r r + M = -l_c C_{t-1} + \eta_d$$

$$\text{(B.4)} \qquad -b_r r + M = + b_m M^* + \eta_s$$

The first two equations summarize the real part of the system while the last two relate to the monetary side representing, respectively, the demand and supply equations for money. We shall refer to these four equations as the system B. According to the standard specifications of the Keynesian model all coefficients in this system should be positive (or at least non-negative).

Equation (B.1) will be readily recognized as identical in form with equation (3.4b), which was used in our test of the income-expenditure theory in Table 3, except that the variable r was omitted. Hence, the term $-c_r r$ must be regarded as included in the error term of (3.4b). Similarly, by solving (B.3) for $(C^f+Z^a)=N$, obtaining

$$\text{(B.3a)} \qquad N = \frac{1}{l_n} M + \frac{l_c}{l_n} C_{t-1} + \frac{l_r}{l_n} r - \frac{\eta_d}{l_n},$$

it is immediately apparent that this equation is identical in form with (4.2a) used in our test of the money model in Table 4, except again for the absence of the term in r which is therefore implicitly lumped with the error term. Finally, if we use (B.4) to eliminate M from (B.3) and solve the resulting equation for C^f+Z^a, we obtain

$$\text{(B.3*)} \qquad N = \frac{b_m}{l_m} M^* + \frac{l_c}{l_n} C_{t-1} + \frac{l_r + b_r}{l_m} r + \frac{\eta_s - \eta_d}{l_n},$$

which clearly corresponds to the test equation (4.4a) of Table 4, except again for the omission of the r term.

[16, p. 80] as Model I). To the extent that this condition is not satisfied in the real world our conclusions can be regarded only as approximations.

Let us now examine how far and in what ways the equations (B.1) to B.4) of the general Keynesian system (hereafter GK) would need to be nodified in order to conform to either of the two extreme views concerning he role of money in income determination. Consider first EDO. Its essence s that effective demand, be it for consumption, capital formation, or government outlay for goods and services, is totally unaffected by interest rates or directly by the money supply). In terms of our system B, this means hat c_r in (B.1) and f_r in (B.2), which provide the link between money and commodity markets, must be zero. It is readily apparent from (B.2) that under these conditions Z^a can be regarded as a predetermined variable. Similarly, we see from (B.1) that Z^a together with the other predetermined variables will determine C^f and hence also C^f+Z^a up to a random term ϵ_c, ndependently of money supply M or M^*. On the other hand EDO has no special implications for equations (B.3) and (B.4) except that the coefficient l_r in (B.3) must definitely be positive (or at least nonzero), for else the rate of interest would not appear in the system at all and the system would be overdeterminate.[32]

Consider now the diametrically opposite MO model. If M is exogenously given, as was assumed in Model II of [16], then for MO to hold, the demand equation for money must contain just one endogenous variable, namely N (or some similar measure of total output). If, however, M is endogenous, as was presumably the case for the United States over the periods of our tests, then the above proposition must be amended to read that the equation obtained after eliminating M by a simultaneous solution of the demand and supply equations must contain only the endogenous variable N. In terms of our model this means that r must not appear in (B.3*), or that l_r of (B.3) and b_r of (B.4) must both be zero. Under these conditions (B.3*) will determine completely N (up to a random component $\eta-\eta_d$) independently of conditions in the commodity markets. If on the other hand r appeared in (B.3*) this equation would contain two unknowns and hence could not by itself determine either. In economic terms this means that a given value of M^* would be consistent with a range of value of N, with the rate of interest performing the role of equating the demand and supply of money, partly by affecting the supply and partly by affecting the demand (changing the velocity of circulation). Finally, we may note that the MO model has no special implications concerning (B.1) and (B.2) except that c_r and f_r cannot both be zero, for otherwise r would appear nowhere in the system, making it overdeterminate.

[32] From a purely formal point of view what we can say is that l_r and b_r cannot both be zero under EDO, for in this case the system (B.1), (B.3), (B.4) has three equations in only two unknowns, C^f and M, and hence is overdeterminate. But economic considerations suggest that even l_r alone cannot be zero. For if it were, then (B.1) and (B.3) constitute a closed system of two equations in C^f and M, and hence sufficient to determine both variables, and this would imply that commodity market conditions plus the demand for money all by themselves determine the *supply* of money, independently of M^*, which we know to be false because that would be inconsistent with institutional arrangements concerning reserve requirements and limitations on bank borrowings from the Fed.

One more "special" version of the Keynesian model, which we will label SK, deserves a brief mention. It asserts that monetary factors do affect aggregate demand but only by way of investment demand and not directly through consumption. This is probably a widely held point of view, and we ourselves lean in this direction. It is apparent that the only implication of SK is that c_r in (B.1) (but not f_r) should be zero.[33]

With this background we can proceed to inquire whether the outcome of the tests reported in the text can possibly help us to discriminate among the four alternative models just described. The results of this inquiry are conveniently summarized in columns, 3, 4, and 5 of Table A.1.

TABLE A.1—CONSISTENCY OF ALTERNATIVE MODELS WITH OUTCOME OF THREE TESTS

Model	Specifications	Test and Outcome					
		1-Equation (3.4b)		2-Equation (4.2a)		3-Equation (4.4a)	
		Favorable*	Unfavorable*	Favorable*	Unfavorable*	Favorable*	Unfavorable*
(1)	(2)	(3a)	(3b)	(4a)	(4b)	(5a)	(5b)
GK	All Coefficients of (B.1) −(B.4) positive	yes	?	yes	no	yes	no
EDO	$c_r=f_r=0; l_r>0, (b_r>0)$	yes	no	yes	?	no	yes
SK	$c_r=0$	yes	?	yes	no	yes	no
MO	$l_r=b_r=0; f_r>0, (c_r>0)$	yes	yes	yes	no	yes	no

* By a favorable outcome is meant here that the regression coefficients of the variables appearing in the test equation—other than C_{t-1}—are "significantly" positive and of reasonable magnitude. These variables are Z^a and X^a for Test 1, M^f for Test 2, and M^* for Test 3. Similarly, an unfavorable outcome means that these regression coefficients are not "significantly" positive or reasonable. The word "significantly" is put in quotation marks advisedly to suggest our subjective judgment rather than the technical use of the word in classical statistics.

As column (3a) indicates, insofar as the income-expenditure test (3.4b) is concerned, a favorable outcome (in the sense that the coefficients of Z^a and X^a are both positive and of reasonable magnitude) is consistent with *every one* of the four models. This conclusion is obvious for the first three models listed since they specify explicitly that (B.1) must hold. But the same conclusion holds for MO because that model has no specific implications concerning (B.1) and is therefore consistent with *any* outcome. To be sure, a close fit of (B.1) implies that *given* Z^a and X^a, C^f, and hence N are not significantly affected by monetary forces. But it would still be quite possible for the *sum* C^f+Z^a to be completely determined by M^* through (B.3*)–(B.1) merely controlling the distribution of this total between C^f and

[33] This is an approximation because C^f differs from consumption by the inclusion of net investment in consumer durables and inventories and imports, the first two of which may well be responsive to r.

Z^a—*provided* the interest mechanism of (B.2) is sufficiently effective to force investment demand into agreement with the value of Z^a determined by (B.3*) and (B.1). Thus FM's effort at showing that a multiplier-type equation does not fit the data was largely wasted; the critical question for assessing the role of money versus autonomous changes in demand is not whether (B.1) holds but whether Z^a is in fact largely exogenous, or largely controlled by M^* through the mechanism of interest rates, or, possibly, "availability."

Our negative conclusions about the discriminatory power of test equation (3.4b) are reinforced by the fact that, as indicated in column (3b), even an unfavorable outcome would only tend to reject EDO, but would not necessarily be inconsistent with the remaining models. The reason is that, except under EDO, Z^a is not an exogenous variable and hence is correlated with the error term ϵ_c. It can be shown that this correlation is necessarily negative tending to bias down the estimate of the coefficient c_z of Z^a.[34] This is the only source of bias under SK. For GK further bias may arise from the omission of the variable r which belongs to (B.1) and it could go in either direction.[35] It is therefore at least questionable whether even the finding that c_z is not very significantly positive would be inconsistent with any of the models, except EDO—hence the question marks in column (3b).

Proceeding now to an examination of the money-model test (4.2a), as shown in column (4a), a favorable outcome must be consistent with the first, third, and fourth models since each of these explicitly specifies that an equation of the form (B.3) must hold. What is perhaps more surprising is that such an outcome is equally consistent with EDO, at least provided b_r is positive, about which EDO has nothing to say. This conclusion is not as paradoxical as it sounds. For, with Z^a exogenous and C^f determined by (B.1), N is in effect predetermined with respect to (B.3), and the only endogenous variable in this equation is therefore M, and implicitly present, r. An increase in N through the mechanism described by (B.1) would be accommodated in the money market partly by an increase in r (and therefore in the velocity) and partly by an increase in the money supply in response to the increase in r, given M^*, through the mechanism of (B.4). Thus, according to EDO, M and N could exhibit a high positive correlation, though the causal direction is exclusively from N to M. However, since correlation coefficients cannot tell us anything about the direction of causality, we must conclude that a favorable outcome of test (4.2a) is consistent with every model, while an unfavorable outcome would be hard to reconcile with any of the four models except possibly EDO (cf. column (4b)).

We have thus substantiated our claim about the low discriminatory

[34] By solving explicitly the system B for the four endogenous variables in terms of the predetermined variables and the error terms, one finds that Z^a is necessarily negatively correlated with ϵ_c. This conclusion implies that, once we allow for indirect effects via the money markets, then, as already noted in footnotes 8 and 30, Z^a must be recognized as not really autonomous, even if we suppose that it is not directly responsive to simultaneous income or consumption.

[35] The correlation between r and Z^a could be negative or positive, depending on whether variations in Z^a are mostly induced by r or mostly due to ϵ_z, and thus exogenous.

power of the type of test run by FM, even in the revised version we have advocated. They are almost of no help in discriminating between alternative hypotheses *regardless* of outcome and of no help at all given the favorable outcome that actually obtained. There still remains to review the last test based on (4.4a) which differs from any of FM's tests by the substitution of M^* for M, and corresponds therefore to (B.3*).

From (B.3*) it is immediately apparent that under the MO specifications the term in r drops out. Since the remaining two variables are both predetermined, it follows that the method of direct least squares will yield an asymptotically unbiased estimate of the coefficients. Thus under MO this test *must* yield a favorable outcome as it in fact does—while an unfavorable outcome would be inconsistent with MO. The implication of this test for the remaining models can most easily be seen by solving explicitly the system B for C^f+Z^a in terms of the predetermined variables M^*, C_{t-1}, and X^a, and error terms. It is then found that the coefficient of M^* is given by the following expression:

$$\frac{b_m[f_r(1+c_z)+c_r]}{\Delta}; \qquad \Delta = l_n[f_r(1+c_z)+c_r]+l_r+b_r.$$

Recalling the EDO specification $f_r=c_r=0$, we can conclude that under EDO the coefficient of M^* in (4.4a) should be zero. From Table 4 we find instead that the coefficient of M^* is positive and four times its standard error.[36] Here finally is one (and the only one) result which is clearly inconsistent with one of the alternative models. This outcome strengthens our a priori judgment that EDO is not a satisfactory approximation to the working of our system, a point on which, we trust, for a change, there will be no disagreement between FM and ourselves.[37] It is apparent from the above formula that the remaining two models also imply a favorable outcome, and are therefore consistent with the empirical results.

Thus the outcome of our entire battery of tests can lead us at most to reject EDO, but is consistent with any of the remaining three hypotheses. We submit, however, that the MO model must also be rejected in the light

[36] One possible source of bias in this test could arise from the chance correlation between M^* and X^a, which is omitted from (4.4a). It turns out, however, that if X^a is added to (4.4a) the coefficient of M^* remains positive and quite significant, .43 with a standard error of .10.

[37] One further test which also points to the rejection of EDO can be carried out readily from our data. By solving the system B for Z^a in terms of predetermined variables, one finds that the coefficients for M^* and X^a are given, respectively, by $f_r b_m/\Delta$ and $-l_n f_r c_z/\Delta$. Hence, if we correlated Z^a with the predetermined variables, according to EDO, the coefficients of M^* and X^a should both be zero, whereas according to any of the three remaining models the first coefficient should be positive and the second negative. The empirical results are as follows:

$$\underset{}{Z^a} = \underset{(.092)}{.282M^*} - \underset{(.69)}{2.04X^a} + \underset{(.060)}{.576C_{t-1}} - \underset{(4.8)}{33.9}; \qquad S_e/\bar{Z}^a = 10.5 \text{ per cent.}$$

A convinced EDO man might not regard this evidence as very conclusive on the ground that even M^* is not necessarily exogenous and the correlation between Z^a and M^* might reflect causation from Z^a to M^* rather than the other way around. As noted in the text, the influence of Z^a on M^* might result in part from a policy of interest rate (or free reserve) stabilization, or also from Central Bank support of government expenditures included in Z^a.

of the sizable body of evidence which indicates, rather conclusively in our view, that both the demand for and the supply of money are decidedly responsive to movements in the rate of interest.[38] No comparable evidence is available to discriminate between the general Keynesian model and the special version we have labeled SK, though the lack of any convincing positive evidence for the effect of interest rates on consumption provides mild justification for leaning toward the SK version.

Our conclusion from the evidence that the determination of income results from the interaction of real and monetary factors still leaves completely unanswered the question as to whether either one of these sets of forces plays a more dominant role in some sense. The above analysis makes it clear that the tests reported in the text are totally unsuited to provide an answer, largely because they are much too rudimentary for the rather sophisticated demands made upon them. We should like to propose an alternative single-equation test which, though far from rigorous or conclusive, may help to shed some tentative light on the issue.

To lay the groundwork for this test, we must eliminate the "missing variable" r from equations (B.1) and (B.3*) by making use of the so far unutilized equation (B.2). After this elimination these equations can be conveniently rewritten in the following symmetric form:

(C.1) $$C^f = c_z' Z^a + c_m' M^* + c_x' X^a + c_c' C_{t-1} + U_c$$

$$c_z' = \left(c_z + \frac{c_r}{c_f}\right), \qquad c_m' = 0, \qquad c_x' = c_x, \qquad c_c' = c_c - \frac{c_r f_c}{f_r}$$

$$U_c = \epsilon_c - \frac{c_r}{f_r}\epsilon_z$$

(C.2) $$C^f = m_z Z^a + m_n M^* + m_x X^a + m_c C_{t-1} + U_m$$

$$m_z = -\left(1 + \frac{l_r + b_r}{l_n f_r}\right), \qquad m_n = \frac{b_m}{l_n}, \qquad m_x = 0$$

$$m_c = -\frac{l_c}{l_n} + \frac{(l_r + b_r)f_c}{l_n f_r}, \qquad U_m = \frac{\eta_s - \eta_d}{l_n} + \frac{l_r + b_r}{l_n f_r}\epsilon_z.$$

Now suppose that we run a regression of C^f on *all* the "independent" vari-

[38] We refer here to the large number of empirical studies which have made explicit use of interest rates. Some of these studies rely on the method of direct least squares, such as those of H. A. Latané, "Cash Balances and the Interest Rate," *Review of Economics and Statistics*, 1954, pp. 456–61; A. C. Stedry, "A Note on Interest Rates and the Demand for Money," *Review of Economics and Statistics*, August 1959; M. Bronfenbrenner and T. Mayer, "Liquidity Functions in the American Economy," *Econometrica*, October 1960; J. A. Meigs, *Free Reserves and the Money Supply*; and R. T. Selden, "The Postwar Rise in the Velocity of Money. A Sectoral Analysis," *Journal of Finance*, December 1961, pp. 483–545. Others rely also on structural estimates such as those of Teigen [19]; K. Brunner and A. Meltzer, "Some Further Investigations of Demand and Supply Functions for Money," *Journal of Finance*, May 1964; de Leeuw [5]; and Goldfeld [11]. All of these studies point to the unmistakable importance of the interest rate in the demand and/or supply equation for money.

ables appearing *both* in (C.1) and (C.2). Denote the resulting regression estimates by

$$C^f = a_z Z^a + a_m M^* + a_x X^a + a_c C_{t-1}. \tag{C.3}$$

Then, from standard least-squares formulas one can readily establish that (in the probability limit) the coefficients of (C.3) are related to those of (C.1) and (C.2) and the two error terms U_c and U_m according to the following expression:

$$a_j = \frac{c_j^f \operatorname{var}(U_m) + m_j \operatorname{var}(U_c) - (c_j^f + m_j)\operatorname{cov}(U_m, U_c)}{\operatorname{var}(U_m) + \operatorname{var}(U_c) - 2\operatorname{cov}(U_m, U_c)}; \tag{C.4}$$

$$j = z, m, c, x.$$

Furthermore, on the assumption that the error terms ϵ_c and ϵ_z are not significantly correlated with η_d and η_z (and without this assumption the role of exogenous changes in aggregate demand cannot be meaningfully distinguished from that of shifts in the money functions), one can infer from (C.1) and (C.2) that

$$\operatorname{cov}(U_m, U_c) = \frac{c_r(l_r + b_r)}{f_r} \operatorname{var}(\epsilon_z).$$

Under SK and MO specifications, this covariance term will vanish, and (C.4) becomes quite easy to interpret. It tells us that the coefficient of any variable in (C.3) is simply an average of the corresponding coefficients of (C.1) and (C.2), each weighted in proportion to the variance of the error term in the *other* equation. Accordingly, a_j will tend to be closer in value to the coefficients of whichever of two equations has a relatively smaller error variance.

Now consider the implications of the two extreme models. Under EDO, the above expression for the covariance does not apply, since both c_r and f_r are zero. However, we have seen that under EDO, the method of direct least squares provides unbiased estimates of (C.1), and by specification the coefficient of M^* is zero, and M^* is uncorrelated with ϵ_c. Hence, the addition of M^* to the test equation (3.4b) should yield an insignificant coefficient for this variable and leave the coefficients of the remaining variables unchanged (asymptotically). In other words, under EDO, a least-squares estimate of (C.3) should yield $a_m \simeq 0$, and values of the remaining coefficients roughly coincident with those of (3.4b). Under MO, $U_m = (\eta_s - \eta_d)/l_n$ and $\operatorname{cov}(U_m, U_c) = 0$, and hence the relative weights in (C.4) become respectively the variances of the error terms of the money market equations and of the "multiplier" equation (B.1). Hence, if the money-market relations held "tightly" and the multiplier equation relatively loosely, we should find that the coefficients of (C.3) are distinctly closer to those of (C.2) than to those of (C.1). In particular, the coefficient of Z^a should be close to *minus one* and *the coefficient of X^a should be close to zero. In economic terms, if $Y = C^f + Z^a$ is determined closely by M^* (and C_{t-1}), any increase in Z^a must tend to dis-*

place an equal amount of C^f. Note that these inferences are precisely the basis for the claim sometimes heard in the Friedman camp that fiscal policy is incapable of affecting aggregate income unless accompanied by changes in money supply—an increase in government expenditure would simply displace other expenditure and an increase in, say, transfer payment (a component of X^a), even if it increases consumption, will simply displace an equivalent amount of investment.

Actual empirical estimates of (C.3) are given in row 3 of Table A.2. For the convenience of the reader we also reproduce in row 1 the values that would be expected under EDO (from (3.4b)) and in row 2 those expected under MO in the limiting case where var (U_m) is negligible relative to var (U_c). (The coefficient of M^* is obtained from (4.4a), which under MO yields an unbiased estimate.) When neither of these extreme models holds, the coefficients of (3.4b) or (4.4a) are not unbiased estimates of (C.1) and (C.2), and we have indicated in parentheses the likely direction of the bias, based on our earlier discussion. The coefficient of Z^a in row 2 is written as $-(1+)$ to remind us that, except under MO this coefficient in (C.2) should be even more negative than minus one.

The results of the tests are rather telling. Though they are not quite consistent with the EDO model, they are far closer to what one would find in an EDO world rather than in a world in which money only mattered, and mattered powerfully. In terms of β coefficients reported in row (4), it is seen that the contribution of M^* to variations in C^f is quite small, both absolutely and in comparison with that of the other variables.[39]

TABLE A.2—EMPIRICAL RESULTS RELATING TO EQUATION (C.3)

			Values of the Coefficients of				
			Z^a	M^*	X^a	C_{t-1}	S_e^2
(1)	Theoretical Values of Coefficients	If multiplier mechanism is dominant (var (U_c) negligible relative to var (U_m))	.84 (−) (.16)	0	2.79 (−) (.62)	.30 (.12)	
(2)		If money supply mechanism is dominant (var (U_m) negligible relative to var (U_c))	−(1+)	.61 (−) (.16)	0	1.25 (.09)	
(3)	Empirical Results	Regression coefficients	.62 (.17)	.21 (.09)	1.98 (.65)	.38 (.11)	24
(4)		β coefficients	.37	.12	.14	.38	

[39] It may be of some interest to compare the results of Table A.2 with those of similar calculations for each of the two major subperiods, 1929–41 and 1947–58. This separation has some advantages in terms of greater homogeneity within subperiods as well as drawbacks due to the smaller number of observations in each of the subperiods and the consequent loss in the reliability of the estimates. One source of serious trouble, making the standard error of the esti-

The modest contribution made by M^* to the variation of C^f and hence N (given Z^a) suggested by the above test may be an understatement of its true relevance, since M^* can also affect N by way of its possible effects on Z^a. Fortunately, the results we have accumulated enable us to put a very rough upper limit on the contribution of monetary forces to the variation of N. We note first that, after netting out the part of the variation of N which can be accounted for by C_{t-1}, the residual variance of N is 197 (standard deviation of 14.0).[40] There are now two separate issues that merit consideration. The first is the question of the effectiveness of monetary policy as a control device. From (4.4a) we observe that once we take into account the effect of M^*, the variance of N is reduced only to 122. Thus, control of M^* still leaves a good deal of scope for variation in income—a standard deviation of some $11 billion, or nearly 6 per cent of average NNP for the period.

The second and separate issue is to estimate the contribution of monetary and nonmonetary factors to the variance of N. From the figures cited above we can infer that the contribution of the money supply is at *most* (197–122)/197 or some 38 per cent. This estimate is an upper bound for it implicitly allocates to M^* the effect of any covariation of M^* with X^a,[41] and relies on the questionable assumption that the entire covariation of M^* and Z^a reflects the causal influence of M^* on Z^a.[42] As can be seen from Table A.2, the addition of the two variables Z^a and X^a reduces the error variance from 122 to 24. This striking result indicates that, even when M^* is kept constant, there is room for substantial variations in Z^a and X^a which are clearly *not* induced by M^*, and that these clearly autonomous variations account for 50 per cent of the variance of N. It also warrants the conclusion that fiscal policies can be quite effective in controlling N, even when monetary policy is neutral, i.e., M^* remains unchanged.

The remaining 12 per cent of the variance of N is due to residual variation in the C^f component of aggregate demand not accounted for by systematic factors (as far as included in our highly simplified analysis). If we are interested in assessing the *total* contribution to the variation in N of real and monetary factors, whether systematic or not, then this 12 per cent must also be classified as a commodity market component, which would bring the total to at least 63 per cent. We must, however, allow for the possibility that a part of the variation in Z^a not accounted for by M^* may be due to

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mated coefficient relatively large, is the small standard deviation of the variable X^a relatively to that of the other variables and of C_f for the first subperiod. In spite of this, the results for the two subperiods are not qualitatively different from those for the entire period, although they are much closer to the prediction of the EDO model for the first subperiod and somewhat less close for the second period.

[40] All figures reported are adjusted for degrees of freedom.

[41] Since there can be little doubt about the exogenous nature of X^a in the short run, while the same is not true of M^*, any covariation of M^* and X^a would most likely reflect causation from X^a to M^*, unless it were due to chance. In fact the net correlation of these two variables given C_{t-1} is only .37.

[42] See on this point footnote 37.

the error components of the money equations η_d and η_s affecting Z^a through the interest-rate mechanism. One might assert, with some justification, that this component of variation, though not directly associated with M^*, is due to "monetary" forces. To estimate explicitly the size of this component would require estimation of the error terms η_d and η_s and of the sensitivity of Z^a to their variations through r—in short, it would require unbiased structural estimates of the system B. However, once we recall that the effect under consideration can arise only in connection with the investment component of Z^a while a very substantial portion of Z^a consists of government expenditure, it seems safe to conclude that the contribution of the money market to the variation on N through this channel must be of rather small order of magnitude.[43]

To summarize, tests of the FM variety, even after the correction discussed in the text for the most obvious biases in FM's original procedure, can throw no real light on the relative importance of monetary versus commodity forces in the determination of income, since the outcome of these tests cannot even discriminate between the hypotheses that "money only matters" and that "money matters not at all." An extension of these tests based primarily on the substitution of M by M^*, supplemented by evidence outside our tests, enables us to discard at least the two extreme hypotheses, confirming the Keynesian position that the determination of income results from the interaction of monetary and real forces. They also indicate considerable short-run variability in the relation between N and M^* as evidenced by a residual standard deviation of N given M^* in the order of 6 per cent of N—implying that passive management of the money supply, e.g., by expanding M^* commensurately with productive capacity, would represent a rather weak stabilization device.[44]

Finally, our tests provide some tentative indication of the relative contribution of monetary and commodity market forces to fluctuations in income after the trend factor is taken into account, though these indications must be taken with great caution in view of the extremely rudimentary nature of the test procedure.[45] With this warning, our results suggest that over the period of observation the contribution of the money supply to income fluctuation was at most slightly over one-third, and that the total contribution of all monetary factors is unlikely to have been significantly in excess of this figure.

[43] When similar calculations are performed for prewar and postwar periods separately, it is found that, while the qualitative results remain unchanged, the contribution of M^* is somewhat larger for the prewar period and is drastically smaller for the postwar period, relative to the entire period.

[44] Note that this conclusion is not inconsistent with the view that, in principle, effective stabilization could be achieved through *active management* of M^* aimed at offsetting short-run changes in "velocity," reflecting disturbances in the commodity markets as well as autonomous shifts in the demand and supply of money. Whether in practice monetary management could be successful in achieving this goal and to what extent it might run into the hurdle of liquidity traps—where changes in free reserves and in the velocity of M tend to offset completely changes in M^*—is of course a quite different issue on which our tests can provide no evidence.

[45] The warning in the third paragraph of Section III of the text is especially relevant here.

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TESTS OF THE RELATIVE IMPORTANCE OF AUTONOMOUS EXPENDITURES AND MONEY

By MICHAEL DEPRANO AND THOMAS MAYER*

The recent study by Milton Friedman and David Meiselmann (hereafter referred to as FM) offers evidence that "the stock of money is unquestionably far more critical in interpreting movements in income than is autonomous expenditures" [8, p. 188].

The FM study is part of a connected body of research starting with the much-discussed Friedman-Becker article [7] which indicates that the Keynesian consumption function predicts worse than a naïve model. Several other investigations of the U.S. and foreign economies offer additional support for this general position. Although our analysis deals explicitly with the FM study, our criticisms, if correct, also raise doubts concerning the conclusions of these other studies.[1]

FM predicted consumption in the United States for the period 1897-1958 from two equations, one using the money stock as an independent variable and one using autonomous expenditures (i.e., Keynesian "investment").[2] The results were dramatic. For the period as a whole, the money equation had a correlation coefficient of 0.98, and the autonomous-expenditures equation had a correlation coefficient of only 0.76. A number of related tests using various subperiods supported the results. Multiple regressions using both autonomous expenditures and money as independent variables in the same equation, suggested that the apparent correlation between autonomous expenditures

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[1] At the National Bureau of Economic Research, Friedman and Schwartz have undertaken a study similar to that of FM using moving standard deviations of logarithmic first differences [10, pp. 47–48]. George Macesich [13], applying the FM technique to Canadian data, reached similar conclusions for that country. Friedman reports that as yet unpublished studies for foreign countries have also yielded similar conclusions [5, p. 84]. We have not seen the unpublished studies for other countries. It is possible that these studies used a better definition of autonomous expenditures than the FM study did.

[2] Since investment is a component of income FM focus on consumption rather than income.

and consumption could be explained by the fact that money and autonomous expenditures are intercorrelated. All in all, autonomous expenditures failed the tests ignominiously.

The evidence appears to be so overwhelming that the authors are willing to insist that the income-expenditures approach as they present it is "almost completely useless" [8, p. 187]. In a popular book, Friedman used these and closely related results to label the widely accepted view of the effects of fiscal policy "mythology" [5, p. 84].

The FM study is an ingenious attempt to evaluate the Keynesian and quantity theories by comparing the predictive power of simple versions of the two. This approach raises several problems. First, what is true for simple versions of two theories need not apply to more complete versions.[3] Even if a simple quantity-theory model predicts better than a simple version of the Keynesian theory, it is still true that a complex Keynesian model—as exemplified by the Klein-Goldberger-Suits model—probably predicts the level of income better for recent years than does the quantity-theory model using Friedman's permanent-income-velocity approach.[4]

Second, how simple is simple? To be fair, the versions to be compared should involve approximately the same degree of (over) simplification. But how does one measure simplicity here? For example, should one juxtapose against a constant-velocity version of the quantity theory, a Keynesian theory which makes consumption a function of net national product, or one which makes consumption a function of disposable personal income? These two consumption functions are likely to show substantial difference in predictive power. Which one has that degree of simplicity which corresponds to the constant-velocity quantity theory?

Third, to what extent do simple versions of the two theories give them fair representation? Granted that taking income as a function only of autonomous expenditures gives an extreme version of the Keynesian theory, and that making income a function only of the money stock gives an extreme version of the quantity theory, how close are the *full* versions of the two theories to these "stripped down" cases? The extreme Keynesian case appears to do more violence to what Keynesians believe than the extreme quantity-theory case does to what quantity theorists believe.[5]

But we will put these questions aside, will assume that the simple

[3] An analogy to this sort of thing occurs in physics. Let us compare two theories, one asserting that heavy objects drop faster than light ones, and one asserting that they fall equally fast. If we take simple versions of these two theories which make no assumption about the absence of air pressure, and test them by dropping a lead ball and a feather from a tower, the theory that heavy objects fall faster will surely emerge the winner.

[4] For the 1950's, Friedman's velocity function gives a bad fit [6, Chart 2].

[5] Especially since money does have a crucial role in the Keynesian model.

versions used by FM do represent fair tests of the respective theories, and will endeavor to show that a correct statistical test of these models gives an important role to both money and autonomous expenditures.[6]

Like Ando and Modigliani [2] (whose research we learned of after preparing our own draft), we are concerned with the misspecifications to be found in the FM models. Our work focuses on FM's autonomous-expenditures concept and questions the derivation of their definition. Our tests indicate the variety of results which can be obtained in moving from expenditures which are usually considered autonomous to the concept used in the FM study. We suggest an alternative definition of autonomous expenditures and adopt several rules for testing the rival hypotheses which differ in some respects from the rules used in the FM study. Hypotheses concerning the influence of the money stock, the FM autonomous-expenditures concept, and our concept are tested not only against each other, but also against hypotheses utilizing a number of other plausible definitions of autonomous expenditures. The results lead to a consideration of the individual effects of several modifications of the FM test. Finally, the results of multiple regressions presented in the FM study are compared with those obtained by using several of the alternative definitions of autonomous expenditures.

I. *The Components of Autonomous Expenditure*[7]

The single-equation estimates undertaken in the FM study are reduced forms of a set of equations, as is shown in Appendix A. Used for regression analysis, these reduced-form equations are consistent with assumptions underlying the statistical method—at least in the *first approximation* that the variables treated as independent in the estimating equations are considered to be exogenous in the underlying theoretical models.[8] However, this first approximation is—when applying the least-square method—far from sufficient.

[6] In doing so we will be concerned with the FM analysis for money income rather than for real income. FM admit that in the real income regressions money is endogenous—and hence we see no purpose in using it as the "independent" variable in a regression.

[7] We deleted or relegated to footnotes several parts of this section because we found that Ando and Modigliani [2] presented that material strongly.

[8] In order to keep distinctions consistent, it will be the practice in this paper to refer to "exogenous" and "endogenous" as relating to variables in the theoretical models underlying empirical investigations. "Independent" and "dependent" will refer to the role played by variables in the empirical work. In regression analysis the variable being explained by the specific regression discussed will be termed dependent, and the explaining variables in the regression will be termed independent—even if they violate the assumptions of the statistical method by being endogenous in the underlying theoretical model. (Of course, in simultaneous methods, including reduced form, it would be the ultimate "explaining" variables which would be considered "independent.")

That serious bias can result from including endogenous elements in an allegedly independent variable is indicated in the paper by Ando and Modigliani [2].

FM define autonomous expenditures as net private domestic investment plus government deficit on income and product account plus net foreign balance. Is this aggregate variable exogenous in the economy which generated the observations used in the tests or does it compound exogenous and endogenous elements?[9]

Their particular definition of autonomous expenditures emerged from a number of correlations using annual aggregate data to test several of the individual expenditure components. This practice of using the same data, or roughly similar data, both to choose the definitions of variables (the definitions being, of course, really part of the over-all hypothesis) and to test the hypothesis is particularly suspect.

In this respect, the FM observations on money, autonomous expenditures, and perhaps even consumption are all suspect. However, our study focuses on the nature of their autonomous-expenditures concept and accepts—only for the purpose of this study we hasten to add—their definition of money and of consumption.

Let us consider the components of FM's autonomous-expenditures concept. First, even fixed private domestic investment might be endogenous, since an increase in consumption (or in the rate of growth of consumption, according to the accelerator theory) can stimulate investment. But on this matter we are inclined to agree with FM. Owing to the lags involved in the investment process, these effects on fixed investment will, to a great extent, occur not in the year in which consumption increased, but in the following year.[10] But even if all of the

[9] Even if all the components are exogenous, one must be cautious in deriving a variable by aggregating components. Components may influence the explained variable in disparate, even contrary ways. If x and z influence y, say as: $y=a+bx+cz$, and if $|b|\neq|c|$ (or, for statistical purposes, they are significantly different), of what meaning is $y=g+h(x+z)$? (The aggregate variable may be $x-z$ or $z-x$ depending on signs of the coefficients.) Some given value of $(x+z)$, say $(\overline{x+z})$, can determine innumerable values of y, each depending on the specific value of x and the specific value of z—of the infinite values of x and z which can equal $(\overline{x+z})$.

[10] In a previous study (admittedly based on a small sample), one of us found that for complete new plants or plant additions there is a lag of 23 months between the first consideration of the project and the start of construction, and a lag of six months between the final decision to build and the start of construction. The construction period is 15 months. For equipment put into already existing plants, however, there is a much shorter lag. For private nonresidential construction, the lag between the final decision to build and the start of construction is eight months, and the construction period is one year. For residential construction, however, the lag between the decision and the start of construction is shorter, and after the start of construction, two-thirds of the value is put in place in the first three months. See Thomas Mayer [15]. Admittedly, the plant and equipment estimates involve some overstatement, since they do not give enough weight to small plants. Note William H. White [19, pp. 143-45]. In a recent study of plant and equipment investment, Frederick Hammer [11, pp. 59, 79-80] found that 62 per cent of desired investment expenditure occurs within one year. In his model the desired capital stock is a function of wealth, the rate of interest paid by the firm and the prospective yield of investment. It is through the last of these variables that an increase in consumption would stimulate investment, and in Hammer's model this variable is estimated from a distributed-lag function of previous yields and shows an average lag of four to five

effect of consumption increases on investment occurs with a one-year lag, the problem does not disappear completely. If, as seems highly probable, consumption is a function of the previous year's income as well as of current income, then an exogenous increase in consumption in year T_1 raises both consumption and investment in year T_2, so that in year T_2 the correlation between consumption and investment is due in part to the effect of consumption on investment.[11]

For inventory investment, the other component of net private domestic investment, the interaction of exogenous and endogenous elements creates a more severe problem. First, since the "lead time" for inventories is generally short, an increase in consumption has a positive effect on inventory investment predominantly in the same year.[12] On the other hand, an increase in consumption can lead to unplanned disinvestment in inventories. Indeed, this induced change in inventories is one of the standard classroom explanations of why *ex post* saving and investment are equal in spite of an *ex ante* divergence.[13]

The next item included in FM's autonomous expenditures, the government deficit on income and product account, is also partially endogenous.[14] Even if one ignores the endogenous part of expenditures (particularly state and local expenditures), tax receipts are clearly endogenous, and this can give a downward bias to the correlation coefficient. Thus, if there is an increase in consumption, tax receipts rise and the deficit falls. The FM regression interprets these events as a decrease in autonomous expenditures "causing" an increase in consumption.[15]

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years. Dale Jorgenson, in his study of manufacturers' plant and equipment expenditures [12, p. 259], found an average lag of about a year and a half between the change in the demand for capital stock and the corresponding net investment. Robert Eisner, using a distributed-lag accelerator, also finds a long lag [4, pp. 237–46].

[11] For this point we are indebted to Professor David Laidler of the University of California, Berkeley.

[12] Admittedly, the reaction of entrepreneurs *may* be slow enough so that a substantial part of the impact occurs only in the following year, but at least some of the impact is likely to occur in the same year.

[13] A classic example is the well-known inventory cycle model of Lloyd Metzler [16].

[14] This paragraph and the next—written before learning of Ando and Modigliani's work—were carried over from our earlier drafts to emphasize the coast-to-coast agreement on the endogenous nature of taxes and imports [2].

[15] FM discuss the question of whether to include the government deficit or only government expenditures in exogenous expenditures. Their statistical test gives results which are "inconsistent and ambiguous." They then decide to treat the deficit rather than government expenditure as autonomous because this is the usual treatment in the literature and because "the sum of consumer expenditures and essentially tax payments would make for a rather novel consumption function" [8, p. 256].

As far as the first point is concerned, there is a legitimate question whether the "usual" practice in the literature is to treat as exogenous the government deficit or government expenditures. In addition, it is likely that they may be confusing the level of discussion in some papers which do treat the deficit as exogenous. Often this is simply a hypothetical statement

The same is true for the net foreign balance, the last item included in the Friedman-Meiselman definition of autonomous expenditures. Again, one component, exports, is largely autonomous; but the other component, imports, is not. Increases in consumption are accompanied by increases in imports, which enter as a negative component into net foreign investment with a resulting downward bias in the correlation coefficient.[16]

II. *A Statistical Examination of the FM Autonomous-Expenditures Concept*

To reject the FM conclusions, it is not sufficient merely to point to the existence of certain biases. One has to go beyond this and show that these biases are large enough to invalidate the conclusion. To do this, we correlated various components of FM's autonomous expenditures with consumption, to show how the correlation coefficient falls as one adds components which are not exogenous.[17] Table 1 shows the re-

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made to investigate the properties of such an assumption. Such a mental experiment is perfectly proper even if *in actuality* the deficit would, in nearly every case, be an induced item. To illustrate, let us assume that government expenditures are kept extremely stable so that the deficit is nearly always the result of a decline in income which is accompanied by a decline in consumption. In this situation it would still be correct to say that an increase in the deficit tends to raise consumption, even though the deficit and consumption are negatively correlated. The fault here lies, not with the theoretical statement, but with the statistical test. In a hypothetical statement, it is permissible to use the deficit as an autonomous injection, but in a (single equation) statistical test, one may not treat the deficit as exogenous. In a full-scale econometric model the deficit is, of course, not taken as exogenous, since there is at least one equation for tax receipts.

FM's second point is purely terminological. Even if both consumption and tax payments are a function of income, there is no reason why a Keynesian must bring both of these outlays into a single equation—it is still permissible to talk of consumption as a function of income and not mention tax payment. In most of the more elaborate Keynesian models, investment is a function of income, but this fact, too, is not expressed in the consumption function.

[16] An argument, similar to the first of the above points, was used by FM in deciding whether to use the foreign balance or exports as exogenous. After pointing out that, in the short run, exports are exogenous but that, in the long run, they depend upon foreign countries' ability to pay, and hence upon United States imports plus foreign investment, FM say that "Theoretical considerations alone cannot decide the matter" [8, p. 250], but that it can be settled on the basis of what the correlations show. Again the results of their correlation tests "are somewhat confusing, but on the whole they are consistent with treating the foreign balance as autonomous" [8, p. 251]. FM then decide that there is a presumption towards treating the foreign balance rather than exports as exogenous, since this is what the literature typically does.

We agree with FM that in the long run imports induce exports. However, our test in Table 1 is not "somewhat confusing." It shows that exports rather than foreign investment should be treated as exogenous. Admittedly, as pointed out below, including exports may create a bias too.

[17] Several of our components are in gross rather than net terms and hence do not strictly correspond to the FM definition. Net expenditure figures are available only for the total of fixed private domestic investment, and not for the constituent parts.

Friedman has suggested that we are implicitly treating the size of the correlation coefficient as a valid test of the correctness of an autonomous concept. This is not the intention. Definitions of terms are part of an empirical hypothesis, but if FM's autonomous term includes

TABLE 1—CORRELATION BETWEEN CONSUMPTION AND VARIOUS CONCEPTS OF AUTONOMOUS EXPENDITURES, 1929–63 IN CURRENT DOLLARS

	Excluding 1942–45				Including War Years			
	First Differences		Levels		First Differences		Levels	
Gross:	*r*	*b* (*s*)	*r*	*b* (*s*)	*r*	*b* (*s*)	*r*	*b* (*s*)
Producers' durable equipment	.644	2.59 (.59)	.979	10.42 (.40)	.633	2.54 (.55)	.976	10.13 (.39)
Producers' durable equipment plus nonresidential construction	.722	2.09 (.39)	.989	6.20 (.17)	.715	1.99 (.34)	.984	5.98 (.19)
Producers' durable equipment plus nonresidential construction plus residential construction = fixed private domestic investment	.793	1.79 (.27)	.993	4.24 (.09)	.764	1.60 (.24)	.986	4.07 (.12)
Fixed private domestic investment plus inventories = private domestic investment	.606	.75 (.19)	.985	3.95 (.13)	.626	.74 (.16)	.977	3.78 (.14)
Private domestic investment plus exports	.682	.77 (.16)	.990	2.98 (.08)	.691	.71 (.13)	.984	2.86 (.09)
Private domestic investment plus net foreign investment = private investment	.632	.80 (.19)	.985	3.92 (.13)	.645	.71 (.15)	.974	3.70 (.15)
Private investment plus government deficit	.609	1.33 (.33)	.992	3.99 (.09)	.108	.15 (.24)	.930	3.78 (.26)
Net:								
Fixed private domestic investment	.609	1.65 (.41)	.918	8.39 (.67)	.622	1.39 (.31)	.901	7.63 (.64)
Private domestic investment	.479	.63 (.22)	.886	6.93 (.67)	.534	.64 (.18)	.874	6.36 (.62)
Private investment plus government deficit = FM's concept	.384	.97 (.45)	.946	7.83 (.50)	.009	−.01 (.26)	.711	5.28 (.91)

r—Correlation coefficient
b—Regression coefficient
s—Standard error of the regression coefficient
Sources: See Appendix B.

sults for the period 1929-63 as a whole, as well as for this period excluding the war years.[18] Although we believe that the experience of the war years is not really relevant to normal conditions, we have included the war years in one part of the Table because FM do so.[19] Table 1 shows the regressions both for first differences and for the levels of the variables. Because there are strong trends in the levels of the variables, we believe that the first differences are a much better guide than are the levels; but since FM stress the levels rather than first differences, we are giving both. Fortunately, for the most part, the levels tell the same story as the first differences, and hence one does not really have to choose between them.

The results shown in Table 1 bear out the above discussion. Starting with plant and equipment investment, the correlation coefficients rise as one adds other types of fixed private investment. This is not surprising. Indeed, if consumption is a function of investment, such a rise is to be expected in a correctly specified model. As long as one uses only a small part of the total autonomous expenditures as the independent variable,

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endogenous elements, that would be inconsistent with the hypothesis. Table 1 demonstrates how the inclusion of elements which are generally agreed to be endogenous, but are part of FM's autonomous term, leads the test against the hypothesis.

Friedman's position against using the correlation coefficient for this demonstration is based on our use of consumption as the dependent variable while changing the autonomous term. He suggests that if other parts of income are considered endogenous in addition to consumption, these should then be included with consumption as a total endogenous term. However, we wished to remain as close as possible to the testing procedure used in FM. As we point out in Appendix A, the consumption equations tested by FM can be viewed as reduced-form equations of more complete models. If the models are revised slightly to contain additional endogenous terms besides consumption, these terms would each have individual reduced-form equations. Friedman's suggestion would mean adding these together! It appears reasonable to follow the FM testing procedure and compare the respective performances of the reduced form consumption equations derived from competing models.

[18] FM cover the period 1897–1958. We excluded the years prior to 1929 because of data problems.

We feel that little is lost by omitting the years prior to 1929. There have been so many changes in the economy that it does not really matter whether or not the consumption function fits these early years. The consumption function normally is used to forecast over a period short enough so that there are few changes in the institutional structure. Hence, even if FM are right, and the consumption function does forecast badly over the whole period 1897–1958, this does not invalidate the use of the consumption function for most purposes. Moreover, FM get a bad fit with the consumption function in the subperiods both before 1929 and after. If, as we are showing, their bad fit for the period subsequent to 1929 is merely a statistical artifact, then presumably the same applies to the fit for the earlier subperiods and for the whole period 1897–1958. For similar reasons we have not repeated their test, using quarterly data, for postwar years.

[19] Friedman and Becker [7], however, exclude the war years. FM give regressions for subperiods as well as for the whole period and, fortunately, the war years affect only two of these subperiods. We are not using subperiods in Table 1 because in this table we are merely trying to show that FM used an inappropriate definition of autonomous investment, and for this subperiods are not needed. In Table 2, where we compare the predictive power of autonomous investment as we define it, and of the money stock, we are using subperiods.

one is able to explain only a small part of consumption, for one takes account only of a small proportion of the factors determining it. As one includes more and more of these factors in the independent variables, the correlation improves.

But as Table 1 shows, adding inventories to fixed private domestic investment reduces the correlation coefficient. Such a decline can be explained by the fact that inventory investment is, in part, endogenous.

Exports are largely exogenous, and the addition of exports raises the correlation coefficients.[20] Imports, on the other hand, are in great part endogenous; hence, if one adds imports as well as exports by using net foreign investment in place of exports, one would expect the correlation coefficients to fall. And they do.

Adding the government's deficit or surplus reduces the correlation of the first differences, and in the data including the war years it also reduces the correlation of the levels. This too is not surprising owing to the endogenous nature of tax receipts. These changes in the correlation coefficients are consistent with what one would expect from the above discussion of specification error in the FM model.

It should be noted in Table 1 that the correlation coefficients are lower for net than for gross autonomous expenditures. Clearly, as FM point out, net expenditures, in principle, is a better variable, but the notorious weaknesses of the capital-consumption measure may account for this result and suggest that the gross figures are more meaningful than the net.[21]

But regardless of whether one uses net or gross figures, Table 1 shows that the Keynesian theory does quite well. Thus, looking at the first differences of the data, excluding the war years, fixed private domestic investment has a correlation coefficient of .793 for the gross figure and .609 for the net figure. While .793 and .609 may not seem very high, it should be remembered that we are using a very simple and crude equation, and that the data are first differences. As is shown in Table 2, the money equation has a corresponding correlation coefficient of .737. FM's finding that predictions based on the autonomous-expenditures equation were inferior to those based on the money equation was due to a particular definition of autonomous expenditures —one that does worse than any of the others in Table 1. It is only fair to point out, however, that while their definition does badly, there are

[20] Since an increase in domestic consumption reduces exports to some extent by raising prices, there is a negative relation between exports and consumption which presumably reduces the correlation coefficient. On the other hand, there is also a positive relation since an increase in U.S. consumption raises exports through its effects on foreign income. Hence, including exports in the autonomous expenditures concept may involve some bias.

[21] This does not necessarily imply that the capital-consumption figures are randomly, or worse than randomly, distributed relative to the true figures.

a few definitions which do even worse. The results for these equations as well as some other equations, are shown in Appendix Table A.

III. *Tests of Rival Hypotheses*

Having rejected the FM test on the theoretical and empirical grounds discussed in the preceding sections, we now subject the data to our own test. Unlike FM, we do not use the data themselves to determine via correlation analysis which variables should be included in autonomous expenditures. Rather, we set up testing rules a priori so that we are testing the theory and its empirical interpretation as a unit. Our first rule is to treat as autonomous only the following items: plant and equipment investment, nonresidential construction, residential construction, government expenditures on income and product account,

TABLE 2—CORRELATION COEFFICIENTS OF CONSUMPTION WITH MONEY AND VARIOUS CONCEPTS OF AUTONOMOUS EXPENDITURES

		Autonomous Expenditures Defined As				
	Money	Friedman-Meiselman Concept	Our Net Concept	Our Gross Concept	Gross Fixed Private Domestic Investment	Gross Fixed Private Domestic Investment + Exports
	Correlation Coefficients—First Differences					
1929–63 excluding war years	.737	.384	.590	.683	.793	.788
1929–63 including war years	.640	.009[a]	.099	.156	.764	.770
1929–39	.901	.900	.950	.963	.959	.957
1938–53	.258	.533[a]	.414[a]	.404[a]	.759	.797
1929–58	.639	.002	.063	.108	.750	.754
1948–63	.407	.308	.518	.548	.808	.777
1953–63	.263	.255	.837	.851	.829	.742
	Correlation Coefficients—Levels					
1929–63 excluding war years	.987	.946	.983	.992	.993	.995
1929–63 including war years	.981	.711	.904	.948	.986	.989
1929–39	.912	.937	.951	.971	.985	.981
1938–53	.962	.330	.667	.744	.960	.966
1929–58	.976	.687	.873	.919	.981	.984
1948–63	.988	.796	.907	.958	.979	.982
1953–63	.977	.675	.901	.970	.952	.957

[a] Regression coefficient is negative.
Sources: See Appendix B.

and total exports. Specifically, we are excluding the following items included by FM: (a) tax receipts, (b) imports, and (c) inventory investment.[22] Second, our hypothesis, for reasons given above, is agnostic concerning the use of gross versus net figures, and therefore we are running the test once using gross and once using net figures. Third, the war years (1942-45) are excluded.[23] Finally, because of correlation due to trends, we look at the correlation of first differences rather than of the levels of the variables.[24]

The rival hypotheses examined are then as follows:

1. Consumption can best be explained by the stock of money where money includes time deposits.

2. Consumption can best be explained by autonomous expenditures defined as net investment in producers' durable equipment, nonresidential construction, residential construction, inventory changes, government deficit on income and product account, and net foreign investment. This is the FM interpretation of the Keynesian hypothesis.

3. Consumption can best be explained by autonomous expenditures defined as investment in producers' durable equipment, nonresidential construction, residential construction, federal government expenditures on income and product account, and exports. One variant of this hypothesis subtracts capital consumption estimates, and the other does not. This is our hypothesis.

To offer a more complete test of the rival hypotheses, we did not stop at this point but included tests of a number of other definitions of autonomous spending as well. It is worth noting, however, that we chose our above definitions of autonomous expenditures a priori and not on the basis of which definition gave the best fit. Thus, two alternative concepts—fixed private domestic investment and fixed private domestic investment plus exports—do markedly better than both our concepts and money.

In all, basic tests were run on 20 different, but not unreasonable,

[22] We were not surprised to find that our net hypothesis differed from the test in the Ando and Modigliani paper [2] in only a few respects. Our net autonomous expenditures differ from (Z^a+X^a) in the following ways: (1) in our hypothesis, all taxes and transfers were considered endogenous; (2) the net amount obtained from aggregating subsidies less current surplus of government enterprises, excess of wage accruals over disbursements, and the statistical discrepancy were ignored as minor; and (3) federal government expenditures rather than total government expenditures were included in the exogenous term.

[23] The Korean War years need not be excluded since there were few, if any, shortages which limited consumption.

[24] The objection that first differences introduce negative serial correlation in residuals, when regressions using original observations have random residuals, is easily overcome if high positive correlation of the residuals exists—which is usually the case when aggregate economic observations are used in regressions. The original observations used in the tests examined here delivered residuals which contained quite high positive serial correlation.

definitions of autonomous expenditures, some of the results of which are shown in Table 2 and Appendix C. (Additional results, which could not be published here, are available upon request.)

Table 2 shows the simple correlation coefficients for the whole period as well as several subperiods. Looking at first differences for the whole period excluding the war years, the correlation coefficient is .737 for money, .590 for our net autonomous expenditures, and .683 for our gross autonomous expenditures, so that our gross autonomous expenditures "explains" about the same percentage of the first differences in consumption as does money. When one considers the various qualifications which have to be attached to the word "explains" here, the difference appears to be without significance. Moreover, looking at the tests by levels of the variables (which we are de-emphasizing, but which FM stress), the correlation coefficients for money and for our concepts of autonomous expenditures are very close, money being slightly higher than our net concept and slightly lower than our gross concept.

Table 2 shows also the regression coefficients for fixed private domestic investment and for fixed private domestic investment plus exports. These two variables have higher correlation coefficients than either money or our autonomous-expenditures concepts, both for first differences and for levels. These correlation coefficients are surprisingly high. In sharp contrast with these results is the FM concept of autonomous expenditures, its correlation coefficient for first differences being equal to only approximately half the other correlation coefficients.

Including the war years as FM do results in a more favorable picture for money than for autonomous expenditures, money having much higher correlation coefficients for first differences than autonomous expenditures. But even in this case, fixed private domestic investment and fixed private domestic investment plus exports do better than money. The FM concept does far worse than any of the other regressions; the correlation coefficient is trivial and the regression coefficient has the wrong sign. Looking at the correlation of the levels, money does somewhat better than autonomous expenditures, though the difference is not so great as it is for the first differences. Fixed private domestic investment and fixed private domestic investment plus exports do best. FM's autonomous expenditures again give the worst results.[25]

[25] We have not undertaken similar tests for the Friedman-Becker study [7], but since their "investment" is apparently similar to the FM's autonomous expenditures, it is likely that their test, too, is subject to considerable doubt. The Friedman-Schwartz test [10] uses an autonomous expenditures concept similar to the FM paper, the only difference being that Friedman and Schwartz use gross instead of net investment. Hence our criticisms also apply to this study.

FM present results not only for the whole period but also for subperiods. We have used some of these subperiods in Table 2, omitting those periods which cover less than 10 years for first differences (1933-38, 1939-48, and 1948-57), but adding two subperiods covering the postwar years.[26]

The first subperiod is 1929-39. This is the only period for which FM found favorable results for autonomous expenditures. Table 2 confirms this finding. For first differences, the correlation coefficients are high, with the coefficient for money being trivially higher than the coefficient for FM's autonomous expenditures. All the other correlation coefficients are higher than the one for money. The levels tell a similar story, money having the lowest correlation coefficient.

The next subperiod, 1938-53, unfortunately includes the war years. For this period, the money equation does badly ($r = .258$) but still much better than FM's and our autonomous-expenditures equations, all of which have the wrong sign for the regression coefficient. But fixed private domestic investment and fixed private domestic investment plus exports do very well. For the levels, money does better than the autonomous expenditures, but slightly worse than fixed investment.

The period 1929-58 also includes the war years. As in the previous period, both for first differences and for the levels, money is superior to autonomous expenditures based on the FM definition and on our definitions, but it is inferior to fixed private domestic investment and fixed private domestic investment plus exports.

The correlations for both postwar subperiods improve the results for money when the levels of the data are used. Money performs better than all autonomous-expenditures concepts—insignificantly better than several and markedly better than the FM concept. For first differences the performance of money is again better than the FM autonomous-expenditures concept, but money is markedly inferior to the other autonomous-expenditures concepts.

In short, Table 2 leads to the following conclusions:

1. For the whole period excluding the war years, both autonomous expenditures as we define them and money give good fits, with money somewhat better.

2. Including the war years lowers the correlation coefficient for money to some extent and reduces the correlation coefficients for our autonomous expenditures much more.

3. For the periods before and after the war, our autonomous expen-

[26] These last subperiods were added at the suggestion of Friedman, who noted that the crucial issue is for short periods and since we had data to 1963, we should take advantage of these additional data to present results for a postwar period.

ditures do better than money for first differences, while results are mixed when the levels of the data are used.

4. For the subperiods which include the war, money does much better than our autonomous expenditures.

5. In all periods fixed private domestic investment and fixed private domestic investment plus exports do extremely well. For first differences, they both have the highest correlation coefficients in most periods and generally perform better than money. For levels, they perform about the same as money—sometimes slightly worse and sometimes slightly better.

6. FM's concept of autonomous expenditures, with few exceptions, does worse than any of the other variables.

TABLE 3—EFFECTS OF MODIFICATIONS ON FM'S AUTONOMOUS-EXPENDITURES REGRESSIONS 1929-63

	Correlation Coefficient	Sign of Regression Coefficient	Correlation Coefficient	Sign of Regression Coefficient
	First Differences		Levels	
Original Definition of FM[a]	.009	–	.711	+
Original Definition but Excluding War Years	.384	+	.946	+
Original Definition Except for Using Gross Investment Instead of Net[a]	.108	+	.930	+
Original Definition Except for Exclusion of Inventories[a]	.211	–	.642	+
Original Definition Except for Exclusion of Taxes and Imports[a]	.226	+	.910	+
All Modifications Jointly	.683	+	.992	+
Money	.737	+	.987	+
Money[a]	.640	+	.981	+

[a] Includes war years,
Sources: See Appendix B.

Table 3 pursues this last point further and indicates what is responsible for the poor showing of FM's autonomous expenditures. It shows that, for first differences, no single characteristic is alone responsible, but that its disappointing performance is due to a combination of failings.

So far, we have discussed only simple regressions. FM compare money and autonomous expenditures not only by running separate regressions for both of them, but also by putting money and autonomous expenditures into a single regression equation to note the improvement over simple regressions using just money or just autonomous expenditures. They conclude that money is important and that

autonomous expenditures are unimportant because their multiple regressions add little to the simple regressions using just money, but add greatly to the simple regressions using autonomous expenditures alone. Except for the two subperiods containing the years 1929-33, their test shows that the relation of autonomous expenditures and consumption is "primarily a disguised reflection" of the effect of money on consumption, money and autonomous expenditures being positively correlated [8, p. 204]. We consider this test untrustworthy because if there is intercorrelation among the independent variables in a multiple regression, small errors of observations and sampling fluctuations can have substantial effects. Nevertheless, Table 4 shows the results of such tests using our definitions of autonomous expenditures as well as the FM definition.

Table 4 shows the results of multiple regressions using both money and autonomous expenditures as independent variables. The results are in line with what could be expected from the simple regressions. Looking at the entire period excluding the war years, for first differ-

TABLE 4 (PART 1)
MULTIPLE REGRESSIONS, 1929–1963,
EXCLUDING WAR YEARS

	First Differences			Levels		
	Multiple Correlation Coefficients	Regression Coefficients: Money	Regression Coefficients: Autonomous Expenditures	Multiple Correlation Coefficients	Regression Coefficients: Money	Regression Coefficients: Autonomous Expenditures
FM Definition	.752	1.10 (.22)	.40 (.34)	.987	1.51 (.16)	−.40 (.94)
Our Net Definition	.807	.96 (.20)	.46 (.16)	.989	.89 (.22)	.97 (.38)
Our Gross Definition	.833	.86 (.20)	.51 (.14)	.995	.52 (.15)	1.22 (.19)
Fixed Private Domestic Investment	.904	.77 (.15)	1.32 (.21)	.995	.43 (.15)	2.99 (.44)
Fixed Private Domestic Investment Plus Exports	.902	.78 (.15)	.97 (.16)	.996	.28 (.15)	2.52 (.32)

Note: Figures in parentheses are standard errors.
Source: See Appendix B.

TABLE 4 (PART 2)

MULTIPLE REGRESSIONS, 1929–1963, INCLUDING WAR YEARS

	First Differences			Levels		
	Multiple Correlation Coefficients	Regression Coefficients		Multiple Correlation Coefficients	Regression Coefficients	
		Money	Autonomous Expenditures		Money	Autonomous Expenditures
FM Definition	.644	.86 (.18)	−.11 (.20)	.985	1.61 (.07)	−1.06 (.36)
Our Net Definition	.640	.85 (.19)	−.01 (.11)	.982	1.61 (.14)	−.29 (.24)
Our Gross Definition	.641	.84 (.19)	.02 (.10)	.981	1.33 (.18)	.16 (.23)
Fixed Private Domestic Investment	.880	.60 (.12)	1.33 (.19)	.994	.64 (.10)	2.34 (.27)
Fixed Private Domestic Investment Plus Exports	.878	.59 (.12)	.97 (.14)	.995	.57 (.10)	1.88 (.20)

Note: Figures in parentheses are standard errors.
Source: See Appendix B.

ences, the equation using our gross definition and money shows both variables to be significant and of very roughly similar importance, with money doing a bit better. Its "*t*" value is 4.4 versus 3.6 for autonomous expenditures. For our net definition, money does better than autonomous expenditures. But if autonomous expenditures is defined as fixed private domestic investment, or as fixed private domestic investment plus exports, it does better than money. Again, the FM definition of autonomous expenditures does much worse than any of the others in Table 4. For levels, the story is quite similar, except that our gross expenditures does better than money.

Finally, it is worth noting that, even for first differences, the multiple correlation coefficients are surprisingly high, showing that even a simple theory, using both money and autonomous expenditures, can predict well.

As could be anticipated from the results of the simple correlations, adding the war years makes a difference to the results. Both the FM definition and our definitions of autonomous expenditure do badly

compared to money both in the first differences and in the levels. But once again, fixed private domestic investment and fixed private domestic investment plus exports do well.

Contrary to the FM results, it can be seen by comparing Tables 2 and 4 that money and autonomous spending together do "explain" consumption a great deal better than money alone—or autonomous expenditures alone. Often, the two together provide a greater improvement over the simple regression of money and consumption than over the simple regression of autonomous spending and consumption.

It is true that the FM definition of autonomous spending, when paired with money in a multiple regression, adds little to the explanation of consumption through money alone (correlation coefficient of .752 versus .737 in first differences excluding the war years), while the multiple regression is much better than the simple correlation using *their* autonomous spending alone (.384). However, other definitions of autonomous spending give a quite different story. Fixed private domestic investment, fixed private domestic investment plus exports, and our gross concept each give multiple correlation coefficients with money (.904, .902, .833, respectively) which significantly improve the results obtained with money alone or with each of the autonomous spending definitions alone (.683 .793, 788).

The results shown in Tables 2 and 4 are, of course, subject to the challenge that the fixed-investment component of our autonomous-expenditures concept contains an endogenous element which may give it a bias. While in principle one would expect there to be an endogenous element in our autonomous-expenditures data, there is no way, short of a full-scale econometric model, of determining if its effects on the regressions are of significant size. There is, however, an additional clear bias. We have omitted inventory investment because it contains an endogenous element. But part of inventory investment is exogenous; therefore, our definition of exogenous investment does not encompass all exogenous expenditures. The omission of some exogenous expenditures from the independent variable reduces the correlation coefficient. In this way, our test is unfair to Keynesian theory, just as it would be unfair to test the quantity theory by making consumption a function of currency in circulation.

We have no way of measuring which bias predominates. But, in any case, since the money variable also contains an endogenous element, there is a bias in the money equation too. As Friedman and Schwartz have pointed out [9, p. 695]:

> While the influence running from money to economic activity has been predominant, there have clearly also been influences running the other

way, particularly during the short-run movements associated with the business cycle. . . . Changes in the money stock are therefore a consequence as well as an independent source of change in money income and prices. . . . Mutual interaction, but with money rather clearly the senior partner in longer run movements and in major cyclical movements, and more nearly an equal partner with money income and prices in shorter run and milder movements—this is the generalization suggested by our evidence.

IV. *Conclusions*

If we assume that the biases in our autonomous-expenditures equation and in the money equation are approximately equal in size and have the same sign, then one can conclude from Tables 2 and 4 that both autonomous expenditures and money are of very roughly equal importance. Due to the specification errors in the models, as well as errors in the basic national income data, we do not want to attach much significance to the relatively minor differences between the correlation coefficients of the autonomous-expenditures equations and the money equation. Moreover, even if one of the two simple models should turn out to be somewhat superior to the other one, a small degree of superiority would not allow one to say which of two more complex, and hence more relevant, models would be superior. FM were able to generalize about complex models only because one of their two simple models performed vastly better than the other. But no such vast difference divides the models in our test.

We know of no direct evidence to substantiate or refute our assumption that the specification biases have approximately equal effects on the correlation coefficients in the two equations. But there is some evidence elsewhere which, in a very general sense, supports our broad conclusion. First, there is the fact that fairly good forecasts can be obtained both from Keynesian models such as the Klein-Goldberger-Suits model [18] and from the quantity-theory model of Brunner and Meltzer [3].[27] More specifically, in comparing the predictive powers of the Keynesian and quantity-theory equations one need not rely on simple models as FM do; one can indeed compare the Klein-Goldberger-Suits model with the Brunner-Meltzer model. Such a comparison shows that the Klein-Goldberger-Suits model predicts somewhat better, though as Brunner and Meltzer point out, such a comparison is unfair because the Klein-Goldberger-Suits model is a model constructed solely for fore-

[27] To be sure, these models are not rigid representations of Keynesian and quantity-theory models. The Klein-Goldberger-Suits model does include liquidity effects, and the Brunner-Meltzer model has an interest elasticity of demand for money substantially greater than zero and hence, depending upon the interest elasticity of expenditures, may yield some Keynesian results. See Albert Ando [1]. Nevertheless, these two models do represent broadened versions of the two theories.

casting and hence uses some shortcuts eschewed in the Brunner-Meltzer model.[28]

Second, if one takes a general model containing both the Keynesian and quantity-theory parameters and uses some recent econometric estimates for these parameters, the results suggest that both autonomous expenditures and money are important.[29]

To conclude, although money is not completely exogenous, we are in general agreement with FM's positive conclusion that money is important. We reject, however, their negative conclusion that autonomous expenditures are not important. Essentially, our results support the intermediate (moderate Keynesian) position that it is incorrect to stress either autonomous expenditures or money to the exclusion of the other variable.

Appendix A

Notes on Friedman and Meiselman's Keynesian Model

For expository reasons, a brief outline of the FM formulation of the Keynesian model compared to our formulation may be useful. For the "velocity" model they mention that the equation $C=\alpha+VM$ is obtained from the equation $Y=a+V'M$, and that the tested equation is a combination of $Y=a+V'M$ and a consumption function.[30] More completely, it may be conceived that the tested equation is the reduced-form consumption equation of a model in which money demand is a function of income, money supply is exogenous, consumption depends on income, and the equilibrium equation states that money demand equals money supply. The "multiplier" equation tested may be conceived to be derived from Model I:

$$C=C(Y^d)$$
$$N=C+P+G+E-I$$
$$Y^d=N-T$$
$$T=\overline{T}$$
$$P=\overline{P}$$
$$G=\overline{G}$$
$$E=\overline{E}$$
$$I=\overline{I}$$

[28] Thus, in many of the equations, the value of the dependent variables depends heavily upon a constant term or upon its own lagged value [3, pp. 342–43].

[29] See Thomas Mayer [14]. In the majority of cases treated in this paper, the Keynesian theory did better than the quantity theory, but still in the majority of cases, the quantity of money did turn out to be important. (In this paper, as will be shown in a forthcoming note by Donald Tucker, the importance of money is overestimated for the price inflexibility case, but if one adds the price flexibility case, money is still of substantial importance.) A similar analysis by Warren Smith [17, pp. 136–43], using a somewhat different model and different values for the parameters, however, attributes substantially less importance to money than to autonomous expenditures.

[30] C is consumption, M is money, Y is income, and A is autonomous expenditures. All other symbols denote coefficients.

where:

C is consumption
N is some measure of income—specifically, in this study, net national product
Y^d is disposable income
T is tax receipts
P is private domestic investment
G is government expenditures
E is exports
I is imports

and transfers, corporate retained earnings, and some minor items are neglected for simplicity. Bars over the symbols indicate the variable is assumed exogenous.

If a linear consumption function is specified (as appears from the FM study), say, $C=a+bY^d$, the reduced-form equation for income will be:

$$(1) \qquad N = \frac{a}{1-b} + \frac{1}{1-b}(\overline{P} + \overline{G} + \overline{E} - \overline{I}) - \frac{b}{1-b}\overline{T},$$

for disposable income it is:

$$(2) \qquad Y^d = \frac{a}{1-b} + \frac{1}{1-b}(\overline{P} + \overline{G} + \overline{E} - \overline{I} - \overline{T}),$$

and for consumption it is:

$$(3) \qquad C = \frac{a}{1-b} + \frac{b}{1-b}(\overline{P} + \overline{G} + \overline{E} - \overline{I} - \overline{T}).$$

Where equation (3) seems to be that tested in the FM study by $C=\alpha+KA$, since $A=(\overline{P}+\overline{G}-\overline{T}+\overline{E}-\overline{I})$: private domestic investment plus government deficit plus net foreign investment (where $\overline{E}-\overline{I}$ may be considered net foreign investment if transfers are ignored). In addition, FM note, in comparing the coefficients of $Y=\alpha+K'A$ and $C=\alpha+KA$, that $K=K'-1$. Neglecting corporate retained earnings and some minor items, FM's "Y" is disposable income. If

$$K' = \frac{1}{1-b} \quad \text{and} \quad K = \frac{b}{1-b},$$

then it is correct that, comparing the reduced-form equations for disposable income and for consumption, $K=K'-1$.[31]

[31] We have learned that FM are quite willing to treat T and I as induced rather than as autonomous, so long as G and E can be considered to be sufficiently induced to allow that $G-T$ and $E-I$ are each exogenous. The reduced form for consumption would then be:

$$C = \frac{a}{1-b} + \frac{b}{1-b}(\overline{P} + \overline{G-T} + \overline{E-I}).$$

These comments, however, do not affect the outcomes of our tests, since we did compare re-

In contrast to Model I, our hypothesis may be derived from Model II, which we claim is closer to the more generally recognized income model.

Model II:

$$C=C(Y^d)$$
$$N=C+P+G+E-I$$
$$Y^d=N-T$$
$$T=T(N)$$
$$G=\overline{G}$$
$$E=\overline{E}$$
$$I=I(N)$$
$$P=F+B$$
$$F=\overline{F}$$
$$B=B(N)$$

where F is private fixed investment and B is inventory. If the consumption, tax, import, and inventory functions are taken as linear, the consumption reduced-form equation will be:

$$C = \alpha + K(\overline{F} + \overline{G} + \overline{E}).$$

Actually, the components of "A" in the FM study were *not* derived from an explicitly stated theoretical framework, such as Model I, but rather from individual tests on several of the more questionable components of "A" combined with net private fixed investment. The tests essentially involved comparisons of correlation coefficients. (Similar tests were conducted on the definition of money.)

Appendix B

Notes on Data and Definitions

U.S. Department of Commerce sources [21] [22] gave us the components for autonomous expenditures and consumption. The money figures up to 1946 were taken directly from the FM study. For 1947–63 the revised figures given in [20] were used.[32]

For autonomous expenditures, our figures differ slightly from the FM data for several reasons. First, we used the latest revisions of the data. We have extended the period of analysis beyond 1958 (presumably the latest figures available to FM) to 1963. Second, although FM say that they used foreign investment, they apparently used the closely related "net exports" data. The difference between the two in the current method of national

sults using government deficit and net foreign balance with tests using simply government expenditures and exports as components of autonomous expenditures.

[32] For 1956, FM apparently used a total for gross private domestic investment given in line 6, Table 1-1 of the July, 1959 *Survey of Current Business*, a total which contains a printing error of $3 billion. We too must confess a computing error, fortunately a trivial one. For one year our money figure was off by a small amount which should not have any effect on the results.

Deposits held by the postal savings system were not subtracted in our study. This is contrary to what FM say they did. We presume that their statement contains a printing error; but in any case, the deposits by the postal savings system are too trivial to matter. Our money figures were rounded to the nearest $0.1 billion.

TABLE A—CORRELATION BETWEEN CONSUMPTION AND VARIOUS CONCEPTS OF AUTONOMOUS EXPENDITURES 1929–63

Independent Variables	Excluding War Years						Including War Years					
	Differences			Levels			Differences			Levels		
	r	b	se	r	b	se	r	b	se	r	b	se
Money	.737	1.18	.21	.987	1.44	.04	.640	.85	.18	.981	1.45	.05
FM Concept	.384	.97	.45	.946	7.83	.50	.009	−.01	.26	.711	5.28	.91
1	.644	2.59	.59	.979	10.42	.40	.633	2.54	.55	.976	10.13	.39
12	.722	2.09	.39	.989	6.20	.17	.715	1.99	.34	.984	5.98	.19
123	.793	1.79	.27	.993	4.24	.09	.764	1.60	.24	.986	4.07	.12
123 4	.606	.75	.19	.985	3.95	.13	.626	.74	.16	.977	3.78	.14
123 45	.632	.80	.19	.985	3.92	.13	.645	.71	.15	.974	3.70	.15
123 5	.702	1.37	.27	.993	4.20	.09	.690	1.15	.21	.982	3.98	.13
123 6	.268	.55	.38	.982	4.17	.15	.138	−.14	.18	.893	3.78	.33
123 56	.298	.63	.39	.989	4.19	.12	.099	−.11	.20	.913	3.88	.30
123 46	.474	.84	.30	.985	3.97	.13	.035	.04	.20	.911	3.70	.29
123 456	.609	1.33	.33	.992	3.99	.09	.108	.15	.24	.930	3.78	.26
123 8	.628	.85	.20	.988	2.23	.06	.056	.04	.13	.920	2.09	.16
123 48	.607	.61	.15	.987	2.16	.07	.174	.12	.12	.926	2.05	.15
123 9	.788	1.33	.20	.995	3.13	.06	.770	1.18	.17	.989	3.02	.08
123 49	.682	.77	.16	.990	2.98	.08	.691	.71	.13	.984	2.86	.09
123 89	.683	.80	.16	.992	1.89	.04	.156	.11	.13	.948	1.83	.11
123 7	.609	1.65	.41	.918	8.39	.67	.622	1.39	.31	.901	7.63	.64
123 47	.479	.63	.22	.886	6.93	.67	.534	.64	.18	.874	6.36	.62
123 567	.036	.08	.44	.941	8.73	.58	.211	−.26	.21	.642	5.02	1.04
123 897	.590	.76	.20	.983	2.48	.09	.099	.08	.14	.904	2.29	.19
123 489 7	.594	.60	.15	.979	2.39	.09	.226	.17	.13	.910	2.23	.18

income accounting is that government foreign transfers are not subtracted in deriving net exports. By including both the government deficit and net exports in autonomous expenditures they, in effect, double counted government foreign transfers.

But the effects of these changes on the regressions are minor; for example, in the FM study the correlation using their definition of autonomous expenditures yields a coefficient of 0.705, while the same definition using our data and periods yields a coefficient of 0.711.

APPENDIX C

Appendix Table A shows, for various periods, regressions for various definitions of autonomous expenditures. The following code is used:

1. producers' durable equipment
2. nonresidential construction
3. residential construction
4. inventories
5. net foreign investment
6. government deficit on income and product account
7. capital consumption allowances (subtracted from other items)
8. federal government expenditures
9. exports

r. correlation coefficient
b. regression coefficient
se. standard error

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REPLY TO ANDO AND MODIGLIANI AND TO DEPRANO AND MAYER

By MILTON FRIEDMAN AND DAVID MEISELMAN*

Because our article ventured into almost virgin territory, it was necessarily tentative, probing, and imperfect.[1] Because it questioned the new orthodoxy, we expected it to provoke controversy. That it has done so impresses us less than the large area of agreement between our critics and ourselves. Professors Ando and Modigliani (referred to hereafter as AM), and DePrano and Mayer (referred to hereafter as DM), all agree with us that changes in the quantity of money are connected with changes in nominal income over short-run periods and that this connection is at least to some extent independent of concurrent changes in autonomous expenditures. So far as we can judge, they also all agree with at least the broad outlines of our analysis of the channels through which the changes in the quantity of money exert their influence, though no doubt there are differences about details.

One other important element of agreement is implicit, and extends also to Professor Hester, who has published a criticism of our work in another journal.[2] Neither we nor our critics had much problem selecting a useful empirical counterpart for the theoretical concept of "money." There is some leeway, but the alternatives are fairly clear-cut and differences in results are relatively minor. No elaborate process of trial and error, of combination and recombination of components, is required to get measures that are closely correlated with nominal income and the nominal value of consumption. We and our critics all used the same measures without much ado.[3] The contrast with "autonomous expendi-

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[1] The only other systematic attempts preceding ours to assess empirically the relative usefulness of the two leading theories for interpreting short-term economic change with which we are familiar are: Clark Warburton, "Monetary Expansion and the Inflationary Gap," *American Economic Review* (June 1944) pp. 302–27, and "The Volume of Money and the Price Level Between the World Wars," *Journal of Political Economy* (June 1945) pp. 150–63; Milton Friedman, "Price, Income, and Monetary Changes in Three Wartime Periods," *American Economic Review* (May 1952); Milton Friedman and Gary Becker, "A Statistical Illusion in Judging Keynesian Models," *Journal of Political Economy* (February 1957) pp. 64–75; Karl Brunner and Anatole Balbach, "An Evaluation of Alternative Monetary Theories," *Proceedings*, Western Economic Association (1960).

[2] See Donald D. Hester, "Keynes and the Quantity Theory: A Comment on the Friedman-Meiselman CMC Paper," "Reply to Donald Hester," and Hester's "Rejoinder," *Review of Economics and Statistics* (November 1964).

[3] To some extent this is unfortunate. We believe that further experimentation with measures of money, particularly in the direction of weighted aggregates of different categories of mone-

tures" could hardly be sharper. Among us, we have produced more measures than there are critics. We settled on one; AM on a different one, which is the sum of two separable components; DM, after running "basic tests . . . on 20 different, but not unreasonable, definitions of autonomous expenditures" settle on two, but also carry two others along for the ride; Hester came up with four measures that only partly overlap the others. And all of us harbor serious doubts about the measures we settled on. However useful "autonomous expenditures" may be as a theoretical construct, it is still far from having any generally accepted empirical counterpart.

This contrast between the stock of money and autonomous expenditures is relevant in judging the import of the gauntlet of tables run by the reader who has arrived at this point. The single set of relations between money and the dependent variables comes from the first test runs. The multiple sets of relations between autonomous expenditures and the dependent variables are the end-product of a whole series of test runs. We could, of course, have made further test runs for alternative measures of money. No doubt we could have found measures that were more highly correlated with income and consumption than the measure we used, especially if we had restricted ourselves to recent years. We have refrained from doing so in order not to confuse the issue, which is not how high a correlation can be wrung from a computer but rather how well two alternative theories correspond to experience when the central concepts in the theories are defined either by earlier work or by reasonably objective criteria applied in advance.

Where our critics disagree with us is that they believe we have understated the independent role of autonomous expenditures operating through a multiplier process. They claim first, that the particular measure of autonomous expenditures we adopted was poor; second, that its use produced results that were biased against the income-expenditure approach; and third, that our procedure for comparing the relative stability of monetary velocity and the investment multiplier is too simple—too "one-equation" an approach.

We readily plead guilty to two of the three charges. Indeed, we emphasized in our original paper that our measure of autonomous expenditures is unsatisfactory and that our approach is too simple. However, in our opinion, our critics have neither established that our measure biased the results nor demonstrated that their alternative measures are more defensible in terms of relevant criteria. They have not shown what

tary assets, may well be extremely fruitful. See Roy Elliott, "Savings Deposits as Money," unpublished Ph.D. dissertation, University of Chicago (1964); J. G. Gurley, *Liquidity and Financial Institutions in the Postwar Period*, Study Paper No. 14, U.S. Congress, Joint Economic Committee, Employment, Growth, and Price Levels, Washington (1960); Edward J. Kane "Money as a Weighted Aggregate," *Zeitschrift für Nationalökonomie* (1964), pp. 221–43.

was wrong with the criterion we used to select a measure of autonomous expenditures and they have not developed alternative criteria capable of being applied to the data. The attempt in AM's Appendix to compare more complex models is commendable but unfortunately unsuccessful for reasons spelled out in our comments on their Appendix.

In the course of trying to satisfy ourselves whether our critics' objections are valid, we formulated more explicitly than in our article the income-expenditure model used in our analysis. We also clarified the theoretical requirements for a valid comparison of the two models in the simple way we compared them. These results, presented partly below in Sections III and IV.A. and partly in our reply to Hester, may contribute more to progress on the main problem than the specific answers to specific criticisms and the countercriticisms that fill the rest of our reply—but then the chaff is often more bulky than the grain.

Our initial draft of this reply was written in response to an earlier version of the AM paper. In our revision, we have tried to incorporate comments on the DM paper as well, but no doubt the final version still bears marks of its origin. Because the DM paper partly duplicates the AM paper, we have generally replied only to the additional subject matter introduced by DM. We have used throughout AM's notation, though we have had to add a few symbols. We found AM's excellent Table 1 invaluable in keeping track of the many balls being juggled.

We apologize for quoting extensively from our paper. We do so to let the reader judge for himself whether we are right in our belief that our critics have given a misleading impression of the aim and content of our paper. We hope also that these tidbits may entice readers who have not yet done so to read our complete paper.[4]

At first sight, the most persuasive argument in both critiques is the alleged statistical demonstration that altering the definition of autonomous expenditures "changes drastically" (AM's words) our conclusions. We shall therefore deal first with this allegation, waiving, for the time being, all questions about the theoretical validity of the procedures used by our critics. We shall then turn to some debating points raised by our critics that simply confuse the issue, and only then, in Sections III, IV, and V, to the substantive matters that are of Scientific importance.

I. *Does Changing the Definition of Autonomous "Drastically" Change Our Conclusions?*

From data for the period 1897–1958 (annual for the whole period and also quarterly for 1946–58) we concluded that "the income velocity of

[4] There is some duplication between this reply and our reply to Hester, whose critique, as AM and DM note, to some extent overlaps theirs. We have tried to reduce duplication by cross-reference but have not been able to eliminate it entirely. See Friedman and Meiselman, *op. cit.*

circulation of money is consistently and decidedly stabler than the investment multiplier except only during the early years of the Great Depression after 1929" (p. 186).[5] AM and DM maintain that this conclusion results from our use of an inappropriate concept of autonomous expenditures (A). AM assert that substitution of their alternative concept ($Z^a + X^a$, which we shall designate A^*) reverses our conclusion. As evidence, they present correlations for the years 1929–58, excluding 1942–46. For those 25 years, the correlation between consumption (C) and A^* is decidedly higher than between C and money (M), which in turn is decidedly higher than between C and A.

DM prefers two other concepts of autonomous expenditures, one on a net basis and one on a gross basis (which we shall designate A^{**} and A^{***} respectively). Their evidence consists largely of correlations between C and A^{**} or A^{***} for several periods since 1929. The correlation between C and A^{**} is generally somewhat higher than between C and A, and between C and A^{***}, a bit higher still. In most comparisons, the effect is to leave M more highly correlated with C than autonomous expenditures are, but to diminish the margin of superiority. DM also correlate components of A^{**} or A^{***} with C, and find that two in particular, namely, gross fixed private domestic investment and this plus exports, are consistently highly correlated with C, generally more highly than M, A^{**}, or A^{***}. They are somewhat at a loss how to interpret this result.

A minor reason why AM's contention is not justified is because of the difference in periods covered. As we noted, "This [the early years of the Great Depression] is the only period when the relations between autonomous expenditures and consumption are impressively close either relative to the money relations or absolutely. This is, of course, also the period that gave rise to the income-expenditure theory in its present form and that sparked the conversion of a large fraction of economists to it" (p. 188). We also pointed out that even this exception was eliminated if the calculations were made for 1930–39 instead of for 1929–39. The exceptional period, 1929–39, which AM extended to 1941, contains more than half the years for which AM make calculations. Had we restricted our calculations to the years since 1929, we would have reached much less clear-cut conclusions than we did. The return of the results after World War II to the pattern prevailing from 1897–1929 enabled us to term 1929–39 the exception rather than the rule. Since data are readily available for the earlier years, we are puzzled why AM did not make their calculations for the time span we analyzed.

[5] All otherwise unidentified page number references are to Milton Friedman and David Meiselman, "The Relative Stability of Monetary Velocity and the Investment Multiplier in the United States, 1897–1958," *Stabilization Policies*, Commission on Money and Credit (New York 1963), pp. 165–268.

TABLE 1—CORRELATION BETWEEN CONSUMPTION AND ALTERNATIVE INDEPENDENT VARIABLES: TWO CONCEPTS OF AUTONOMOUS EXPENDITURES AND THE QUANTITY OF MONEY, FOUR PERIODS, ANNUAL DATA, 1929–58

Period	Correlation Coefficient Between C and			Squared Standard Errors of Estimate (S^2_E) for Regression of C on			Standard Error of Estimate as a Percentage of Mean Value of C for Regression of C on		
	A	A^*	M	A	A^*	M	A	A^*	M
1929–41	.936	.906	.910	16	23	23	6.3	7.5	7.4
1930–41	.939	.939	.965	14	14	8	6.0	5.9	4.5
1947–58	.808	.976	.989	704	98	47	11.7	4.3	3.0
1929–58, exc. 1942–46	.964	.996	.989	601	69	174	16.9	5.8	9.2

The major reason why AM's contention is not justified is very different. They have confused secular shifts with the year-to-year changes that are relevant to the central issue. That issue, as we pointed out, "has to do with short-run relations. . . . Here the chief assertion [of the Keynesian view] is that . . . short-run changes in income can be regarded as largely caused by and reflecting corresponding short-run changes in investment expenditures" (p. 167). Hence, we stated, "it seems desirable to make the comparisons [between the income-expenditure and quantity theories] for relatively short periods" (p. 174).

AM apparently overlooked this caveat. All the calculations in their text are for the 25 years 1929 through 1941 and 1947 through 1958 taken as a whole. Since their calculations are all for aggregates in nominal values (we made calculations also in real terms) and since prices roughly doubled from the first set of years to the second and population rose by about 25 per cent, it is clear that their calculations are for two distinct sets of observations clustered about very different means. The results of any calculations that do not allow for trend are therefore dominated by the differences between the means for the two periods and have little to say about the short-run relations that are at issue.[6] When the relevant ones of their calculations are made for each period separately (1929 through 1941 and 1947 through 1958), the results turn out to be consistent with our earlier results and to require little change in our conclusions.[7]

These statements are documented in Table 1, which gives simple cor-

[6] Of the total variance of consumption for the 25 years, 88 per cent is accounted for by the differences between the means for the two subperiods.

[7] In the present version of their paper, AM refer in footnotes 23 and 39 to calculations for the periods 1929–41 and 1947–58 separately. These references did not appear in the original version, though the present reference to unspecified alternative "subperiods" in their footnote 8 did.

relation coefficients for three regressions and four periods.[8] The reason for including 1930–41 is to check whether the conclusion we reached in our paper that the exclusion of the 1929 observation eliminated the exception also holds for the AM definition of autonomous.

If we had used A^* instead of A and had still correlated it with C (which we would not have done, see Section III.C, below), and if we had used 1929–41 instead of 1929–39, our conclusion about the greater stability of velocity than the multiplier would have been slightly strengthened for the prewar period, and greatly weakened for the postwar period. For the prewar period, the correlation between C and M is higher than between C and A^* even for 1929–41.[9] For the postwar period, the correlation between C and M exceeds that between C and A^* by a much smaller margin than it exceeds that between C and A. Since the difference between A^* and A would very likely be even less for the pre-1929 period than for 1929–41, presumably the results for still earlier years would be much the same for A^* as for A.[10] Hence, the substitution of A^* for A would have required only a minor change in our final conclusions, which were based entirely on the results for subperiods and not on those for lengthy periods. The correlations for longer periods were included simply as additional information.

AM make much of the superiority of the squared standard error of estimate over the correlation coefficient as a measure of goodness of fit. This is purely a debating point that has no relevance to any of our results. *When the dependent variable is the same*, the squared standard errors for different sets of independent variables are directly proportional to squared correlation coefficients. In our paper, we made no comparison for which the italicized qualification is not satisfied. Hence, substitution of squared standard errors for the correlation coefficients we presented would have changed nothing of substance. When the dependent variables are not the same, both correlation coefficients and standard errors are likely not to be comparable. That is precisely why we insisted that every comparison between the alternative theories should involve the identical dependent variables, both in concept and years covered.

To keep our results as close to AM's as possible, however, we have supplemented the correlation coefficients in Table 1 with squared standard errors of estimate (their S^2_E). To bring out some of the issues involved in interpreting this measure, we have included also the percentage error ($100\ S_E/\bar{C}$), which perhaps comes closer than the absolute error to being comparable for different periods of time. It will be noted that only for M is the squared standard error larger for the whole

[8] We are indebted to Ando and Modigliani for making some of their data available to us.

[9] However, for 1929–39, r_{CA^*} is higher than r_{CM}, though lower than r_{CA}.

[10] The correlation between A and A^* is .977 for 1929–41, .883 for 1947–58.

TABLE 2—PARTIAL CORRELATION BETWEEN CONSUMPTION AND ALTERNATIVE INDEPENDENT VARIABLES: TWO CONCEPTS OF AUTONOMOUS EXPENDITURES AND THE QUANTITY OF MONEY, LAGGED CONSUMPTION HELD CONSTANT, FOUR PERIODS, ANNUAL DATA, 1929–58

Period	Square of Multiple Correlation Coefficient, C regressed on C_{t-1} and			Partial Correlation Coefficient (C_{t-1} Held Constant) Between C and			Standard Error of Estimate as Percentage of Mean Value of C for Regression of C on			
							C_{t-1}	C_{t-1} and		
	A	A^*	M	A	A^*	M		A	A^*	M
1929–41	.974	.940	.886	.96	.92	.84	11.1	3.0	4.6	6.3
1930–41	.978	.968	.947	.97	.96	.94	11.4	2.7	3.3	4.2
1947–58	.987	.990	.992	.02	.46	.57	2.2	2.3	2.1	1.9
1929–58, excl. 1942–46	.997	.997	.997	.61	.69	.66	4.6	3.7	3.4	3.5

period than for any of the subperiods. The reason, as is obvious from a glance at our Chart II-8 (p. 198), is that there was a sizable shift of the regression of C on M from before to after the war. The slope was not much altered but the position was.

In our paper, we supplemented correlations like those in Table I with correlations between first differences, in order to assure that our results were not produced simply by common trends.[11] For the same purpose, AM use an alternative technique: the inclusion of lagged consumption as an independent variable. This allowance for trend eliminates most of the distorting effect of combining two very different periods, but even so, the results for the period as a whole are less illuminating than those for the separate periods, as Table 2 shows. The only appreciable difference from Table 1 for the subperiods is that the exclusion of 1930 does not suffice to reverse the order of the correlations: that between C and either autonomous concept remains higher, though by a much reduced margin, than between C and M. For the postwar period, the trend of consumption is so regular that the variation over and above that accounted for by prior-year's consumption is very small (2.2 per cent). Allowing for M as well reduces this percentage error to 1.9 per cent, or by over twice as much as allowing for A^* reduces the percentage error. Allowing for A renders matters worse.[12] Given this situation, the

[11] Though we plotted all the first differences and gave some correlation coefficients in the text, we did not publish tables of our first difference correlations comparable to those we published for correlations among levels. This was a mistake that we have tried to rectify by publishing the basic tables in our reply to Donald Hester, Tables 1 and 2, pp. 375 and 376, *op. cit.*

[12] The *sum* of squared residuals is, of course, reduced by including A, but by a smaller fraction than the number of degrees of freedom is reduced. Hence the mean square residual per degree of freedom is raised.

quarterly data available for this period are especially valuable. The analysis of them in our paper—on both a synchronous and lagged basis—confirmed the results we obtained from annual data.

When account is taken of trend, the results for A^* are again much the same as those we obtained for A with a different method of allowing for trend. Again, the substitution of A^* for A worsens the income-expenditure correlations for the pre-World-War II years but still leaves them higher than the quantity-theory correlations, and improves the income-expenditure correlations for the post-World-War II years, but still leaves them lower than the quantity-theory correlations.[13]

DM do consider prewar and postwar periods separately, which is why they get no such striking reversals of our results as AM assert they get.

II. *Some Debating Points*[14]

A. *Treatment of War Years*

AM take us strongly to task because, they say, we "do not even once mention the possibility that any of [our] results might be distorted by the inclusion of the war years!" This is a debating point pure and simple, which AM are led to raise only by their own statistical error of treating the period from 1929–58 as one whole. It has no relevance to our paper, as the following quotations demonstrate.

> In choosing the periods of time, we have taken several considerations into account. First, since the question at issue is mainly the short-term stability of the relations being compared, it seems desirable to make the comparisons for relatively short periods. Second, since the relations may differ at different phases of the cycle, it seems desirable that any one comparison should cover one or more complete cycles. . . . Third, since most of the available data are annual, single business cycles generally provide too small a number of observations to yield statistically meaningful results. The compromise we have adopted among these somewhat conflicting considerations is to divide the period for which data are available into two sets of overlapping segments, one set marked out by the troughs of the major depressions during the period (1896, 1907, 1921, 1933, 1938) except for the post-World-War II period, which we have marked off simply by the end of the war; a second set, by peaks intermediate between the troughs of major depressions, except again for dates separating out World War II. . . . The dates we have used are 1903, 1913 (to get a period

[13] Many of AM's other results are also drastically changed by considering each period separately. For example, they conclude that including Z^a and X^a in the correlations separately rather than only their sum reduced the residual variance by about one-third. For the separate periods, the residual variance is sometimes larger, sometimes smaller. For some periods, the coefficient of X^a turns out to be negative.

[14] We leave out of this section the charge that we have misspecified the consumption function because we deal with that in Section IV.B, below.

excluding World War I), 1920, 1929, 1939, (to get a period excluding World War II), 1948, and 1957. We have made computations for the period as a whole as well as the separate segments (pp. 174–75).

The results turned out to be so consistent, except for the 1929–39 decade, that we had no occasion to discuss the results for other subperiods in detail: the peacetime subperiods alone gave the same results as did the subperiods including some war years; and our conclusions were based on the results for the subperiods, not for the period as a whole.

B. *One-Equation-One-Independent-Variable Models in Search of the Highest Correlation*

AM several times characterize our procedure in these or very similar words, describe us as "arbitrarily" imposing these conditions and as picking the variable *A* "arbitrarily," and say "there is no justification for FM's juxtaposition of the income-expenditure framework and the quantity theory model as mutually exclusive hypotheses."

A few quotations from our article will help to put these remarks in their proper light.

1. *Simple versus sophisticated models.* We wrote:

The central issue in dispute is not theoretical but empirical. It is easy enough to construct an analytical system that embodies both the relations between investment and consumption and the relations between money and income, that, is, both the multiplier relations and the velocity relations. Economists who regard monetary changes as primary are divided from those who regard them as secondary much less by different theoretical systems than by different empirical judgments, different judgments about which set of relations in the more generalized theoretical system is (a) critical in the sense of being in practice the primary source of change and disturbance and (b) stable in the sense of expressing empirically consistent relations which can be depended on to remain the same from time to time. . . .

In seeking to examine the relative stability of velocity and the multiplier, we faced an initial choice between two major approaches. The issue can be investigated by a sophisticated analysis involving many variables. Such an analysis must inevitably be restricted to a narrow segment of space and time. . . . Alternatively, the issue can be investigated initially on a rather simple level for a wide range of space and time. Our choice has been the second. It is our view that the issue that divides economists is extremely basic and one that should lend itself to a common answer over a wide range of circumstances. If it does not, it means that the dichotomy posed is much too simple, that the key issue is not which view to accept but rather the circumstances under which the one or the other view is likely to be the more fruitful. Moreover, in an investigation of this kind, it seems better to rely ini-

tially on a wide range of evidence interpreted on a rather simple level than on the more indirect and longer chain of connections inevitable in a sophisticated analysis resting on a narrower base (pp. 168–70.)

The fact that we have done so [neglected most refinements] makes it necessary to emphasize that our results cannot be decisive. On the simple level on which we propose to test the two theories, equation (1) [the quantity theory equation] might turn out to be better than equation (2) [the income-expenditure theory equation], or conversely; whereas on a more sophisticated level, when additional variables are introduced, the relative advantage of the two might be reversed. This possibility cannot be ruled out, although it seems a reasonable presumption that the relationship which explains the most in its simplest version is the relationship that will be most fruitful to explore further to convert into a more sophisticated form (p. 174).

2. *Picking A "arbitrarily."* We wrote:

One by-product of this investigation was the discovery that there is neither clear-cut agreement on the specific statistical definition of autonomous and induced expenditures nor any well established criteria for choosing particular definitions for a particular problem or period or body of data (p. 180).

In our actual empirical work, much the greatest amount of time was spent in trying to draw the appropriate boundary lines [between autonomous and induced expenditures] rather than in the calculations and analysis designed to compare and test the two hypotheses. We are by no means satisfied that we have used the appropriate criteria in drawing the lines. Neither are we satisfied with the precise lines we have drawn, some of which we regard as highly tentative. Much further work remains to be done on this fundamental problem, in particular in determining statistical tests for making the best choice (p. 181).

Pages 181 to 183 of our paper are then devoted to developing the criterion we used (see the excerpts from these pages in the next subsection), and a 16-page Appendix A, "Selection of Variables," summarizes the statistical tests we made. In the course of our tests, we considered explicitly and rejected definitions very close to the one AM uses.[15]

We quite agree with AM and DM that if A^* or A^{**} or A^{***} is a better measure of autonomous than A, then the correlations we computed are not the right ones for comparing the goodness of the two alternatives.

[15] AM do not even mention our criterion for choosing the concept of autonomous expenditures, or our explanation of why it is that the decision to include the net foreign balance rather than exports in A is entirely consistent with our regarding imports as being induced, and similarly, our decision to include the net deficit rather than government expenditures, with our regarding taxes as being induced.

DM at least note in their footnotes 15 and 16 that we explicitly considered alternative definitions, but they do not discuss how we did so.

See our reply to Hester for a fuller discussion of these points, *op. cit.*, pp. 374–75.

However, they must equally agree that if A is a better measure, their correlations are not the right ones. And AM and DM give no evidence, theoretical or empirical, that we have used the wrong criterion in selecting A or that we interpreted the empirical evidence we present incorrectly. AM merely assert, without evidence, that A^* is better, justifying its selection only by their intuition about what is "beyond reasonable doubt," and, for government expenditures and receipts, by reference to a paper by Ando, Brown, and Adams. The same intuitions are the sole basis for their first charge that our estimates of the investment multiplier have a downward least-squares bias. If A is a better measure than A^*, then of course their estimates, not ours, are biased.

Similarly, although DM refer to "theoretical and empirical grounds discussed in the preceding sections" for rejecting our measures, so far as we can tell from a careful reading of the preceding sections, no theoretical grounds whatsoever are given for such a conclusion, only references to "expenditures which are usually considered autonomous," to what "might be," and to how the treatment of inventory investment as autonomous if in fact it were induced "leads to wrong results," but with no theoretical (or other) evidence to justify the conclusion that inventory investment should be excluded from the total called autonomous. The only empirical evidence cited consists either of assertions about what items are or are not endogenous, or simple correlations with consumption expenditures (referred to later as "basic tests".)[16] As is clear

[16] Casual empiricism runs rampant throughout both papers, as the following examples, for which no evidence is cited, testify. From AM:

1. "The extreme short term variability of undistributed corporate income makes it unlikely that consumption would significantly respond to it from year to year" (footnote 1).

2. "Since S includes terms correlated with ϵ, it will in general be itself correlated with ϵ." This is an empirical not logical statement since the correlations of separate terms with ϵ may be such as to cancel.

3. "The expression in brackets . . . is positively correlated with ϵ." This and the preceding item exemplify the kind of untested assertions on which AM base their charge of "least-squares bias."

4. A hypothetical model for which no evidence is given and which is for an economy in which there are no corporations and no foreign trade is cited as one of three items justifying the proposition that "the laborious battery of tests presented by FM is basically irrelevant for the purpose of assessing the *empirical* usefulness of the income-expenditure framework" (italics added).

5. "The condition of *given* income is more likely to be binding in the full employment situation, even in terms of current prices," i.e., prices are constant at full employment (footnote 6).

From DM:

1. "These two consumption functions are likely to show substantial difference in predictive power."

2. "If, as seems highly probable, consumption is a function of the previous year's income as well as of current income. . . ."

3. "An increase in consumption has a positive effect on inventory investment predominantly in the same year. On the other hand, an increase in consumption can lead to unplanned disinvestment in inventories"—so anything can happen. AM are less indefinite—they come out solidly for the negative relation.

from the quotation in the next subsection, this is not a valid test, whether or not it be "basic."

We must confess that we are puzzled by the smoke screen thrown up by both pairs of critics on this issue. If the concept of "autonomous" expenditures is to be useful as something more than an incantation for the faithful and an empty box for our ignorance, there must be some objective and operational criteria for giving it empirical content, something more clearly reproducible than intuition and unstated "theoretical and empirical" grounds. It is hardly satisfactory to have some score of alternative definitions all in the running at the same time. Our criterion (see next subsection) may not be a good one or we may have applied it incorrectly, but if so, we should appreciate it if our critics would explain what is wrong with what we have done.[17]

Incidentally, even if A^* or A^{**} or A^{***} were a better concept than A, the correlations AM and DM compute are not the right ones for comparing the goodness of the two alternative theories (see Sections III.C and IV.C below).

3. *"Search for the highest correlation."* On this point, which is closely related to the preceding, both AM and DM have simply confused, as they have throughout their papers, two different issues: (i) comparison of goodness of fit of two alternative models; (ii) selection of variables to use in the models.

As already pointed out, the correlation coefficient is an appropriate criterion of the relative goodness of fit of two alternative models when the dependent variables they predict are the same. This is the one and only purpose for which we used the height of the correlation coefficient as a test. As noted, the results would have been identical if we had used residual variance. On the other hand, in discussing the problem of distinguishing between autonomous and induced expenditures, we said in our original paper (pp. 181–83):

> What criterion should be used to fix the boundary lines? One simple method is to correlate alternatively defined measures of the independent variable with the dependent variable [income] and then select the concept which yields the highest correlation. . . . Applying this criterion to the definition of autonomous reveals, however, that it is not satisfactory. . . . It is possible to get a correlation as close to unity as desired, simply by including all items of income that vary

4. "The government deficit . . . is . . . partially endogenous . . . tax receipts are clearly endogenous . . . exports [are] largely autonomous."

5. "Since an increase in domestic consumption" raises "prices" (footnote 20). This footnote contains several other fine examples.

[17] In his rejoinder to our reply, Hester offers a criticism of our criterion that has some merit. He points out that our criterion handles only one questionable item at a time and may give misleading results because of omitted items. This is true, but he has suggested no alternative that is free of this defect.

much over time in "autonomous," which is to say, by correlating these variable items with themselves. The procedure adopted above to evade the difficulties raised by the spurious correlation, namely, correlating the rest of income with autonomous expenditures, is no solution for the present problem, since each definition of autonomous would then be correlated with a different variable, . . . [and] the resulting correlations would not be comparable. . . .

[An] alternative approach to the definition of autonomous expenditures can be illustrated by considering the question whether durable consumer goods should be included in consumption or in autonomous expenditures. Let D stand for consumption expenditures on durable goods, N on non-durable goods, C for their total, and A for autonomous, according to some tentative definition that excludes durable consumer goods but settles other doubtful items. The question to be decided is whether $D+A$ or A alone is a preferable definition for autonomous expenditures. If D and A were perfect substitutes as autonomous or income-generating expenditures, then a shift of $1 from D to A or from A to D would have no effect on N. Hence N would tend to have a lower correlation with either D or A alone than with their sum. Consequently, this approach implies that a necessary condition for the inclusion of D in autonomous is that

$$(1) \qquad r_{N(D+A)} > \left[\begin{array}{l} r_{ND} \\ \text{and} \\ r_{NA} \end{array}\right.$$

The requirement that the sum of autonomous and induced expenditures equal income gives rise to a similar test in the other direction, ... Suppose (1) is not satisfied. If this occurred because D was a part of induced expenditure along with N, one might expect shifts between D and N to be independent of changes in A. Changes in A would affect only their sum. But this would imply that

$$(2) \qquad r_{A(D+N)} > \left[\begin{array}{l} r_{AD} \\ \text{and} \\ r_{AN} \end{array}\right.$$

This approach therefore yields the following criterion:

Possibility	*Condition (1)*	*Condition (2)*	*Conclusion*
(a)	Satisfied	Not satisfied	D autonomous
(b)	Not satisfied	Satisfied	D induced
(c)	Satisfied	Satisfied	Ambiguous
(d)	Not satisfied	Not satisfied	Ambiguous

There is nothing about the arithmetic of the relations among the correlation coefficients that requires either (a) or (b) to hold. It is entirely possible, and in our work has frequently happened, that

> either (c) or (d) should hold. In consequence, this criterion is not one that is necessarily decisive.
>
> Unfortunately, however, we have been able to devise no criterion that seemed better to us. Consequently, we have employed the criterion just outlined. When the results have been ambiguous, we have followed the procedure that seemed more in accord with the general presumptions in the literature about income-expenditure relations.
>
> In applying the criterion, we have in each case set up the problem as in the above example. That is, we have tentatively decided all questions of inclusion or exclusion except one, leaving us with a division of total income into three parts, the treatment of one of which is in doubt.

Clearly, we did not use the highest correlation to fix the boundary lines between autonomous and induced expenditure or to choose the appropriate definition of money. Our criterion uses correlation coefficients but in a very different and more sophisticated way. It is AM and DM, not we, who implicitly use the highest correlation as a means of selecting the definition of variables, when they cite the higher correlations they get as relevant evidence of the superiority of their definitions over ours.

C. *Use of Same Data for Choosing Definitions and Testing Hypothesis*

DM criticize us for using the same data for defining variables and testing hypotheses incorporating these variables. In this criticism they presumably refer only to our tests for 1929–58, since these are the only years which we use in constructing our definitions, though we tested the hypotheses for the whole period 1897–1958.

For the years 1929–58, their criticism would be entirely valid if indeed we had used the highest correlation as a means of selecting the definition of variables. In that case, we would have been using up degrees of freedom, as it were, in constructing definitions that we later used over again in testing hypotheses. However, as pointed out in the preceding section, we did not use the highest correlation as a criterion. In effect, we used different information for the same years in deriving definitions and in testing hypotheses. However, the two classes of information probably overlap. Hence, the criticism is partly valid. Certainly we would prefer to use independent evidence, but where are we to get it? DM talk about a priori definitions. But from nothing you can get nothing. They are simply using "a priori" as a euphemism for casual empiricism organized by unstated criteria, so, in effect, they, and AM too, are using, in a less explicit way, the same data to define terms and test hypotheses. This is inevitable in scientific work. The cure is ultimately in the cumulative weight of evidence.

It is amusing that this criticism of DM precisely cancels one of AM's. If we had done what DM criticize us for, the effect would have been to bias upwards the correlations for both the quantity theory and

income-expenditure hypotheses, but to bias the latter more than the former because we considered a much wider variety of definitions for autonomous expenditures than we did for the stock of money. Yet AM criticize us for a procedure that they regard as biasing the income-expenditure correlations downwards.

III. *The General Theoretical Issue*

The theoretical exercise of AM's Section I(iii) is unnecessary. They are led into it by two factors: (1) They attempt to derive our equation from a consumption function we did not use. (2) They and DM attribute to us an unexplained and unexplainable desire to correlate consumption with other things regardless of the definition of autonomous expenditures. We had no such desire. What we used as a dependent variable was critically dependent on our concept of autonomous expenditures; had we chosen a different concept of autonomous expenditures, we would have used a different dependent variable.

Since AM and DM as well as Hester have misunderstood us on both points, we obviously failed to make it clear that our theoretical structure for income-expenditure analysis is the standard structure used by all of them and by most other economists. Instead of following the detailed convolutions of AM's analysis, we shall try instead to rectify the defects of our prior exposition by setting forth explicitly the general theoretical structure we used, even at the risk of repeating what is well known.

A. *The Simple Model*

The central feature of simple income-expenditure models is that they distinguish between two categories of expenditures entering into the income stream: expenditures that are closely linked to (induced by) current income flows, and expenditures that are autonomous, not in the sense of being random or arbitrary or unexplainable but simply in the sense of not being determined by current income flows. This distinction between induced and autonomous expenditures—not that between consumption and investment—is fundamental. The tendency has arisen to use the term "consumption" as synonomous for "induced" because of the empirical judgment that consumption (or, better, change in consumption) is predominantly induced and, conversely, that the bulk of induced expenditures consists of consumption. Because our definitions led to this result, we adopted the common terminology. We now believe that it would have contributed to less misunderstanding if we had been pedantic and had used the term "induced" throughout instead of simply including it in a parenthesis after our first use of the word consumption, and then dropping it (p. 175).

To formalize this simple model, let U stand for induced expenditures (since AM use the more mnemonic I for imports), A for autonomous expenditures, and Y for income. We then have, neglecting disturbances,

$$U + A = Y, \tag{1}$$

$$U = f(Y), \tag{2}$$

or

$$U = g(A). \tag{2a}$$

No essential complications are introduced by subdividing U into components, provided all can be treated as induced by the same income total, or by subdividing A into components. Equations (1) and (2a) may be reduced-form expressions for much more complicated models.[18]

[18] A simple example will perhaps clarify the point in question. To avoid complications, assume there are no corporations and no government but there is foreign trade. Using AM's notation, let

$$\text{(a)}\quad Y = C + K + E - I,$$
$$\text{(b)}\quad C = a + bY,$$
$$\text{(c)}\quad I = c + dY,$$

so C and I are both induced and both by Y.

If both K and the net foreign balance are treated as autonomous (as we did in our paper) then, in the notation of equations (1) and (2),

$$\text{(d)}\quad U = C$$
$$\text{(e)}\quad A = K + E - I,$$

and equation (2a) becomes

$$\text{(f)}\quad C = \frac{a}{1-b} + \frac{b}{1-b}A.$$

In addition, I can also be expressed as a function of A, yielding

$$\text{(g)}\quad I = c + \frac{ad}{1-b} + \frac{d}{1-b}A.$$

Alternatively, if K and E are treated as autonomous (as AM do), then in the notation of equations (1) and (2)

$$\text{(h)}\quad U = C - I$$
$$\text{(i)}\quad A = K + E.$$

Equation (2a) becomes

$$\text{(j)}\quad U = \frac{a-c}{1-b+d} + \frac{b-d}{1-b+d}A.$$

C and I can also be expressed separately as functions of A yielding:

$$\text{(k)}\quad C = \frac{a - bc + ad}{1-b+d} + \frac{b}{1-b+d}A$$

$$\text{(l)}\quad I = \frac{c - bc + ad}{1-b+d} + \frac{d}{1-b+d}A.$$

Similarly, no essential complication is introduced by having more than one income total, provided the differences between them consist of items that can themselves be regarded as induced.[19] The situation is different, however, if different components of U are induced by different income totals, which themselves differ by noninduced components, or if we are interested in forecasting, or determining, an income total which differs by a noninduced component from the one that can be regarded as inducing U. The model must then be made more complex.

B. *A Slightly More Complex Model*

To illustrate, suppose it is desired to predict Y of equation (1), but that U is regarded as induced by some other income total, say Y'. Equation (2) then becomes

$$U = f(Y'). \tag{3}$$

Let us define

$$A_1 = Y' - U \tag{4}$$

$$A_2 = A - A_1, \tag{5}$$

so that we can rewrite (1) as

$$U + A_1 + A_2 = Y \tag{1a}$$

and the counterpart of (2a) as

$$U = g(A_1). \tag{2b}$$

If either A_1 or A_2 can itself be regarded as induced by either Y or Y'—which is not ruled out by the fact that their total is autonomous—this model will reduce to the one of the preceding section. Otherwise, there is no way of solving (1a) and (2b) simultaneously, except by specifying A_1 and A_2 separately. Of course, in a fuller model, this may be done by making A_1 and A_2 functions of variables other than Y or Y'. But on the present level, there is no alternative to having separate figures for A_1 and A_2. AM's theoretical exercise in their Section I(iii) comes from trying to force a model like the one of this subsection into the mold of the one of the preceding subsection by treating A_2 as induced.

C. *The Use and Testing of a Simple Model*

The use of a simple model to "predict" or "forecast" or "determine" income involves four steps: (i) forecasting in some way the value of A

Note that the same basic model [equations (a), (b), and (c)] is compatible with alternative specifications of autonomous expenditures, but that these affect the specific character of the reduced form model. The model specified in AM's equations (4), (5)', and (6)' is another example. See also our reply to Hester for a fuller discussion of this point.

[19] In the example of the preceding footnote, one can readily derive gross domestic output, $Y+I$, as well as Y from the model.

(or of A_1 and A_2 separately); (ii) estimating in some way, presumably from past experience, the form and parameters of equation (2a) [or (2b)]; (iii) using the value of A (or of A_1) in equation (2a) [or (2b)] to forecast U; and (iv) adding U and A to forecast Y.

It is obvious that the income-expenditure theory per se is used only in steps (ii) and (iii). Some other approach (which could, for example, be the quantity theory) is used in step (i) to forecast A. The *usefulness* of the model depends on how easy it is to forecast or, for policy purposes, determine A, and on how large U is relative to Y, but the *goodness* or *validity* of the model depends only on how accurately it forecasts U. If we are to compare the goodness of this model with an alternative, we must either (a) use the alternative to forecast only U also and see how well they do the same job, assuming for both that the values of the independent variables they contain are known with certainty; or (b) append to both models a procedure (i.e., another model) for forecasting their independent variables (A for the income-expenditure model, M for the quantity-theory model) and compare how well the two alternative pairs of models forecast Y. Needless to say, if procedure (b) is used, there is no way of attributing any difference in results to the separate parts of the joint models (hypotheses) tested except by reverting to procedure (a).

In our paper, we used procedure (a). The quantity theory, we said, implies a relation between M and Y. However, we have no "standard" alternative theory for forecasting A and, in any event, our aim is to compare the quantity theory with the income-expenditure theory, not with a joint theory. Hence, we said, we shall revise the quantity theory by taking it to imply a relation between U and M. This puts the quantity theory at a disadvantage, but it enables us to make a valid comparison; if that comparison happens to be in favor of the quantity theory, then the "true" superiority of the quantity theory is greater yet. If it happens to be in favor of the income-expenditure theory, the verdict will strictly speaking have to be "unproved," though obviously if the difference in favor of the income-expenditure theory were large, there would be some presumption against the quantity theory.[20]

[20] We do not understand AM's footnote 24, which seems simply wrong. If, as they say, "the only meaningful measure for performance of these models is the residual variance of NNP given the autonomous variables," and if, as in the present instance, some of the autonomous variables are components of NNP, then, as we pointed out in our paper (see quotation above from our pp. 181–83), this residual variance can easily be made as small as anyone likes by choosing the autonomous variables to be the counterpart of a larger and larger part of NNP. We can make sense out of their statement only if the proviso is attached that the autonomous variables are items other than components of NNP which is not how they apply it. For example, the quantity theory model would satisfy this proviso, the quantity of money being the autonomous variable. See also Friedman and Becker, *op. cit.*

It follows from this discussion that, in the simple model, what should be correlated with the autonomous variable depends on how the autonomous variable is defined. We used C as the dependent variable because that turned out to be our U. Had we used A^* as the autonomous variable, we would have had a different U and would have used a different dependent variable.

It follows also that, by using the more complex model, this procedure can be rendered meaningless in a way like that outlined at the beginning of the lengthy quotation given above from pages 181–83 of our original paper. Designate as A_2 all items of income that are highly variable and not closely related to other components of income. Separate the rest of income into two parts, U and A_1, which show smooth and regular movements. These two parts will be highly correlated, so knowledge of A_1 will give excellent estimates of U. If we then *assume* that A_2 as well as A_1 is known with certainty, the error in estimating total income from A_2 and A_1 will be small. However, what has been done in the process is essentially to scrap the income-expenditure theory as an explanation of fluctuations in income. The major fluctuations are relegated to A_2 and this is *assumed* to be known by some other unspecified theory and to exert no influence on income through the multiplier process. The great appeal of the income-expenditure theory is precisely that it professes to explain movements in aggregate income through the multiplication of fluctuations in autonomous expenditures. The whole question that the theory is designed to answer is simply begged if the part of income that is hard to predict is assumed to be outside the multiplier mechanism.

We stress this possibility because it is precisely what DM have done and explains the high correlations that puzzle them. In their Table 2, the concepts of autonomous expenditures in the final two columns (gross fixed private domestic investment and this plus exports) cover only a small part of the items that need to be added to consumption (their dependent variable) to get income (however defined). And the omitted items are the most variable and hardest to predict.[21] Instead of finding an answer to the initial question, they have in effect found a question to which there is an answer. This same comment applies to their so-called "basic tests" of 20 different definitions of autonomous expenditures.

IV. *Some Specific Issues*

Four specific issues require attention: (a) alternative income totals, (b) our consumption function, (c) AM's and DM's specific models, (d) the introduction of M^* to replace M.

[21] For example, inventory investment has a correlation of only .445 with C for 1953–63.

A. *Different Income Totals*

The existence of several income totals that may be of interest derives primarily from the undistributed income of corporations, and the absence of any way to evaluate governmental output analogous to the market valuation of private output. Three such totals were implicitly considered by us and explicitly considered by AM.

1. Y^d = Personal disposable income on a cash basis. On the factor-payments side, this is the current income of consumer units on a largely cash receipts basis after payment of direct personal taxes.[22] On the product side,

$$Y^d = C + S, \tag{6}$$

where C is personal consumption and S is saving defined by equation (6) and hence is a cash concept of saving, excluding any saving in the form of undistributed corporate earnings. Note that consumption is at market prices, so has as its counterpart not only factor payments but also indirect and direct business taxes.

2. Y = Personal income after direct taxes on an accrual basis. This total is connected to Y^d by

$$Y = Y^d + R + W, \tag{7}$$

where R is the undistributed income of corporations, and W is the excess of wage accruals over wage payments.[23] On the factor-payments side, Y, like Y^d, is also the current income of consumer units after payment of direct taxes, but on a largely accrual basis, R adding the increase in the equity of consumer units in corporations, as measured by undistributed net corporate income after taxes, and W converting wage payments from a cash to an accrual basis. On the product side,

$$Y = C + A, \tag{8}$$

where A is what we treated as saving of consumers in our analysis. On the product side, A can also be regarded as the sum of private capital

[22] We say "largely" because of the difficulty of getting from the national income accounts any totals strictly on a cash or accrual basis.

[23] AM include two additional items on the right-hand side, namely, H, the statistical discrepancy, and T^f, foreign transfer payments by government. The first has nothing to do with the concepts involved, only with the particular method of measurement. The Department of Commerce measures some items from the payments side, some from the value of products. Given the accounting identities, this means that there are in fact two different estimates of each item. The inclusion of the statistical discrepancy is a signal that some of the items have been taken from one set of estimates, some from the other. Its inclusion serves to see to it that all items are in effect measured from the same side.

AM are correct in saying that we overlooked T^f. It should not conceptually be included in either Y or A, yet our numbers apparently include it. Since the item is small, indeed nonexistent for most of the period our data covers, its inclusion cannot materially have affected our results.

formation plus governmental expenditures on goods and services financed by borrowing (or, on one interpretation, government capital formation).[24]

3. N = Net national product. This total is connected to Y by

$$N = Y + T, \tag{9}$$

where T can be regarded as governmental expenditures on goods and services financed by taxes.[25] It is not easy to give N a simple interpretation on either the factor payments or product side.

On the factor-payments side, insofar as the taxes are direct taxes (personal income taxes, and corporation profit taxes), N includes total factor payments to individuals on an accrual basis and before taxes. However, it includes more than that, namely, indirect business taxes. There is no simple way to identify these with factor payments which is why they are deducted in computing still a fourth income total used by neither AM nor us, currently termed national income, sometimes termed national income at factor cost, to distinguish it from net national product, sometimes termed national income at market prices. Hence, N has no clear interpretation as a sum of factor payments to individual consumers or income recipients. An alternative possible interpretation in terms of factor payments is to regard government as an ultimate income recipient strictly parallel to the class of individual consumers rather than as an intermediary serving them. T is then the government's net income, $C+A$ the income of the private sector.[26] The total has no special significance for the class of ultimate individual income recipients.

On the product side, the situation is no better. We can write N as

$$N = C + (K + E - I) + G, \tag{10}$$

or as the sum of the market value of (i) consumption expenditures, (ii) private capital formation (net private domestic investment plus the net foreign balance) and (iii) government expenditures on goods and services. The first two have a clear meaning. Both are values of what are regarded as final products. But the third is a mixture of expenditures on (1) intermediate products, which should be excluded to avoid double

[24] This interpretation is somewhat arbitrary because it implicitly involves matching specific governmental expenditures with specific receipts. The governmental item included in A is the deficit on income and product account. Its calculation implicitly treats all government transfer payments as being paid out of taxes rather than borrowing.

[25] The *caveat* of the preceding footnote is relevant here also. As an accounting matter, T is total taxes less government transfer payments, net interest paid by government, and subsidies less current surpluses of government enterprises.

[26] Along the lines of the preceding footnotes, this requires treating transfer payments, etc., as a prior charge against receipts in computing the net income of government.

counting, (2) final products for consumption, which in principle should be treated as part of C, and (3) final products for governmental capital formation, which should be added to private capital formation. The difficulty is that it has proved virtually impossible, despite many attempts, to classify actual governmental expenditures into these categories. The result has been what so often happens: the statisticians measure what they can rather than what economic theory calls for, and then they and economists who use the figures attempt to rationalize what can be measured as what "really" should be measured.[27] The current wide use of N reflects this process, not any resolution of the basic theoretical ambiguities about its meaning.

In an accounting sense, N can be regarded as including indirect business taxes twice: once as part of the value of goods and services included in consumption and private capital formation; and once as part of governmental expenditures. This is the item mentioned above as not capable of being identified as a factor payment and which is therefore deducted (along with a number of other items) from Net National Product to compute National Income.

Despite its ambiguities, N (adjusted for price change) may sometimes be a better index of total "activity" or "output" than Y. Although N includes too much, Y includes too little because some allowance should be made for final output of government. The truth, therefore, seems to be in between. DM implicitly add a fourth income total, Gross National Product, derived from N by adding capital consumption. This is a hybrid concept that has no clear theoretical meaning.

B. *Our Consumption Function and Model*

Y is the income total that we treated as inducing consumption expenditures. AM imply that we should have used Y^d and state that the use of Y involves "grievous misspecification" of the consumption function—though in their footnote 1, they acknowledge that recent work suggests including in the relevant income total one component of the difference between Y and Y^d (R=corporate savings) and three paragraphs later in their text they dismiss the other components of the difference ($H+W+T^f$) as "minor reconciliation items." We share the view described in their footnote but not accepted by them, that recent work by them, by one of us, and by others recommends the use of Y rather than Y^d. However, we were led to use Y not by such considerations but by the empirical evidence and theoretical criterion we used in choosing a concept of autonomous expenditures.

We are not sure we fully understand AM's criticism. Perhaps their

[27] The use of gross rather than net national product is the most glaring other example of this process in national income accounts. See the early volumes of ***Studies in Income and Wealth*** for extensive discussion of these issues.

point is not that Y^d would have yielded an empirically better consumption function but that we should have used Y^d to be faithful to the prior Keynesian usage.[28] If that is their point, it has much merit. While we relied primarily on our own empirical tests, and only secondarily on the literature, we did not in fact test an autonomous concept corresponding to Y^d. In view of the widespread use of Y^d as the argument of consumption functions, we should have done so.

Y is also the income total that we treated as the one that it is desired to forecast or determine. Finally, A is the total that we designated as autonomous. Hence our decisions made our model of the simple type described above in Section III.A.

If we had decided to treat N as the total that it is desired to forecast or determine, our model would still have remained of the same simple type, since we explicitly regarded T as induced, not autonomous. Hence in that case, we would have defined U as equal to $C+T$, and have correlated $C+T$ alternatively with A and with M.[29] We have tried this for periods since 1929, and our conclusions are not much affected.[30]

C. *AM's Consumption Function and Model*

AM explicitly assert that they treat consumption as induced by Y^d rather than Y, which is what gives rise to most of the complexities of their discussion. They describe N as the income total they desire to forecast or determine, and Z^a+X^a as the total of expenditures they regard as autonomous. The model they finally use can be restated in terms of the simple model described above in Section III.A, though like them we have found no easy way to rationalize it. We have

$$\begin{aligned} N &= (C - X^a + Z^i) + (Z^a + X^a) \\ &= \qquad U^* \qquad + \quad A^*, \end{aligned} \tag{11}$$

where U^* is what AM implicitly regard as the net total of induced expenditures, and A^* is what they explicitly designate autonomous expenditures.[31] If Y rather than N is treated as the income total to be

[28] Incidentally, as an aside on the "search for the highest correlation," Y^d is generally more highly correlated with M than Y. See our Appendix Table II-A1, p. 244.

[29] There is a typographical error on our page 256 that misled us and also DM. On line 14, the expression $C+T$ should have been $C-T$.

[30] The standard errors of estimate as a percentage of the mean value of $C+T$ are as follows for the regressions of $C+T$ on A and M:

	$C+T$ on	
	A	M
1929–41	5.0	6.0
1930–41	5.3	4.2
1947–58	11.7	4.3
1929–58, excl. 1942–46	17.7	11.0

forecast or determined, U^*-T becomes the induced component of Y, since AM regard the bulk of T as induced. Minor items aside, U^* equals consumption plus net inventory change minus imports minus transfer payments.

AM describe $C^f=C+Z^i$ as the total of induced expenditures, but this is clearly wrong in the context of a model that purports to explain the whole of income by autonomous expenditures plus multiplier reactions. As just noted, induced expenditures are either $(C-X^a+Z^i)$ or $(C-X^a+Z^i-T)$ according as N or Y is the income total to be determined. AM report that they were led to use C^f by a comment made in a letter by one of us. The point made in that letter is the one made above in Section III.C, namely, that in using the income-expenditure theory to explain income, only induced expenditures are explained by the theory. Unfortunately, they interpreted the wrong total as induced. For testing their concept of autonomous expenditures neither C nor C^f is the relevant total and we can see no advantage to using the one rather than the other. C^f would be the relevant total if Z^a alone were regarded as autonomous.

DM do not recognize that the whole problem is to test a model for predicting or determining a concept of income, but rather treat the problem as one of predicting consumption, presumably for its own sake (contrast their specification of the rival hypotheses in their Section III with our Section III.C above). They assert (in their Appendix) that they regard Y^d as inducing consumption. They are not explicit about what income total they wish to determine and do not recognize that what is relevant is total induced expenditures and that this total shifts with a change in the concept of autonomous expenditures. Interpreted in our framework, the induced component that is implicit in their procedure, if N is the income total to be predicted is as follows:

Concept of Autonomous	Induced Component
A^{**}	$U^{**}=U^*+$State and local government expenditures on goods and services
A^{***}	$U^{***}=U^{**}-$Capital consumption allowances

[31] Minor items aside, their total A^* differs from our A in three main ways: by treating (1) inventory investment as induced rather than autonomous; (2) total exports rather than the net foreign balance as autonomous; (3) total government expenditures for goods and services rather than the deficit or surplus on income and product account as autonomous. See our Appendix A for an explicit discussion of the alternatives in (2) and (3) and for the evidence that led us to decide these as we did. We did not consider item (1). DM's autonomous A^{**} is the same as A^* except that they include only federal government expenditures on income and product account, omitting entirely state and local expenditures. Their A^{***} adds capital consumption to A^{**}. Neither AM nor DM, as noted, give any empirical evidence justifying their decisions.

Gross Fixed Private Domestic Investment +Exports	$U^{****}=U^{***}+$Federal Government Expenditures on income and product account
Gross Fixed Private Domestic Investment	$U^{****}+$exports

In each case, these are the induced expenditures that should be correlated with autonomous expenditures. If Y is the income total to be predicted, T should be subtracted from each induced component.[32]

It may be worth introducing a few numbers to show how AM and DM have emasculated the income-expenditure model. Let us take the mean values for the period 1947–58 for illustrative purposes and compare the division between induced and autonomous elements for their models and our model, and for Y and N as alternative income totals to be determined (Table 3).

By our model, we in effect treated the income-expenditure theory as saying: if you know from other sources what is going to happen to roughly one-tenth of Y or N, then the multiplier analysis will tell you (or give you an estimate of) what will happen to the other nine-tenths. AM convert the model into one that says: if you know from other sources what is going to happen to nearly half of Y or over one-third of N, then the multiplier analysis will tell you what will happen to the other half of Y or two-thirds of N. DM's two models treat only slightly less of total income as autonomous. If AM and DM were to continue along this line of "improving" the model by having it predict a smaller and smaller percentage of income more and more accurately they would soon arrive at the point where it is predicting nothing—perfectly! In the old saw, with such friends, the income-expenditure theory hardly needs any enemies.

Properly to test the relative goodness of the AM model or the DM models and the quantity theory we must, as noted earlier, either force the quantity theory to predict U^*-T (or $U^{**}-T$ or $U^{***}-T$), if Y is to be determined, or U^* (or U^{**} or U^{***}), if N is to be determined, or else supplement the income-expenditure theory by some model that will

[32] Another way to interpret what DM have done is in terms of the more complex model of Section III.B above, with their autonomous concepts as A_1, and consumption regarded as their U and as induced by $Y'=C+A_1$. But this leads to such unusual consumption functions that we have preferred the alternative of interpreting their calculations in terms of the model of Section III.A (e.g., for A^{***}, which is the concept they generally prefer, the alternative interpretation would lead to regarding consumption as a function of N plus capital consumption allowances plus imports less inventory investment and less state and local government expenditures for goods and services).

TABLE 3—DIVISION BETWEEN INDUCED AND AUTONOMOUS EXPENDITURES, FOUR MODELS, MEAN VALUES, 1947–58
(Y=$254.4 billion)
(N=$319.7 billion)

	Model			
	FM	AM	DM	
	A	A^*	A^{**}	A^{***}
Autonomous expenditures (billions of dollars)	27.2	115.0	80.4	106.2
Induced expenditures (billions of dollars) in determination of				
Y	227.1	139.3	174.0	148.2
N	292.4	204.7	239.3	213.5
Percentage of expenditures autonomous in determination of				
Y	10.7	45.2	31.6	41.7
N	8.5	36.0	25.1	33.2
Percentage of expenditures induced in determination of				
Y	89.3	54.8	68.4	58.3
N	91.5	64.0	74.9	66.8

predict A^* (or A^{**} or A^{***}), so that we can test the ability of the pair of models to predict Y or N compared with the ability of the quantity theory to do so. We do not know how to do the second, so we must perforce do the first. In doing so, as we shall for the AM model but not, for want of patience, for the DM models, it should be emphasized how much we are stacking the cards against the quantity theory. It is one thing to change the quantity theory from a relation with income to a relation with a total that accounts for some 90 per cent of income. If the quantity theory works well for the first it should also work well for the second. It is a much more drastic wrench to convert the quantity theory into a relation with half to two-thirds of income. There is no theoretical reason why, even if the quantity theory works well for income as a whole, it should work well for half to two-thirds of income. Hence, this time, there is far more point to the caveat that if the quantity theory under these handicaps does better than the income-expenditure theory, it must have a much greater "true" superiority but that if it does worse, the evidence is inconclusive.

Table 4 shows correlation coefficients and the percentage errors of estimate for regressions of induced expenditures on A^* and M, for the two alternative totals to be determined (Y and N) and for the four periods

TABLE 4—CORRELATION BETWEEN MONEY OR AUTONOMOUS EXPENDITURES AS DEFINED BY AM AND INDUCED EXPENDITURES, FOR TWO ALTERNATIVE INCOME TOTALS AND FOUR PERIODS, ANNUAL DATA, 1929–58

Period	Correlation Coefficient Between U^*-T and		Standard Error of Estimate as Percentage of Mean Value of U^*-T for Regression of U^*-T on		Correlation Coefficient Between U^* and		Standard Error of Estimate as Percentage of Mean Value of U^* for Regression of U^* on	
	A^*	M	A^*	M	A^*	M	A^*	M
1929–41	.790	.814	10.6	10.0	.929	.928	7.1	7.1
1930–41	.858	.921	8.0	6.0	.960	.979	5.2	3.9
1947–58	.904	.958	6.3	4.2	.980	.976	3.7	4.0
1929–58, excl. 1942–46	.984	.991	9.0	6.9	.996	.990	5.2	8.6

Symbols:
M = Money
A^* = Autonomous expenditures as defined by AM
U^*-T = Induced expenditures, when Y is income total to be determined
U^* = Induced expenditures, when N is income total to be determined
T = Taxes less government transfer payments, net interest paid by government, and subsidies less surpluses of government enterprises

we used earlier. These are the relevant correlations for comparing the AM version of the income-expenditure theory with the quantity theory.[33]

If Y is taken to be the total to be determined, the quantity-theory correlations are consistently higher than the income-expenditure correlations, even for 1929–41, though the difference for that period is negligible. If N is taken as the total to be determined, the quantity theory is in a dead heat with the income-expenditure theory for 1929–41, has a slightly higher correlation for 1930–41, and a slightly lower one for 1947–58. Clearly, these results, even waiving all our doubts about whether A^* is a valid definition of autonomous, if anything reinforce our earlier conclusions and certainly do not contradict them.

One interesting feature of Table 4 is the sharp difference between the performance of the income-expenditure theory in determining Y and N. The correlations are much lower and the standard errors much higher when U^*-T is the induced component than when U^* is. There is a difference in the same direction for the quantity-theory correlations, but of appreciably smaller magnitude. The reason for this feature of the table is the point made earlier in discussing different income totals: the double counting of indirect business taxes in N. These give U^* and A^*

[33] First-difference correlations, or correlations using a lagged value to adjust for trend, would be also, but we have not calculated them.

a common element. The taxes (or part of them) are already included in the market value of consumption items entering into U^*; they are also included in A^*, via government expenditure, as AM defines it. The result is to make the correlation between U^* and A^* spuriously high. On the other hand, when T is subtracted from U^* to give U^*-T, an opposite bias may be introduced, since while the common element is properly subtracted out, so is the rest of T, so that errors of measurement, which enter positively into A^* enter negatively into U^*. It is likely therefore that the correlations for the Y income total are biased against this version of the income-expenditure theory; the correlations for the N income total are biased in its favor. Hence a valid estimate of its performance would be in between.

D. *The Use of M^* Instead of M*

We accept as valid the theoretical idea leading AM to substitute M^* for M, namely, that some part of the movement in M may itself be induced. We do not, however, accept their particular expression of the idea. For the period since the end of World War II, M^* and M must be very highly correlated, though we do not have the estimates of M^* with which to check this conjecture. For the period before World War II, M^*, if we understand its construction, is a poor variable on theoretical and empirical grounds. M^* is described as "the estimated maximum amount of money (in the conventional definition) that could be created by the banking system on the basis of the reserves supplied by the money authority (except in response to commercial bank borrowings), account being taken of reserve requirements and currency holding habits," where apparently reserve requirements are interpreted as meaning *legal* reserve requirements but currency holding habits as meaning *actual* habits.[34] This inconsistency is a fatal flaw. The theoretical difficulty it raises can be readily seen by applying the concept to a banking system with no legally required reserves (which, certainly until recent years, has been the situation in most of the world). M^* on this basis is limited only by currency holding habits and completely unaffected by banking behavior; if the ratio of currency to deposits were to approach zero, or banks could issue currency, M^* would approach infinity. For M^* to be meaningful, the reserve requirements for which it is calculated must be the banks' desired (perhaps long-run or secular desired) reserve requirements, not legal reserve requirements. For the period since World War II, and also most years prior to 1929, these did not differ greatly although there were significant changes in the rela-

[34] This interpretation is based on the paper by Teigen referred to by AM. Since it was contained in the first draft of our reply and AM have not indicated that we have misinterpreted them, we assume that it is correct.

tion between the two.[35] But beginning in 1932, increasingly after 1933, and until World War II, desired reserves differed widely from legally required reserves. This was not a difference in any sense "induced" by contemporaneous changes in income. It was a reaction to the failure of a third of the banks from 1929 to 1933, the resulting disillusionment with the Federal Reserve System as a "lender of last resort," and the slow acceptance of the FDIC as an effective substitute. Banks decided that required reserves were no reserves at all, and that they had to provide their own liquidity by keeping reserves in excess of requirements. M^* during this period must be much larger than the actual money stock and is not at all comparable with its values before 1929, from 1929–32, or after World War II. To regard it as the variable relevant to the quantity theory makes about as much sense as to regard the investment variable relevant to the income-expenditure theory as being the maximum investment that could have been made if business had borrowed the funds that banks could have loaned if they had eliminated legal excess reserves. Hence, we regard all of AM's correlations containing M^* as worthless and have made no attempt to repeat them for the separate subperiods.

V. *Comments on AM Appendix*

This Appendix, which is the second to which we have written a reply,[36] represents a step in a highly desirable direction—the formulation of more complex alternative hypotheses of income determination than those we considered and the comparison of their conformity with experience. Unfortunately, it is as yet an unsuccessful experiment because the models formulated, while complex, are unsatisfactory.

The Appendix falls into two rather distinct parts: (1) the first two-thirds which presents the models and a qualitative analysis of them; (2) the final third, which attempts a quantitative analysis and comparison.

1. *The first two-thirds.* This part of the Appendix (through the discussion of Table A.1) strikes us as a rather extended exegesis of a few sentences in our original paper, namely:

> In practice, M and A [money and autonomous expenditures] are positively correlated and that is to be expected under either of the theories under consideration. Under the quantity theory, changes in M involve changes in all components of income, including both consumption and other components. Under the income-expenditure theory, changes in M influence rates of interest and thereby affect investment

[35] See Phillip Cagan's forthcoming NBER monograph, *Determinants and Effects of Changes in the Stock of Money.*

[36] We mention this for the benefit of those readers who may have seen AM's original manuscript which was widely circulated in mimeographed form. Their present Appendix is almost entirely new.

> and consumption; in addition, changes in investment may induce corresponding changes in M via their effect on banks. Hence, a positive simple correlation between A and C may simply be a disguised reflection of the effect of M on C; alternatively, a positive simple correlation between M and C may simply be a disguised reflection of the effect of A on C (p. 177).

Hence we agree with the general conclusion of this part, that it is generally not possible to discriminate among the kinds of hypotheses in question simply be examining the statistical significance of individual correlations, or the signs and statistical significance of separate regression coefficients. That is why we instead *compared* relations computed in as similar a way as possible and for as many subperiods as possible.

Our only disagreement with this part is that AM do not carry their analysis to its logical conclusion. They assert that the EDO (effective demand only) and MO (money only) models are contradicted: the EDO model because the coefficient of M^* in their equation (4.4a) is not zero; the MO model because the quantity of money demanded and the quantity supplied have been shown elsewhere to be affected by interest rates. Both results do contradict the specific models in question as they formulate them. But at least the assertion about the MO model simply reflects a defect in the way AM formulate it. They are required to rule out interest-rate effects in their MO model because they have left out the price level. Restore the price level to the model, as it should be restored, and it is entirely consistent with an MO model that both the *nominal* quantity of money supplied and the *real* quantity demanded should be responsive to interest rates. Indeed, we doubt that there has ever been a reputable quantity theorist who did not believe that both these effects were present, at least in the short run. Such a theorist would have gone on to deny, and correctly, that this implied that interest rates were, in the long run, determined by monetary factors. It is this proposition, very different from the one considered by AM, that distinguishes the quantity theorist from the income-expenditure theorist—or, perhaps better, the liquidity-preference theorist.

2. *The final third.* The rest of the Appendix is a different matter. Here AM quite properly go beyond the coefficients of each equation separately and, in effect, compare different equations. Here they try to find quantitative comparisons comparable to those that we used that can discriminate among the different theories but that are based on more complex and general models than those we used. This is an admirable direction in which to proceed. However, the reliability of the results depends critically on how satisfactory the more complex models are. Unfortunately, the particular models AM use seem to us so unsatis-

factory as to render their numerical results of no interest for the purpose for which they are intended.

The chief defects in their models seem to us to be:

(a) *The omission of the price level.* We noted in our paper: "Both sets of equations [quantity theory and income-expenditure] have a major gap, and incidentally, the same gap. . . . Neither . . . says anything about the proportion in which changes in income will be divided between output and prices." Yet this division "may have a significant effect on the numerical values of the multiplier and of the velocity and on their stability" (pp. 171–72). If the simple models are to be elaborated, the introduction of a price level seems like the first and most important elaboration that is required. (Some of our calculations are among real magnitudes but we did not satisfactorily integrate our equations involving real and nominal magnitudes.)

(b) *The nonhomogeneity of the equations.* As AM correctly note in their footnote 31, "In order for the subsequent developments to be strictly valid, the functions c, f, L, and β must be homogeneous of degree one in the flow variables and the stock of money." This is related to the preceding point, since, on a theoretical level, one way to justify leaving out the price level is to assume that the relevant elasticities are unity with respect to both prices and real magnitudes and hence that the response can be calculated from nominal magnitudes alone. Yet the equations (B.1) to (B.4) that AM use as their approximation to a more complex and general system are not homogeneous because of the presence of interest-rate terms. According to that system, doubling all flow variables and the stock of money would imply doubling the interest rate! And this is so regardless of whether the change consists of a change in prices alone or in real magnitudes alone.

(c) *The use of M^*.* M^* plays a critical role in the equations and the tests. Yet for reasons outlined in our text, M^*, as AM have apparently defined it, is so logically defective and so empirically misleading a variable that it is hard to attach much meaning to results in which it plays a critical role.

(d) *The use of C^f.* C^f also plays a critical role in the equations. Yet this is a strange construct that AM were led to only by a misunderstanding on their part of a suggestion one of us made to them in connection with some of their earlier work. It has no clear economic significance in the context of comparing different models such as U^* or U has.

VI. *Conclusion*

What is the net outcome of this interchange? In one respect, it is rather surprising. A reader of modern economic textbooks would con-

clude from them that the Keynesian Revolution produced a carefully formulated, logically coherent theory of income determination that had the great virtue of using concepts that have direct and readily available empirical counterparts. He might also conclude, though more from the tone than from any explicit statements, that the theory had been tested by evidence and had passed the tests with flying colors.

The reader of this interchange will come out with a very different impression. The central concept of the income-expenditure theory—autonomous expenditures that are the driving force of income—apparently has more symbolic than substantive content. All of us use words to describe it—like "independent," "uncorrelated with the residual error," "exogenous"—that are figurative rather than operational. We proposed an operational criterion for choosing its empirical counterpart that we regard as far from satisfactory and that our critics apparently regard as beneath mention. They rely on what is "beyond reasonable doubt" or "plausible." One of our critics states flatly that: "Theory or 'intuition' is necessary to specify components of autonomous expenditure,"[37] without, of course, specifying what the content of the one theory is that is to give content to the other theory, or how different "intuitions" are to be reconciled.

Even in the absence of an unambiguous criterion, there might be a large measure of agreement about what is the most useful empirical counterpart for autonomous expenditures, as there is about the counterpart for money. However, that is clearly not the case, in view of the richness of the alternatives that have been served up.

In our paper, we may have settled on a concept that was particularly unfavorable to the income-expenditure approach and as a result may have been led to too sweeping a conclusion about its lack of conformity with experience. Certainly, the fishing expedition conducted by our critics has turned up some constructs that are highly correlated with some components of income for some periods. However, our construct at least had the merit that it was derived by a specifiable and reproducible procedure, and that it was fitted into a system of constructs that accounted for the whole of the income total we regarded the income-expenditure theory as trying to explain. The alternative constructs have neither merit. They all leave loose ends of income dangling. They all treat a high fraction of income as autonomous and so limit sharply the scope of the income-expenditure theory. What the calculations of our critics do is to establish a presumption that further research along similar lines may be more rewarding than we thought was likely on the basis of our earlier work.

It is worth recording that our critics have all accepted the monetary

[37] Hester, *op. cit.*, p. 377.

relations we presented and have not experimented with any wide range of alternative concepts of money. All remark on the closeness of the monetary relations, and with one possible exception (Hester), do not question that monetary changes play an independent role that is more than the disguised reflection of changes in autonomous expenditures. Ando and Modigliani have gone farther and experimented with a different monetary concept, M^*, designed to separate out the autonomous monetary element in income change. We agree that this is a desirable direction in which to move, but question the validity of their particular formulation.

The task we set ourselves was to compare, in as unambiguous and unbiased a fashion as possible, the relative stability of monetary velocity and the investment multiplier. None of the calculations made by our critics for supposedly the same purpose is correct because they omit some components of income for the income-expenditure calculations, set the two theories different tasks, or use lengthy periods combining two different subperiods. We have made some of the correct calculations for one of the alternative concepts of autonomous expenditures (Ando and Modigliani's). Though less clear-cut, the results are in the same direction as those from our original calculations. Hence, we are left with no reason to change our earlier conclusion that "so far as these data go [and, we may now add, those adduced by Ando and Modigliani, DePrano and Mayer, and Hester] the widespread belief that the investment multiplier is stabler than the monetary velocity is an invalid generalization from the experience of three or four years. It holds for neither later nor earlier years" (p. 166).

REJOINDER

By A. Ando and F. Modigliani

We share the feeling of Professors Friedman and Meiselman that this exchange has had some merit in clarifying the meaning of their original paper, and in bringing all of us closer to agreement on two conclusions: (1) that the single-equation tests under discussion cannot be expected to throw much useful light on the relative importance of money supply and of autonomous components of demand and fiscal policy variables in the determination of the levels of output and employment; and (2) that insofar as the outcome of these tests indicate anything, they do not warrant the conclusion that either the relation between expenditure and money or that between expenditure and the above autonomous variables is "consistently and decidedly" more stable than the other.

We cannot, however, accept what seems to be FM's main criticism of our procedure for testing the income-expenditure model, namely, that we are wrong in using C^f as the induced component and Z^a and X^a as autonomous variables, and that we "leave loose ends of income dangling" [3, Sec. V, paragraph 4]. The specification of our model was chosen to fit, as closely as possible, the standard Keynesian formulation. We have accordingly regarded aggregate output (as measured by N) as the variable to be predicted, and Y^d (not N) as the income total inducing C, the major part of the induced component of output. The reader can verify that the resulting model corresponds to the one given by FM under the heading "A Slightly More Complex Model" [3, Sec. III.B], by noting the following identities relating their symbols to ours:

$$U \equiv C + Z^i \equiv C^f; \qquad Y' \equiv Y^d; \qquad A_1 \equiv Z^a + X^a + X^i;$$
$$A_2 \equiv -(X^a + X^i);$$
$$A \equiv Z^a; \qquad Y \equiv U + A \equiv (C + Z^i) + Z^a \equiv N.$$

The induced part of the income total to be predicted ($Y \equiv N$) is thus $C+Z^i$ which is our C^f, and not $C^f - X^a$, as FM maintain in their reply [3, Sec. IV.C]. X^a, like M, is *not* a part of the income total to be predicted. Furthermore, it is apparent that in our formulation both A_1 and A_2 are partly induced and partly autonomous. Hence, as FM correctly hint in their discussion of their more complex model, the solution for the induced component ($U \equiv C^f$) is a linear function of Z^a and X^a (and not of $Z^a + X^a$, except under a set of very special assumptions, as we spelled out in our paper).[1]

As for the issue of which variables should be treated as autonomous, we

[1] As indicated in our paper and noted in FM's reply, the use of C^f was first suggested to us in a letter by Friedman containing the following paragraph: "The crucial point is that what must be estimated is $C+Z^i$. This is the part which is to be obtained from the multiplier process."

have given in our paper a specific definition of an autonomous variable, namely, that the variable should be uncorrelated with the error term of the test equation. The method for testing the specification that a set of variables is autonomous in the above sense is well known to those familiar with the econometric literature, and requires that the system of equations under consideration be identified even in the absence of the specifications to be tested. To be sure, we have not carried out such a test, since it would have required the specification of a complete system and a set of tests far beyond the scope of our paper. On the other hand, FM have equally failed to carry out such a test of their specification. As far as we can see, their procedure using simple correlations has no discernible relation to the proper test described above, and is no less heuristic than our appeal to common sense and informal observation, although it appears so much more formidable and objective. Under these conditions, the reader must decide whose specifications are more likely to be roughly correct, and here we put the issue simply: On which of the following two propositions would he rather bet? (1) That the aggregate deficit of federal, state, and local governments over the course of any year is roughly autonomous and total expenditure of these governments is mostly induced by income, as FM specify; or (2) that expenditure is largely autonomous while most taxes, and hence the deficit, are largely induced, as we assume?

The main remaining criticism of FM is that we have presented results for the entire period 1929–58, excluding the war years, rather than for the two periods, 1929–41 and 1947–58, separately. The reason we did not regard it worthwhile to present separate results for the two subperiods chosen by FM is that, as indicated in our comment [1, footnote 8], this separation—in contrast to the treatment of the war years—does *not* change any of the relevant results materially. In this connection, we invite the reader to inspect the results reported in FM's reply, Table 2 (which is the more nearly relevant test), or even those of Table 4 (which are seriously biased against the income-expenditure model)[2] and decide whether they are more nearly in conformity with FM's claim that "the results are strikingly one-sided," [2, p. 166] and that "the income velocity of circulation of money is consistently and decidedly stabler than the investment multiplier except only during the early years of the Great Depression after 1929" [2, p. 186]; or with our conclusion that "On the basis of these results, it is nearly impossible to answer the question posed by FM as to which of the two models of income determination . . . should win the prize for the highest correlation" [1, paragraph 4].

However, even the results given in Tables 2 and 4 are not really the relevant ones. For reasons that we have explained in detail in our paper [1, Sec. I, discussion of Table 3.b], the relevant comparison is in terms of the error variance with C' as the dependent variable in the case of the income-expenditure model and with N or C' as the dependent variable in

[2] For one thing, X^a is included in U^* with *negative* sign and in A^* with positive sign, thus reducing the positive correlation between relevant parts of U^* and A^*.

the case of the money model (see Table R).[3] From the figures reported in Table R below, the reader can satisfy himself that the results for the two subperiods are very similar to those for the period as a whole; if anything, with the separation into two subperiods they are slightly more favorable to the income expenditure model.[4]

Two brief comments are in order concerning the test of the money model. (1) FM complain that our use of M^* in place of M^f is absurd for the period 1929 through 1941 because banks' desired reserves differed widely from required reserves. While it is true that excess reserves grew quite large after 1933, the evidence suggests that their behavior was broadly consistent with the very low prevailing level of short term yields. To plead that M^* should be adjusted for desired excess reserves is much the same as to propose that in testing the stability of the relation between income and money, the money supply should first be adjusted for changes in velocity of circulation! It may be relevant to add that the very significant association between N and M^* (given C_{t-1}) reported in Table 4 is due almost entirely to the period 1929–41, for which FM regard M^* as an unacceptable measure (partial correlation .79); for the "acceptable" period 1947–58, the association is entirely insignificant (.22).[5] (2) We regret that FM did not see fit to comment on our interpretation of Friedman's theory of the demand for money and on the failure of the empirical test to support its implications. Such comments would have helped in clarifying the mechanism through which they

[3] Instead of the error variance one might use the ratio of the standard deviation of the residual error to the mean of the dependent variable, as suggested in FM's reply. Accordingly, this measure is also provided in Table R.

[4] It should also be noted that the results using the entire period, which incorporate the relation between the positions of the means for the two subperiods (relative to the within-period regression estimates) are by no means to be disregarded, as they provide highly relevant information on one of the two issues which FM label "central," namely, "which set of relations (the multiplier or velocity) is . . . stable in the sense of expressing empirically consistent relations which can be depended upon to remain the same from time to time" [2, p. 169]. The results reported in Table 1 of the reply by FM imply that, of the two relations tested, the income-expenditure one is relatively more stable as between the first and second period, and that the velocity relation with the parameters estimated using the first period would provide a very poor explanation of what happened in the second. This implication can be explicitly verified by comparing the regression equations underlying Table 1 of FM's reply.

	Income-Expenditure Model	Money Model
1929–41	$C_t = 46 + 1.24(Z^a + X^a)$	$C_t = 13 + 1.17M^f$
	(.17)	(.16)
19?/–58	$C_t = 61 + 1.44(Z^a + X^a)$	$C_t = -150 + 2.28M^f$
	(.10)	(.11)

While we have not performed the formal analysis of covariance, it is fairly clear that the income-expenditure model will pass the test of homogeneity while the money model would not. In view of the above results, we find it hard to see how FM can state that "there was a sizable shift of regression of C on M from before to after the war. *The slope was not much altered* but the position was" [3, Sec. I, paragraph 9 (italics added)].

[5] This same conclusion holds for the association of M^* with C'. For the first subperiod the partial correlation is .76, for the second, −.03!

TABLE R—EFFECTS OF SEPARATING THE PERIOD 1929–1958 INTO TWO SUBPERIODS

Period	Dependent Variable		Independent Variables			
			$M^{\prime}, C_{t-1}$	M^{*}, C_{t-1}	$(Z^{a}+X^{a}), C_{t-1}$	Z^{a}, X^{a}, C_{t-1}
1929 to 1941	$C^{\prime}$	$R=$	.948	.898	.977	.977
		$S_e^2=$	16	31	7	8
		$\frac{S_e}{C^{\prime}}=$	6.6	9.2	4.4	4.7
	N	$R=$	.959	.896		
		$S_e^2=$	35	86		
		$\frac{S_e}{\bar{N}}=$	7.7	12.1		
1947 to 1958	$C^{\prime}$	$R=$	.984	.983	.986	.990
		$S_e^2=$	57	61	50	41
		$\frac{S_e}{\bar{C}^{\prime}}=$	3.5	3.7	3.3	3.0
	N	$R=$	.982	.981		
		$S_e^2=$	188	189		
		$\frac{S_e}{\bar{N}}=$	4.3	4.3		
1929 to 1941 and 1947 to 1958	$C^{\prime}$	$R=$	.997	.997	.997	.998
		$S_e^2=$	41	42	43	29
		$\frac{S_e}{\bar{C}^{\prime}}=$	4.8	4.8	4.9	4.0
	N	$R=$	.996	.997		
		$S_e^2=$	147	118		
		$\frac{S_e}{\bar{N}}=$	6.3	5.6		

see money determining income. If they are prepared to accept our interpretation given in Section II of our paper as representing their theory, then we must take strong exception to their statement at the beginning of their reply, that we "agree with at least the broad outline of our analysis of the channels through which the changes in the quantity of money exert their influence." [3, paragraph 1].

Finally, we wish to add a short rejoinder to FM's comment on our Appendix. As we clearly indicate, the purpose of this appendix is not that of competing with the voluminous and rapidly growing body of econometric literature devoted to estimating and testing macroeconomic models. It is only intended to clarify why the single-equation tests proposed by FM cannot be used as a basis for accepting or rejecting the Keynesian income expenditure framework or for assessing the importance of money. We believe that our rudimentary system is quite adequate for that limited purpose. FM are right in pointing out that our linearized system B is not homogeneous. But this system must be regarded as a Taylor expansion to first-order terms around the equilibrium point of system A which *is* homogeneous. Such a linear approximation is the standard tool in the type of analysis with which we are concerned, where the direction rather than the actual magnitude of the effects is the issue. While recognizing that linear approximations have shortcomings, we do not feel that these shortcomings are such as to render our results meaningless. But if one were to agree that even the more complex model of the Appendix is too crude to throw light on the relative merits of the income expenditure theory as against the simple quantity theory, then surely the results of FM using an even cruder framework cannot be considered more useful.

References

1. Albert Ando and Franco Modigliani, "The Relative Stability of Monetary Velocity and the Investment Multiplier," *Am. Econ. Rev.*, this issue.
2. Milton Friedman and David Meiselman, "The Relative Stability of Monetary Velocity and the Investment Multiplier in the United States, 1897–1958," in *Stabilization Policies*, Commission on Money and Credit, Englewood Cliffs, N. J., 1963.
3. ——— and ———, "Reply to Ando and Modigliani and to DePrano and Mayer," *Am. Econ. Rev.*, this issue.

REJOINDER

By Michael DePrano and Thomas Mayer

To save space we are only answering briefly the main points of FM's reply here; a duplicated Appendix with detailed answers is available upon request.

We were surprised by many of the points in FM's reply, since we thought that they had been settled by correspondence. This is especially so with respect to the question of whether or not the tests must predict all of the endogenous part of income. We admitted that it would be useful to test the relative performance of money and "A" in predicting inventory, imports, and so on. But each of these are separate reduced-form equations. What FM wish to do really adds together all the reduced-form equations for the induced components of income. Why do this?

If the basic purpose of the research had been to predict income, then neither our approach nor the FM model would have been adequate. A user of FM's model first has to determine government expenditures and taxes, exports and imports, private domestic investment, and so on. He then finds consumption as the induced variable. To use our model he does not need to know imports, taxes, or inventory to find consumption. However, he still has to find them (by separate equations) to determine total income. FM overlook all this because they apparently forget that the government deficit is the result of two variables: government expenditures and taxation, and that net exports are also the result of two variables. An excellent example of this confusion is their claim that they are able to predict total income using a smaller proportion of income than we do. This is illustrated in their Table 3. It is, of course, an illusion.[1]

Our basic difference with FM can be summarized in their accusation at the end of their second paragraph, "DM, after running 'basic tests . . . on 20 different, but not unreasonable, definitions of autonomous expenditures' settle on two but carry two others along for the ride." The point is that we "settled" on our two definitions *before*, not after, running the tests. Rather than letting the correlation coefficients determine the definitions, we settled on our formulations from the start. They were not the ones with the highest correlation, as Appendix Table A shows.

Ironically, the grounds for our a priori determination of autonomous expenditures are not crucial. We tried to explain them to some extent, and thought that they were obvious to some extent: the government determines the *amount* of government spending, but the *rate* of taxation; exports

[1] Two of their components—imports and taxes—are negative items in their autonomous term. Actually, they must have a knowledge of a greater dollar amount of the various components, but the sum is smaller because they subtract taxes from government expenditures and imports from exports. At times one obtains the impression from FM that the way to find government spending is to determine the government deficit, determine total taxes, and add them together. It never fails.

are determined abroad. (Are these observations "casual empiricism"?)Our selection could have been wrong. Whatever the reasons for our selection, it was up to the tests to determine which hypothesis—including the rules for measuring the variables—was correct. This is exactly what FM ask for when they inquire how well "alternative theories correspond to our experience when the central concepts . . . are defined . . . by earlier work. . . . "

FM continually insist that we do not state what is wrong with their criteria for choosing "A" (until they are ready to attack our criticisms in Part III.C).[2] Their procedure leads them to a definition of "A" whose performance is inferior to almost any other definition. Granted that one cannot determine the correct definition by the size of the correlation coefficient alone, surely a good definition should not give one of the lowest coefficients. Still, contrary to FM's assertion, we did point out our displeasure with their criteria in the article and in correspondence. It is true that we focus our attention on their use of the same information to test terms and to test the hypothesis. We do not discuss the procedure at length because we did not really take it seriously, and we are surprised that FM take it as seriously as they now appear. In their selection of an autonomous expenditures measure, their procedure really settled little. Usually they obtained results which were "inconsistent and ambiguous" or "somewhat confusing" and then fell back on vague (and incorrect) references to the usual treatment in the literature. Now they claim this is a "specifiable and reproducible procedure." Even if it had given unambiguous results, the procedure depends on all components except the one in question having been already settled. Upon what grounds would they have been settled? How can components which are themselves in doubt be used to test other components?[3]

FM attempt to play down the degree to which our work changes their results. This is misleading as the reader can easily see by comparing our results with the sweeping claims which label Keynesian economics "mythology."[4]

[2] We tried to make it clear to FM that we do not accuse them of using the highest correlation coefficient to choose their terms. Still, they insist on claiming criticisms for which they have ready answers. It would be more useful if they had answers for criticisms which we do raise, such as the amazingly poor performance of their formulation of autonomous expenditures as compared to almost any other reasonable formulation.

[3] Note also that if $N=N(D, A, u)$ and if the coefficients of D and of A are very different, the correlation of the sum of D and A with N can be lower than the correlation of either one individually with N.

[4] Compare, for example, the correlation coefficients in the top line with those in the bottom three lines of our Table 3. To FM our results are "generally somewhat higher" and "a bit higher." They claim we "diminish the superiority of money." What tables did they look at? Tables 2 and 4 also show the superiority of money not only diminished, but eliminated. These appear to us to "drastically change" FM's conclusions.

Too, FM may be misled on the areas of agreement among us. Our acceptance of their definition of money was simply because we desired to concentrate on what we considered the most significant weakness in their study. Our conclusion—that both money and autonomous expenditures have strong influence—could be taken as partial agreement with FM, or simply as agreement with Keynes, for whom money played a substantial role.

SECOND ESSAY ON THE GOLDEN RULE OF ACCUMULATION

By EDMUND S. PHELPS*

Four years ago, I presented a theorem on maximal consumption in a golden age [7]. The same theorem was discovered and published by Allais [1], Desrousseaux [3], Mrs. Robinson [10], Swan [15], and von Weizsäcker [17].[1] The theorem established may be expressed as follows:

> If there exists a golden-age growth path[2] on which the social net rate of return to investment equals the rate of growth (hence, in one class of models, the fraction of output saved equals the capital elasticity of output)—or, in market terms, a golden-age path on which the competitive interest rate equals the growth rate and hence gross investment equals the gross competitive earnings of capital—then this golden age produces a path of consumption which is uniformly higher than the consumption path associated with any other golden age.

The consumption-maximizing golden age will be referred to in this paper, as in [7], as the Golden Rule or GR path.

The papers cited raise two sorts of questions. The first concerns the conditions for the existence of the GR path. Some of the papers (including my own) erroneously suggest that the GR path can exist only in "neoclassical" models, i.e., models in which capital and labor are continuously substitutable. Some of the papers leave the false impression that the GR path exists only if there is no technical progress, while my own paper errs with respect to the type of technical progress which permits a GR path. The first part of this paper examines in two kinds of models the conditions for the existence of the GR path. We show, as a few writers have indicated, that the GR path may exist in the un-

* The author is associate professor of economics at Yale University. He owes a great debt to Tjalling C. Koopmans who contributed the basis for the theorems established in the second part of this paper. David Cass, Peter A. Diamond, Paul A. Samuelson, and Robert M. Solow made useful comments on an earlier draft. The author alone is responsible for any errors in the final product.

[1] Mention should also be made of an unpublished paper by Beckmann [2] in which the theorem is proved for the Cobb-Douglas case and the dissertation of Srinivasan [14] in which the existence of a state of maximum per capita consumption with a growing labor force is shown. All these authors made the finding independently, circa 1960.

[2] By a golden-age path we mean a growth path in which literally every variable changes (if at all) at a constant relative rate. It follows immediately that if investment is positive then output, investment, and consumption must all grow at the same (constant) rate. Various other properties can be derived.

neoclassical Harrod-Domar model as well as in the neoclassical model. And we show that a positive-investment GR path can exist only if technical progress can be described as purely "labor-augmenting."

Question also arises as to the normative significance of the theorem. We called the saving rule prevailing on the consumption-maximizing golden-age path the Golden Rule of Accumulation because, on that path, each "generation" saves (on behalf of future generations as it were) that fraction of income which it would have past generations save, subject to the constraint that all generations past and present are to save the same fraction of income. But no proof of the "optimality" of the GR path was given nor was any suggestion of its optimality seriously intended. Society need not confine itself to golden-age paths (should they exist) nor aim to achieve golden-age growth asymptotically. And even if some golden-age path should be utility-maximizing (at least for some initial conditions) the rate of time preference may make that path different from the GR path. It was evidently reflections such as these which led Pearce [6] and Samuelson [11] to doubt whether the GR path has any important normative significance at all.

In the second part of this paper it will be shown, however, that, whether or not it is "optimal," the GR path has the following important normative property: Any growth path on which, at some point in time and forever after, the capital-output ratio always exceeds its GR level by at least some constant amount—equivalently, any path which eventually keeps the social net rate of return to investment (or competitive rate of interest) permanently below its GR value by at least some finite amount—is *dynamically inefficient* in the sense that there always exists another path which, starting from the same initial capital stock, produces more consumption at least some of the time and never less consumption. This is the proposition conjectured by the author in reply to Pearce [8]. Its proof here is based on a proof provided by Tjalling C. Koopmans. The significance of the theorem is this: no path which is dynamically inefficient can be optimal; hence no path which transgresses the GR path in the manner described can be optimal. (Warning: It is only paths which so transgress the GR path for *infinitely long time* that can be shown to be dynamically inefficient.)

Since the conditions for a GR path are stringent, this theorem is only of theoretical interest. But we are able to prove analogous theorems even when no golden-age path, and hence no GR path, need exist. Thus we show that the possibility of "excessive capital deepening," despite a continuously positive rate of interest, is quite general.

A fuller summary of the paper and some concluding remarks close the paper.

I. *Existence of the Golden-Rule Path*

Section A will study the neoclassical and the Harrod-Domar models on the postulate that technical progress can be described as solely labor-augmenting. Section B will show that a GR path can exist only if technical progress can be described as labor-augmenting.

A. *Labor-Augmenting Technical Progress*

In both the neoclassical and Harrod-Domar cases, output, $Q(t)$, is a continuous function of capital, $K(t)$, labor, $L(t)$, and time:

$$Q(t) = F[K(t), e^{\lambda t}L(t)], \qquad \lambda \geq 0. \tag{1}$$

It is assumed here that technical progress can be described as solely labor-augmenting—time enters only in the second (labor) argument of the function—and that labor augmentation occurs at the constant exponential rate λ. The function is supposed to be homogeneous of degree one (constant returns to scale).

We suppose that the labor force grows exponentially at rate γ:

$$L(t) = L_0 e^{\gamma t}, \qquad \gamma \geq 0. \tag{2}$$

Capital is taken to be subject to exponential decay at rate δ, so that if $I(t)$ denotes the rate of gross investment:

$$I(t) = \dot{K}(t) + \delta K(t), \qquad \delta \geq 0. \tag{3}$$

Finally, consumption, $C(t)$, is the difference between output and gross investment:

$$C(t) = Q(t) - I(t), \qquad C(t) \geq 0. \tag{4}$$

The neoclassical case. We suppose now that the production function has the following "neoclassical" properties: it is twice differentiable (smooth marginal products), it is strictly concave (diminishing marginal products), and it has everywhere positive first derivatives (marginal products). That is,

$$\frac{\partial F}{\partial K} > 0, \qquad \frac{\partial F}{\partial L} > 0; \qquad \frac{\partial^2 F}{\partial K^2} < 0, \qquad \frac{\partial^2 F}{\partial L^2} < 0. \tag{1a}$$

By virtue of constant returns to scale and (2):

$$Q(t) = L_0 e^{(\gamma+\lambda)t} F\left[\frac{K(t)}{L_0 e^{(\gamma+\lambda)t}}, 1\right]. \tag{5}$$

Hence, if we let $k(t)$ denote capital per unit "effective labor,"

$$k(t) = \frac{K(t)}{L_0 e^{(\gamma+\lambda)t}}, \tag{6}$$

and if we define

$$f(k(t)) = F[k(t), 1], \tag{7}$$

we can express the production function for all t as

$$Q(t) = L_0 e^{(\gamma+\lambda)t} f(k(t)), \qquad f'(k(t)) > 0, \qquad f''(k(t)) < 0. \tag{8}$$

We show now that if $k(t)$ is equal to any positive constant $k > 0$, then the economy will grow in the manner of a golden age, provided of course that the constraint $I(t) \leq Q(t)$ is satisfied.

Clearly, output will grow exponentially at rate $g = \gamma + \lambda$,

$$Q(t) = L_0 e^{(\gamma+\lambda)t} f(k) = Q(0) e^{gt}, \tag{9}$$

as will the capital stock:

$$K(t) = L_0 e^{(\gamma+\lambda)t} k = K(0) e^{gt}. \tag{10}$$

Hence, from (3) and the relation $\dot{K}(t) = gK(t)$, investment will also grow at the rate g:

$$I(t) = (g + \delta) K(0) e^{gt} = (g + \delta) L_0 k e^{gt}. \tag{11}$$

Since investment and output will grow at the same rate, g, so will consumption, $C(t)$, (where $C(t) = Q(t) - I(t)$)

$$C(t) = [Q(0) - (g + \delta) K(0)] e^{gt} = [f(k) - (g + \delta) k] L_0 e^{gt}. \tag{12}$$

The gross investment-output ratio, s, will be constant:

$$s = \frac{I(t)}{Q(t)} = \frac{(g + \delta) K(0)}{Q(0)} = \frac{(g + \delta) k}{f(k)}. \tag{13}$$

So will the marginal productivity of capital,

$$\frac{\partial F(K(t), e^{\lambda t} L(t))}{\partial K}:$$

$$\frac{\partial F}{\partial K} = \frac{\partial F\left(\frac{K(t)}{L_0 e^{(\gamma+\lambda)t}}, 1\right)}{\partial \left(\frac{K(t)}{L_0 e^{(\gamma+\lambda)t}}\right)} = f'(k). \tag{14}$$

And so will the share of gross output going to capital, a, if capital

receives its marginal product:

(15) $$a = \frac{\partial F}{\partial K} \frac{K(t)}{Q(t)} = \frac{f'(k)k}{f(k)}.$$

Conversely, it can be shown that every golden-age path in which investment is positive implies a constant value of $k(t) > 0$ and a growth rate equal to $\gamma + \lambda$.[3] Therefore, a golden age with positive investment occurs if and only if $k(t) = k$, a constant.

Hence, in every golden age with positive investment, the growth rate of output, investment, and consumption is $\gamma + \lambda$. These golden-age consumption paths are therefore logarithmically parallel. Associated with each golden age is a certain value of s, of $\partial F/\partial K$, of $K(0)$ and of k. Let us assume for the moment (we drop this assumption later) that the golden age yielding the maximal consumption path, if such exists, is one in which k, and hence $K(0)$, is greater than zero. We assume, in other words, that if a maximum exists, it is an *interior* one rather than a corner maximum at $k = 0$. Then, for every t, the derivative of $C(t)$ with respect to $K(0)$ in (12) must be zero on the GR path:

(16) $$\frac{\partial C(t)}{\partial K(0)} = \frac{\partial F}{\partial K} - (g + \delta) = 0.$$

Equivalently, one can differentiate (12) with respect to k to obtain:

(16a) $$f'(k) - (g + \delta) = 0.$$

That is, on this assumption, the marginal product of capital will equal $g + \delta$ on the GR path (if it exists).[4] Transposing terms in (16), we have:

(17) $$\frac{\partial F}{\partial K} - \delta = g.$$

The left-hand side of (17) is the social net rate of return to investment.[5] Hence this result states that if an *interior* golden-age consumption maxi-

[3] In a golden age, if investment is positive, then investment, consumption, and output must all grow at the same constant relative rate, denoted g. Hence $Q(t) = Q(0)e^{gt}$ and $I(t) = I(0)e^{gt}$. And capital must grow at some constant relative rate, denoted h. Hence $\dot{K}(t) = hK(t)$. Therefore, by (3), $I(t) = (h+\delta)K(t)$ which implies $h = g$. But if $K(t) = K(0)e^{gt}$ then, from (1) and the postulate that $\partial F/\partial L > 0$, if follows that $g = \gamma + \lambda$, hence that $k(t)$ is constant.

[4] A common-sense explanation of (16) has been provided by Solow [13]. Imagine that capital is initially free but that we are to invest so as to maintain a golden age once the initial capital stock has been chosen. Consider a small increase of initial capital, $\Delta K(0)$. The rules of the game require that we then increase the rate of investment by $\Delta I(0) = (g+\delta)\Delta K(0)$ to make capital grow at rate g. The increase of initial capital will increase output by $\Delta Q(0) = (\partial F/\partial K)\Delta K(0)$. Hence consumption will increase by $\Delta C(0) = \Delta Q(0) - \Delta I(0) = [(\partial F/\partial K) - (g+\delta)]\Delta K(0)$. As long as $(\partial F/\partial K) > g + \delta$ it pays to accept more capital. The consumption-maximizing golden age is reached when $K(0)$ has increased to the point where $(\partial F/\partial K) - (g+\delta) = 0$.

mum exists, it is where the social net rate of return to investment equals the golden-age growth rate. This is the first (and most general) way to characterize the GR path in purely technological terms.

The other technological characterization is obtained by multiplying both sides of (17) by $K(t)/Q(t)$ and rearranging terms:

$$\frac{\partial F}{\partial K}\frac{K(t)}{Q(t)} = (g+\delta)\frac{K(t)}{Q(t)} = \frac{I(t)}{Q(t)}\,. \tag{18}$$

Hence

$$s = \frac{\partial F}{\partial K}\frac{K(t)}{Q(t)}\,. \tag{19}$$

This states that on the interior GR path the saving ratio is equal to the elasticity of output with respect to capital. (This was the characterization of the GR path employed by Swan and the present author; of course, such a capital elasticity exists only in one-commodity models in which output is a function of "capital.")

Conditions (17) and (19) can be translated into "market" terms if the economy is purely competitive and free of externalities in production. On these assumptions, $\partial F/\partial K$ is the gross rental rate of capital and $(\partial F/\partial K)-\delta$ is the (equilibrium) rate of interest. Then (17) implies that on the interior GR path the interest rate is equal to the golden-age growth rate. (19) implies that the saving ratio equals capital's gross relative share, or that net investment equals net profits.

Now we shall investigate the conditions for the existence of the GR path. For this purpose we adapt, in Figure 1, a diagram first presented by Pearce [6] and later employed by Koopmans [5]. It is a diagram of the relation between $K(0)$ and $C(0)$ in a golden age as given by (1) and (12):

$$C(0) = F[K(0), L_0] - (g+\delta)K(0). \tag{20}$$

(Some readers may wish to diagram $c=f(k)-(g+\delta)k$ where c is consumption per unit effective labor force and observe that c is maximal where k is such that $f'(k)=g+\delta$.)

Figure 1 depicts a golden-age consumption maximum at $K(0)=\hat{K}(0)$ where $(\partial F/\partial K)=g+\delta$. It is easy to see from the diagram, however, that there are two cases in which no such interior GR maximum exists.

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[8] By the (instantaneous) social net rate of return to investment at time t we mean

$$\lim_{h\to 0}\left\{\left[\frac{\partial C(t+h)}{\partial C(t)}-1\right]\Big/ h\right\}.$$

For a discussion of the rate of return to investment see Solow [12].

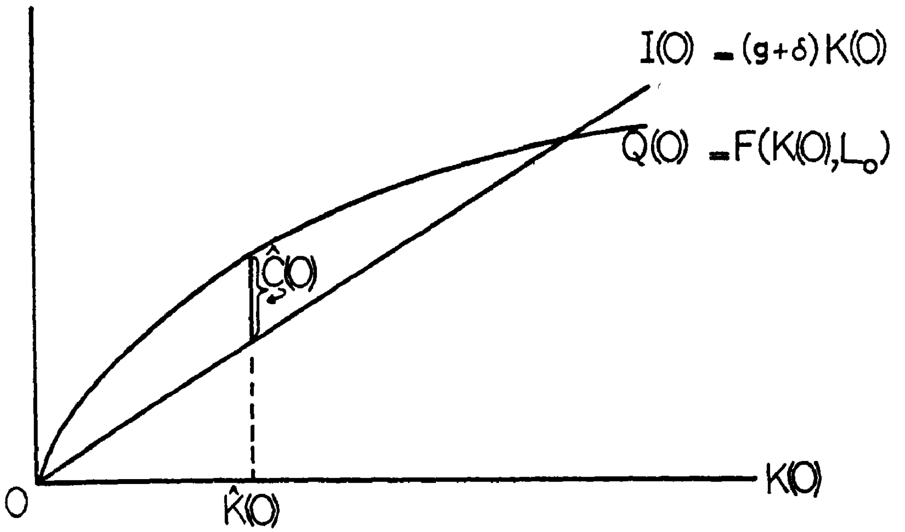


FIGURE 1

In one case, neither an interior nor a corner maximum exists. This is the case in which

$$\lim_{K \to \infty} \frac{\partial F}{\partial K} > g + \delta;$$

then the $Q(0)$ curve is everywhere steeper than the $I(0)$ line so that the distance between them always increases with $K(0)$. It is easy to see that this case implies

$$\lim_{K \to \infty} \frac{Q(0)}{K(0)} \geq g + \delta.$$

While our assumptions on the production function do not exclude this possibility, it can be shown however that, if $g+\delta>0$, this case can arise only if positive output can be produced without labor. Proof: $(Q/K)=F(1, L/K)$. Hence

$$\lim_{K \to \infty} \frac{Q}{K} = F(1, 0).$$

But $F(1, 0)=0$ if $F(K, 0)=0$. Hence

$$\lim_{K \to \infty} \frac{Q}{K} \geq g + \delta > 0$$

only if labor is not required for positive production.

The other case in which no interior maximum exists occurs when

$$\lim_{K\to 0} \frac{\partial F}{\partial K} \leq g + \delta;$$

then the $Q(0)$ curve is everywhere flatter than the $I(0)$ line so that a corner maximum exists at $K(0)=0$. We shall show that, in this case, $K(t)=0$ can be considered the GR path.

There are two sub-cases to consider. Suppose first that $F(0, L_0)>0$. Then

$$\lim_{K\to 0} \frac{\partial F}{\partial K}\cdot\frac{K(t)}{Q(t)} = 0$$

since $Q(t)$ does not go to zero in the limit. Hence, when $K(t)=0$,

$$\frac{\dot{Q}(t)}{Q(t)} = \left[\lim_{K\to 0} \frac{\partial F}{\partial K}\cdot\frac{K(t)}{Q(t)}\right]\frac{\dot{K}(t)}{K(t)} + \left[1 - \lim_{K\to 0} \frac{\partial F}{\partial K}\,\frac{K(t)}{Q(t)}\right](\gamma+\lambda) = \gamma+\lambda.$$

That is, output grows at the usual golden-age rate, or "natural" rate $\gamma+\lambda$. So does consumption. This golden-age path, $C(t)=F(0, L_0)e^{gt}$, is maximal and hence it is the GR path since

$$\lim_{K\to 0} \frac{\partial F}{\partial K} \leq g + \delta;$$

investment would have to increase more than output to maintain a golden age with positive $k(t)$.[6]

The other sub-case is $F(0, L_0)=0$. In this case the $Q(0)$ curve lies uniformly below the $I(0)$ line (since they both start from the origin and $I(0)$ rises more steeply from the start). This implies that no golden age with $K(0)>0$ is possible for it would require $I(t)>Q(t)$. But $K(t)=0$ clearly implies a "golden age" for then $C(t)=Q(t)=I(t)=F(0, L_0e^{(\lambda+\gamma)t})=0$. Since this is the only golden age that exists, it is the maximal golden age and hence the GR path.

Summarizing, if labor is required for positive output and $g+\delta>0$ then a GR path always exists in the model under consideration. If there exists a golden-age capital path $K(t)=K(0)e^{gt}$ such that $(\partial F/\partial K)=g+\delta$ then this is the GR path; if there does not exist such a path then $K(t)=0$ is the GR path. In short, $K(t)=K(0)e^{gt}$ produces the GR path if $(\partial F/\partial K)=g+\delta$ for some $K(0)>0$ or if $(\partial F/\partial K)\leq g+\delta$ when $K(0)=0$.

The Harrod-Domar case. To illustrate the fact that no neoclassical assumptions are required for the existence of the GR path, we now drop the assumptions of twice differentiability, strict concavity and everywhere positive marginal products and specialize (1) to the Harrod-

[6] Note that on this GR path, where $K(t)=0$, the saving ratio and capital's relative share are equal, since they are both equal to zero. But the interest rate may be less than the growth rate.

Domar case:

$$Q(t) = \min\left[\alpha K(t), \beta e^{\lambda t} L(t)\right]. \tag{1b}$$

We retain equations (2), (3), and (4).

By virtue of (2) and the constant returns to scale implied by (lb):

$$Q(t) = L_0 e^{(\gamma+\lambda)t} \min\left[\alpha \frac{K(t)}{L_0 e^{(\gamma+\lambda)t}}, \beta\right] \tag{21}$$

or

$$Q(t) = L_0 e^{(\gamma+\lambda)t} \min\left[\alpha k(t), \beta\right]. \tag{21a}$$

It is easy to show again that if $k(t)$ is equal to any constant $k>0$ then, provided the restraint $I(t) \leq Q(t)$ is satisfied, golden-age growth results. Clearly output, capital and investment will grow at the constant rate $g=\gamma+\lambda$; hence, so will consumption. As before, $s=(g+\delta)K(0)/Q(0)$; if $\alpha K(0) \leq \beta L_0$ (meaning that capital is not in surplus) then $(K(0)/Q(0)) = 1/\alpha$ and if $\alpha K(0) > \beta L_0$ then $(K(0)/Q(0)) = (K(0)/\beta L_0)$. $\partial F/\partial K$ will be constant, either equal to α (if labor is in surplus) or zero (if capital is in surplus).

Conversely, $k(t)$ is constant in every golden age with positive investment. If investment (hence output and consumption) is growing at some constant rate, g, and capital is growing exponentially then capital must also be growing at rate g. Now if g were less than $\gamma+\lambda$, then labor would become redundant (if it was not initially) and the unemployment ratio would grow nonexponentially, which contradicts the notion of a golden age; if g were greater than $\gamma+\lambda$, then labor would eventually become scarce (if it were not initially) and growth of output at the rate g would then be impossible Hence, in a golden age with positive investment, capital grows at the rate $\gamma+\lambda$ and $k(t)$ is therefore constant. Therefore, golden-age growth with positive investment occurs if and only if $k(t)$ is constant.

To investigate the GR path we use Figure 2 which differs from Figure 1 only in that, in (20), we have substituted the Harrod-Domar function $\min\left[\alpha K(0), \beta L_0\right]$ for $F\left[K(0), L_0\right]$:

$$C(0) = \min\left[\alpha K(0), \beta L_0\right] - (g+\delta)K(0). \tag{20'}$$

The diagram depicts an interior golden-age consumption maximum at $K(0)=\beta L_0/\alpha$. At this point the capital stock is just large enough to employ the entire labor force. A larger capital stock would put capital in surplus; a smaller stock would cause a surplus of labor. In the Harrod-Domar model, therefore, the interior GR path, if it exists, is the golden-age path in which there is full employment of both labor and capital.

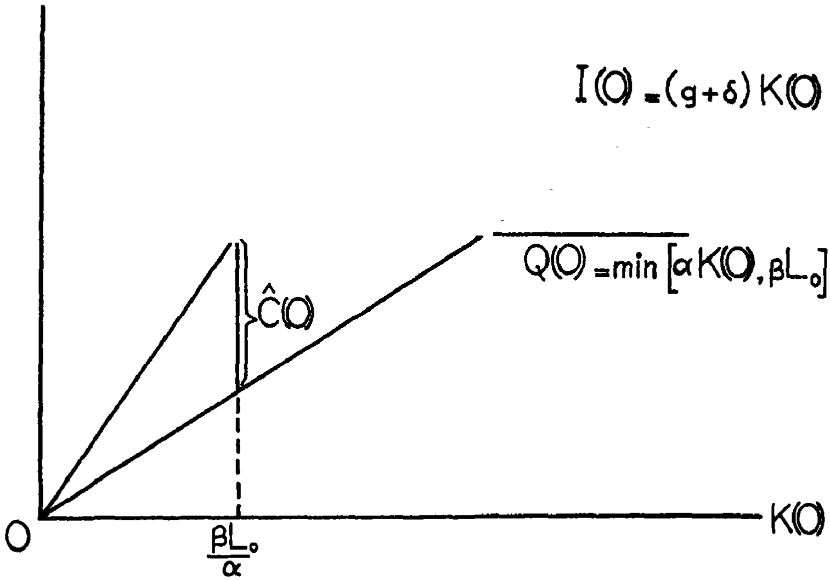


FIGURE 2

What of the usual characterizations of the GR path in terms of the interest rate and capital's relative share? On the interior GR path the saving ratio is $(g+\delta)/\alpha$ and the growth rate $\gamma+\delta$. But relative shares and the rate of interest are indeterminate: we can say only that capital's share is between zero and one and that the interest rate is between zero and $\alpha-\delta$. But it is true that this interior GR path is the only golden-age path with positive investment in which it is possible that the saving ratio equal capital's share and the interest rate equal the growth rate; for in all other positive-investment golden ages capital's relative share and the interest rate are determinate and do not satisfy these equalities. Thus it remains valid that if there exists a golden age in which the interest rate equals the growth rate and the saving ratio equals capital's relative share, then this golden-age path is the GR path. Hence the Golden Rule theorem applies to the Harrod-Domar model as well as to the neoclassical model. (See Robinson [10] and Samuelson [11] for similar comments on more complicated fixed-coefficient models.)

As in the neoclassical case, however, an interior GR path may not exist. Figure 2 shows that if $\alpha<g+\delta$ then no golden age with positive investment exists, hence no interior GR path. In this case the "golden age" $K(t)=Q(t)=I(t)=C(t)=0$ is the only possible golden age; hence it can be regarded as the GR path.

Note that, in the Harrod-Domar case, either an interior or a corner GR path must exist since positive labor input is required for positive output.

B. *The Necessity that Technical Progress Be Labor-Augmenting*

The necessity that any technical progress be describable as labor-augmenting for the existence of a positive-investment (interior) GR path follows from analyses of technical progress by Diamond [4], Robinson [9] and Uzawa [16]. I shall merely indicate one line of proof.

The existence of an interior GR path depends upon the existence of a continuum of logarithmically parallel growth paths on which output, consumption, and investment all grow at some common exponential rate, say g. Since investment grows at rate g and has always been doing so, capital also grows at rate g on each of the paths.

Suppose that the production function is neoclassical (see (1a)) and is homogeneous of degree one:

$$Q(t) = F[K(t), L(t); t]. \tag{22}$$

Differentiating this totally with respect to time and dividing the resulting equation by $Q(t)$ yields

$$\frac{\dot{Q}(t)}{Q(t)} = a(t)\frac{\dot{K}(t)}{K(t)} + [1 - a(t)]\frac{\dot{L}(t)}{L(t)} + \frac{F_t}{Q(t)} \tag{23}$$

where $a(t)$, capital's relative share at t, denotes $(\partial F/\partial K)/(K(t)/Q(t))$, so that, by Euler's theorem, $1-a(t)=(\partial F/\partial L)/(L(t)/Q(t))=$labor's share. $F_t/Q(t)=(\partial F/\partial t)/Q(t)$ is the relative rate of technical progress at time t.

If the aforementioned parallel paths exist, we may substitute g for $\dot{Q}(t)/Q(t)$ and $\dot{K}(t)/K(t)$ and obtain

$$\frac{F_t/Q(t)}{1 - a(t)} = g - \frac{\dot{L}(t)}{L(t)} = \Phi(t). \tag{24}$$

Hence, the rate of technical progress expressed as a ratio to labor's relative share is a function solely of time (independent of the capital-labor ratio) if these parallel paths exist.

Diamond [4] has shown the equivalence of the property expressed in (24) and the Harrod-neutrality, for all K, L, and t, of the technical progress represented by $F[K(t), L(t); t]$. (By definition, progress is Harrod-neutral if and only if relative shares or the capital-output ratio are constant over time for a constant rate of interest or marginal product of capital.)

Now the Robinson-Uzawa theorem [9] [16] proves that if technical progress is everywhere Harrod-neutral then technical progress can be described as purely labor-augmenting:[7]

[7] There are cases in which Harrod-neutral progress can be described as capital-augmenting. The Cobb-Douglas function is such a case (and the only case under constant returns to scale) for the function $K^{\alpha}[A(t)L]^{1-\alpha}$ can be written $[B(t)K]^{\alpha}L^{1-\alpha}$.

$$Q(t) = F[K(t), L(t); t] = G[K(t), A(t)L(t)]. \tag{25}$$

All this proves that, if an interior GR path exists, any technical progress present must be describable as labor-augmenting.

Note that if $Q(t)$ and $K(t)$ both grow exponentially at rate g then, by constant returns to scale, $A(t)L(t)$ or "effective labor" must also grow exponentially at rate g. (It is not essential that $A(t)$ and $L(t)$ each grow exponentially.)

Labor augmentation is, of course, a very restrictive type of technical progress. But the notion of the Golden Rule path has considerable heuristic value even if progress cannot be described as labor-augmenting or even as "factor-augmenting" in general. It will be shown in the next part of this paper that there still exists in these cases a critical path—which we call the Quasi-Golden-Rule path—having, in one respect, the same normative significance as the GR path.

II. *Inefficient Growth Paths*

The preceding analysis can be made to show immediately that some golden-age paths are inefficient. Consider any golden age in which the capital-effective labor ratio forever exceeds its GR value. It will be dominated by a policy of immediately gobbling up the "excess" capital and forever after maintaining the capital-effective labor ratio at its GR value, i.e., following the GR path; such a policy will clearly make consumption higher at every point in time. It follows that any investment policy which at some point permanently fixes the capital-effective labor ratio at a level exceeding the GR level is inefficient and therefore cannot be optimal (since a policy to be optimal must be optimal at every stage).

In the author's reply to Pearce an obvious generalization of this result to non-golden-age paths was conjectured: "Any policy which causes the capital-output ratio [equivalently, the capital-effective labor ratio, since the one ratio is a monotonically increasing function of the other] permanently to exceed—always by some minimum finite amount—its GR level is inefficient and hence cannot be optimal" [8, p. 1099]. A proof of this conjecture was later communicated to the author by Tjalling Koopmans. In what follows we present what is essentially Koopmans' proof and then employ the technique to prove an analogous theorem for the case in which technical progress must be described as (at least partially) capital-augmenting, for the case of nonexponential labor growth and factor augmentation, and finally for the case in which technical progress cannot necessarily be described as factor-augmenting.

We confine our analysis to the neoclassical production function, although the theorems proved clearly carry over to the Harrod-Domar production function.

A. *Pure Labor Augmentation at a Constant Rate*

Suppose first that technical progress can be described as solely labor-augmenting and that the rate of labor augmentation is a constant, λ. Then, as was shown above, when $k(t)$ is fixed, the consumption path is given by the equation

$$C(t) = [f(k) - (\gamma + \lambda + \delta)k]L_0e^{(\gamma+\lambda)t} \tag{12}$$

where $f'(k)>0, f''(k)<0$.

We show now that if $k(t)$ is not fixed, then the consumption path is given by the equation

$$C(t) = [f(k(t)) - (\gamma + \lambda + \delta)k(t) - \dot{k}(t)]L_0e^{(\gamma+\lambda)t} \tag{26}$$

Proof: From (3), (4), and (9) we have

$$C(t) + \dot{K}(t) + \delta K(t) = L_0e^{(\gamma+\lambda)t}f(k(t)) \tag{27}$$

or

$$\frac{C(t)}{L_0e^{(\gamma+\lambda)t}} = f(k(t)) - \delta k(t) - \frac{\dot{K}(t)}{L_0e^{(\gamma+\lambda)t}}. \tag{28}$$

Now, differentiating $k(t)$ with respect to time, we have

$$\dot{k}(t) = \frac{\dot{K}(t)}{L_0e^{(\gamma+\lambda)t}} - (\gamma + \lambda)\frac{K(t)}{L_0e^{(\gamma+\lambda)t}} \tag{29}$$

or

$$\frac{\dot{K}(t)}{L_0e^{(\gamma+\lambda)t}} = \dot{k}(t) + (\gamma + \lambda)k(t). \tag{30}$$

Substituting (30) into (28) yields (26).

Assume now that there exists a GR path, hence a GR value of $k(t)$, say $\hat{k}$. For simplicity only, we assume that the GR maximum is an interior one so that $\hat{k}$ is determined by the equation, derived from (12) (see also (16a)):

$$f'(\hat{k}) = \gamma + \lambda + \delta. \tag{31}$$

As a consequence of (31), the expression $f(k)-(\gamma+\lambda+\delta)k$ is monotonically increasing in k up to $k=\hat{k}$ and monotonically decreasing in k for all $k>\hat{k}$.

Consider now any capital-path which "violates" the Golden Rule in that, at some point in time (perhaps initially) and thereafter, it keeps the capital-effective labor ratio in excess of its GR value by at least some positive, constant amount. That is, consider any path $k(t)$ such that, for all $t \geq t_0 \geq 0$,

(32) $$k(t) \geq \hat{k} + \epsilon, \qquad \epsilon > 0 \text{ and independent of } t.$$

Then the following theorem can be proved:

> Any path satisfying (32) is "dynamically inefficient" or (equivalently) "dominated," for there always exists another path which, starting from the same initial capital stock, provides more consumption at least some of the time and never less consumption.

Proof: Define another path, $k^*(t)$, such that

(33) $$k^*(t) = \begin{cases} k(t), & 0 \leq t < t_0; \\ k(t) - \epsilon, & t \geq t_0. \end{cases}$$

In the first interval, $0 \leq t \leq t_0$, the two paths are identical so that $C^*(t) = C(t)$ in this interval (which will not exist if $t_0 = 0$). At $t = t_0$, the starred path gives a discontinuous consumption bonus, for an amount of capital equal to $\epsilon L_0 e^{gt_0}$ is instantly consumed so as to make $k^*(t) = k(t) - \epsilon$ at $t = t_0$. In the remaining interval, $t > t_0$, the difference between the consumption rate offered by the starred path and the path specified in (32) is implied by (26) to be

(34) $$C^*(t) - C(t) = \{[f(k^*(t)) - (\gamma + \lambda + \delta)k^*(t) - \dot{k}^*(t)] - [f(k(t)) - (\gamma + \lambda + \delta)k(t) - \dot{k}(t)]\} L_0 e^{(\gamma+\lambda)t}.$$

But observe that, for all $t > t_0$, $\dot{k}^*(t) = \dot{k}(t)$ since the two paths differ after t_0 by only a constant, ϵ. Hence (33) and (34) imply

(35) $$C^*(t) - C(t) = \{[f(k^*(t)) - (\gamma + \lambda + \delta)k^*(t)] - [f(k(t)) - (\gamma + \lambda + \delta)k(t)]\} L_0 e^{(\gamma+\lambda)t}.$$

The righthand side of (35) is strictly positive for all $t > t_0$ since $k^*(t) \geq \hat{k}$, $k(t) > k^*(t)$ and $f(k) - (\gamma + \lambda + \delta)k$ is strictly decreasing in k for all $k > \hat{k}$. Hence, in the interval $t > t_0$ the starred path gives more consumption at every point in time. Therefore, the starred path dominates the other path for it is never worse and is better for all $t \geq t_0$.

To elaborate a little on the last step of the proof, note that $k^*(t) \geq \hat{k}$ because $k^*(t)$ is only ϵ smaller than $k(t)$ and the latter is at least ϵ larger than $\hat{k}$ for all t. Figure 3 illustrates why $f(k^*(t)) - (\gamma + \lambda + \delta)k^*(t) > f(k(t)) - (\gamma + \lambda + \delta)k(t)$ for any $t > t_0$.

The theorem can be expressed in another way. Since the social net rate of return to investment (and the competitive rate of interest), $f'(k(t)) - \delta$, is a monotonically decreasing function of $k(t)$ and independent of time, an equivalent proposition is that any growth path which keeps the rate of return to investment forever and finitely below its GR value (the golden-age growth rate on the assumption expressed by (31)

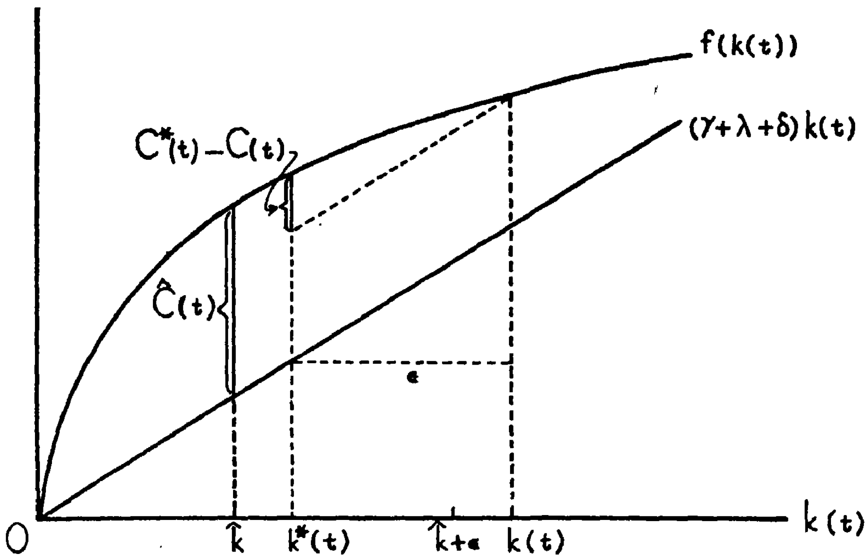


FIGURE 3

is dynamically inefficient. Or the proposition can be expressed in terms of the capital-output ratio, as we first conjectured it.

Another remark is that the neoclassical assumptions $f'(k)>0$ and $f''(k)<0$ for all k are far stronger than necessary for the theorem. If $f''(k)=0$ for all $k>\hat{k}$, for example (where $\hat{k}$ is now defined as the smallest k for which $f'(k)=(\gamma+\lambda+\delta)$), then, while the two paths will yield the same consumption path after t_0, the starred path still offers the consumption bonus at t_0, and hence dominates the other path. Secondly, the theorem is trivial in the Harrod-Domar case, where $f'(k)=0$ for $k>\hat{k}$, for it simply means that any path which keeps capital permanently in surplus is inefficient, and this hardly needs proving.

B. *Factor-Augmenting Progress*

We turn now to the case in which technical progress can be described as factor-augmenting and may be partially or wholly capital-augmenting. Suppose that the rate of capital augmentation is a constant, $\mu\geq 0$. And suppose once again that we have a neoclassical production function. Then

$$Q(t) = F[e^{\mu t}K(t), e^{\lambda t}L(t)], \qquad \mu \geq 0, \lambda \geq 0. \tag{36}$$

In the spirit of the first part of this paper, we define

$$k(t) = \frac{K(t)}{L_0 e^{(\gamma+\lambda-\mu)t}}, \tag{37}$$

which is the ratio of "effective capital" to "effective labor," and

$$f(k(t)) = F[k(t), 1] \tag{38}$$

to obtain, by virtue of constant returns to scale,

$$Q(t) = L_0 e^{(\gamma+\lambda)t} f(k(t)). \tag{39}$$

To obtain the consumption path as a function of $k(t)$, we follow the same procedure used to obtain (26). From

$$C(t) + \dot{K}(t) + \delta K(t) = L_0 e^{(\gamma+\lambda)t} f(k(t)) \tag{40}$$

we have

$$\frac{C(t)}{L_0 e^{(\gamma+\lambda-\mu)t}} = e^{\mu t} f(k(t)) - \delta k(t) - \frac{\dot{K}(t)}{L_0 e^{(\gamma+\lambda-\mu)t}}. \tag{41}$$

From

$$\dot{k}(t) = \frac{\dot{K}(t)}{L_0 e^{(\gamma+\lambda-\mu)t}} - (\gamma + \lambda - \mu) \frac{K(t)}{L_0 e^{(\gamma+\lambda-\mu)t}} \tag{42}$$

we have

$$\frac{\dot{K}}{L_0 e^{(\gamma+\lambda-\mu)t}} = \dot{k}(t) + (\gamma + \lambda - \mu) k(t). \tag{43}$$

Hence, from (41) and (43),

$$C(t) = \{e^{\mu t} f(k(t)) - (\gamma + \lambda + \delta - \mu) k(t) - \dot{k}(t)\} L_0 e^{(\gamma+\lambda-\mu)t}. \tag{44}$$

(If $\mu = 0$, we obtain (26) again.)

Now we define $\hat{k}(t)$ as the value of $k(t)$ which, for fixed $\dot{k}(t)$ and a particular t, maximizes $C(t)$. For simplicity only we assume an interior maximum is attained so that $\hat{k}(t)$ is defined by[8]

$$e^{\mu t} f'(\hat{k}(t)) = \gamma + \lambda + \delta - \mu. \tag{45}$$

Of course, $e^{\mu t} f'(k(t))$ is just the marginal productivity of capital at time t.[9] Hence the path $\hat{k}(t)$ defined by (45) is a constant interest-rate path in which the (competitive) interest rate is $e^{\mu t} f'(k(t)) - \delta = \gamma + \lambda - \mu$.

We know that $\hat{k}(t)$ is not the GR path; no family of golden-age paths exist when $\mu > 0$, and hence no GR path exists. Nevertheless we shall dub this path the Quasi-Golden-Rule path. For we shall demonstrate

[8] If $f'(k) > 0$ for all k, as we assume, then $\gamma + \lambda - \mu > 0$ is required for the existence of such a value of $k(t)$.

Note that $\hat{k}(r)$ must be increasing over time if $\mu > 0$; and if $\lambda + \delta - \mu > 0$, then so must $\hat{K}(t)$, by (37).

[9] $$\frac{\partial F(e^{\mu t} K(t), e^{\lambda t} L(t))}{\partial K(t)} = e^{\mu t} \frac{\partial F\left(\frac{e^{\mu t} K(t)}{e^{(\gamma+\lambda)t} L_0}, 1\right)}{\partial \left(\frac{e^{\mu t} K(t)}{e^{(\gamma+\lambda)t} L_0}\right)} = e^{\mu t} f'(k(t)).$$

that it is like the GR path in the following respect: Any path which, at some point in time and forever after, keeps the ratio of effective capital to effective labor in excess of the Quasi-GR value of that ratio, $\hat{k}(t)$, is dynamically inefficient.[10]

Such a path is one which causes $k(t)$ to satisfy, for all $t \geq t_0 \geq 0$,

$$k(t) \geq \hat{k}(t) + \epsilon, \qquad \epsilon > 0 \quad \text{and} \quad \text{constant.} \tag{46}$$

We show now that the following path dominates any such path:

$$k^*(t) = \begin{cases} k(t), & 0 < t < t_0; \\ k(t) - \epsilon, & t \geq t_0. \end{cases} \tag{47}$$

Comparing the associated consumption paths, we observe first that the two paths yield identical consumption paths until t_0. At this point the starred path yields a consumption bonus, unlike the other path. Subsequently, $\dot{k}^*(t) = \dot{k}(t)$, since, for $t > t_0$, $k^*(t)$ and $k(t)$ differ only by the constant, ϵ. Hence for all $t > t_0$,

$$\begin{aligned} C^*(t) - C(t) = \{ & [e^{\mu t} f(k^*(t)) - (\gamma + \lambda + \delta - \mu) k^*(t)] \\ & - [e^{\mu t} f(k(t)) - (\gamma + \lambda + \delta - \mu) k(t)] \} L_0 e^{(\gamma + \lambda - \mu) t}. \end{aligned} \tag{48}$$

The right-hand side of (48) must be positive for every t, since $k(t) > k^*(t) \geq \hat{k}(t)$, and $e^{\mu t} f(k(t)) - (\gamma + \lambda + \delta - \mu) k(t)$ is, for every t, monotonically decreasing in $k(t)$ in the range $k(t) > \hat{k}(t)$ (since $\hat{k}(t)$ is maximal and $f''(k(t)) < 0$). Hence, the starred path dominates the path which transgresses the Quasi-Golden-Rule path. Therefore, any path which violates the Quasi-Golden-Rule path in the manner described in (46) is dynamically inefficient.[11]

[10] While the Quasi-GR path does not dominate other constant interest-rate paths, it does dominate all $k(t)$ paths parallel to it so it is in fact a Generalized Golden Rule path.

[11] We have just shown that (46), that is, $k(t) \geq \hat{k}(t) + \epsilon$, is a sufficient condition that a $k(t)$ path be dominated by another path on which $k(t)$ is smaller by a constant amount. We show here that $k(t) > \hat{k}(t)$ is necessary that a path $k(t)$ be dominated *in this way*; but that $k(t) > \hat{k}(t)$ is not sufficient for such dominance.

First we show that every $k(t)$ path so dominated is a path along which $e^{\mu t} f'(k(t)) < \gamma + \lambda + \delta - \mu$, and hence $k(t) > \hat{k}(t)$, for all $t \geq t_0$.

Proof: Choose any path $k(t) \geq 0$ and suppose that it is dominated by another path $k^*(t) = k(t) - \epsilon$, $\epsilon > 0$ for $t \geq t_0$. Then, for every $t \geq t_0$ we have

$$\begin{aligned} C^*(t) - C(t) = \{ & [e^{\mu t} f(k(t) - \epsilon) - (\gamma + \lambda + \delta - \mu)(k(t) - \epsilon)] \\ & - [e^{\mu t} f(k(t)) - (\gamma + \lambda + \delta - \mu) k(t)] \} L_0 e^{(\gamma + \lambda - \mu) t} \geq 0. \end{aligned}$$

Then it is immediately clear that, for every $t \geq t_0$, $k(t)$ must exceed $\hat{k}(t)$; that is, $k(t)$ must lie on the right side of the hill whose peak occurs at $k(t) = \hat{k}(t)$, i.e., where $e^{\mu t} f(k(t)) - (\gamma + \lambda + \delta - \mu) k(t)$ is at a maximum.

This proves that $k(t) > \hat{k}(t)$ is a necessary condition that a path be dominated in the manner described. We show next that $k(t) > \hat{k}(t)$ is not a sufficient condition. Consider a path $k(t) > \hat{k}(t)$ with

$$\lim_{t \to \infty} [k(t) - \hat{k}(t)] = 0.$$

We can relax without difficulty the assumptions that the labor force and the technology increase at constant rates. Further, we may allow the depreciation rate at time t, $\delta(t)$, (the same for capital goods of every age) to vary with time. Write

$$Q(t) = F[B(t)K(t), A(t)L(t)] \tag{49}$$

where $A(t)$, $B(t)$ and $L(t)$ are continuously differentiable functions of time. Then, defining $k(t)=B(t)K(t)/A(t)L(t)$, one can easily derive

$$C(t) = \left\{B(t)f(k(t)) - \left[\frac{\dot{L}(t)}{L(t)} + \frac{\dot{A}(t)}{A(t)} + \delta(t) - \frac{\dot{B}(t)}{B(t)}\right]k(t) - \dot{k}(t)\right\} \cdot \frac{A(t)L(t)}{B(t)} \tag{50}$$

where $f(k(t))=F[k(t), 1]$.

Next we define the Generalized Quasi-GR path, $\hat{k}(t)$, by

$$B(t)f'(\hat{k}(t)) = \frac{\dot{L}(t)}{L(t)} + \frac{\dot{A}(t)}{A(t)} + \delta(t) - \frac{\dot{B}(t)}{B(t)}. \tag{51}$$

This may be a variable interest-rate path.

It can then be shown, in precisely the same manner as before, that any path which makes $k(t) \geq \hat{k}(t)+\epsilon$, $\epsilon > 0$, is dynamically inefficient.[12]

Then, for any $\epsilon>0$ and sufficiently large t,

$$C^*(t) - C(t) = \{[e^{\mu t}f(k(t) - \epsilon) - (\gamma + \lambda + \delta - \mu)(k(t) - \epsilon)] - [e^{\mu t}f(k(t)) - (\gamma + \lambda + \delta - \mu)k(t)]\}L_0e^{(\gamma+\lambda-\mu)t} < 0$$

since, for any $\epsilon>0$,

$$\lim_{t\to\infty} [k(t) - \epsilon - \hat{k}(t)] < 0$$

and

$$[e^{\mu t}f(k(t)) - (\gamma + \lambda + \delta - \mu)k(t)] < [e^{\mu t}f(\hat{k}(t)) - (\gamma + \lambda + \delta - \mu)\hat{k}(t)] \quad \text{whenever } k(t) < \hat{k}(t).$$

Hence, $k(t) > \hat{k}(t)$ is not a sufficient condition that the path $k(t)$ be dominated.

It does not follow that (46) is necessary for a path to be dominated by a path described in (47), although that can probably be shown, at least on certain additional assumptions. In any case, it should be emphasized, however, that (46) is not a necessary condition for a $k(t)$ path to be dominated in *any* way. In other words, it is not argued that (46) is a necessary condition for dynamical inefficiency; it has only been suggested in the present paragraph that (46) is a necessary condition for a path to be dominated by a path which relates to it in the particular way specified in (47).

[12] In the purely labor-augmenting case, our theorems imply that all paths which keep the interest rate always finitely below the GR or Quasi-GR value are dynamically inefficient, provided that $(\dot{L}(t)/L(t)+(\dot{A}(t)/A(t))+\delta(t)$ has an upper bound. For if, for all t, $r(t) \leq \hat{r}(t)-\eta$, $\eta>0$, where $r(t)=f'(k(t))-\delta(t)$ and $\hat{r}(t)=f'(\hat{k}(t))-\delta(t)=(\dot{L}(t)/L(t))+(\dot{A}(t)/A(t))$, then $f'(\hat{k}(t))-f'(k(t)) \geq \eta$; but if $f''(k)<0$ and $f'(\hat{k}(t))$ is bounded from above (because $(\dot{L}(t)/L(t))+(\dot{A}(t)/A(t))+\delta(t)$ is bounded), then it follows that $k(t) \geq \hat{k}(t)+\epsilon$ for some constant $\epsilon>0$.

But if there is capital-augmenting progress and $B(t)\to\infty$ as $t\to\infty$, then our theorems do

Note that if technical progress is Hicks-neutral, so that

$$Q(t) = A(t)\, F[K(t), L(t)]$$

then, since (by constant returns to scale)

$$A(t)\, F[K(t), L(t)] = F[A(t)\,|\,K(t), A(t)\, L(t)],$$

we have $B(t) = A(t)$ and $(\dot{B}(t)/B(t)) = (\dot{A}(t)/A(t))$ in (51). In this case, the interest rate path corresponding to the Generalized Quasi-GR path is the same as for the case of no technical progress; the interest rate at t equals $\dot{L}(t)/L(t)$.

This observation suggests that if rates of factor augmentation are not defined then the Generalized Quasi-GR interest rate path is just the path of $\dot{L}(t)/L(t)$. We now demonstrate this.

C. *Nonfactor-Augmenting Progress*

Here we write the neoclassical production function in the form

(52) $$Q(t) = F[K(t), L(t); t]. \qquad (52)$$

Then, by constant returns to scale,

(53) $$Q(t) = L(t) f(k(t); t)$$

where

(54) $$k(t) = \frac{K(t)}{L(t)}$$

and

(55) $$f(k(t); t) = F\left[\frac{K(t)}{L(t)}, 1; t\right].$$

From (53), (3) and (4) we have

(56) $$\frac{C(t)}{L(t)} = f(k(t); t) - \delta k(t) - \frac{\dot{K}(t)}{L(t)}.$$

From (54) we have

(57) $$\dot{k}(t) = \frac{\dot{K}(t)}{L(t)} - \frac{\dot{L}(t)}{L(t)} k(t).$$

---

not imply that all paths which keep the interest rate finitely below the Quasi-GR value are dynamically inefficient. To see this, consider a path such that $r(t) \leq \hat{r}(t) - \eta$, $\eta > 0$, where now $r(t) = B(t) f'(k(t)) - \delta(t)$ and $\hat{r}(t) = B(t) f'(\hat{k}(t)) - \delta(t) = (\dot{L}(t)/L(t)) + (\dot{A}(t)/A(t)) - (\dot{B}(t)/B(t))$. Then $f'(\hat{k}(t)) - f'(k(t)) \geq \eta / B(t)$. If $B(t) \to \infty$ as $t \to \infty$ then, while $k(t) > \hat{k}(t)$ for all t, $k(t) \to \hat{k}(t)$ as $t \to \infty$ is possible. Hence "$k(t) \geq \hat{k}(t) + \epsilon$, $\epsilon > 0$" is not necessarily true of such a path, so the inefficiency of all such low-interest-rate paths is not implied. For $k(t) > \hat{k}(t)$ is not a sufficient condition that a path $k(t)$ be dominated, as the preceding footnote showed.

Equations (56) and (57) yield

$$C(t) = \left\{ f(k(t); t) - \left[\frac{\dot{L}(t)}{L(t)} + \delta \right] k(t) - \dot{k}(t) \right\} L(t). \tag{58}$$

It is clear now that the Generalized Quasi-GR path, $\hat{k}(t)$, is defined by

$$f_k(\hat{k}(t); t) = \frac{\dot{L}(t)}{L(t)} + \delta. \tag{59}$$

It can be shown, by the same method that we have been using, that any path which, at t_0 and forever after, keeps $k(t) \geq \hat{k}(t) + \epsilon$ is dominated by a path $k^*(t) = k(t)$, $t < t_0$, $k^*(t) = k(t) - \epsilon$, $t \geq t_0$, so that such a path is dynamically inefficient.

Note that the interest rate, $f_k - \delta$, associated with the Generalized Quasi-GR path is the path of $\dot{L}(t)/L(t)$ which is independent of t. Hence if technical progress cannot be described in purely input-augmenting terms then the critical interest rate path is just the path of $\dot{L}(t)/L(t)$.

III. *Concluding Remarks*

It was demonstrated that a Golden-Rule path, that is, a consumption maximizing golden-age path, always exists in the neoclassical and Harrod-Domar models if the labor force increases at a constant rate, the depreciation rate is constant, technical progress, if any, is purely labor-augmenting, labor augmentation occurs at a constant rate, and positive labor is required for positive output. It was also demonstrated that a positive-investment GR path exists only if any technical progress present can be described as purely labor-augmenting.

It was then shown that any path which permanently deepens capital in excess of the GR path is dynamically inefficient—it is dominated with respect to consumption by another path. Further, if labor augmentation or labor-force growth is nonexponential or if technical progress cannot be described as purely labor-augmenting, then, while no GR path will exist, there may exist a Generalized Quasi-GR path having the same property, namely, that any path which permanently deepens capital in excess of that path is dynamically inefficient. (Note that such paths do not exhaust the class of dynamically inefficient paths. For example, even if no Quasi-GR path exists, the growth path produced by a permanently unitary saving ratio is clearly dynamically inefficient.)

Concerning the significance of these findings, I believe that it is of considerable theoretical interest to know that certain growth paths, even growth paths with continuously positive interest rate and less-than-unitary saving ratio, are dynamically inefficient. The practical importance of these findings is arguable. Beware of the weakness of what

has been proved here. The growth paths shown to be dynamically inefficient are paths on which capital is excessive *forever*, that is, for infinite time. Whatever a nation does over a finite time cannot be shown to be dynamically inefficient in the sense of this paper; for what the nation does subsequently may save the entire growth path from being dominated.[13] At best, the economist armed with this paper can say to a country—be it a Soviet-type economy or a capitalist economy—that its public policies and private propensities are such that, if not *eventually* changed, dynamical inefficiency will result. But he cannot say that these policies must be changed within the year or in the next billion years. Such wisdom is not without practical value, I think. But it is to be hoped that some day economists will have stronger recommendations to make in the area of growth policy.

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[13] This observation leads to another qualification. In a world of uncertainty, as Pearce has observed [6], an economy may rationally deepen capital "excessively" in order to possess a "war chest" of capital for consumption in the event of an earthquake, a war, and other probabilistic phenomena. If these events never occur, so that the war chest is never consumed and capital is always "excessive," then, while the war chest strategy will be regretted from hindsight, it cannot be said to be irrational. But I doubt that such uncertainties are of sufficient quantitative importance to justify an appreciable war chest.

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COMMUNICATIONS

"Surplus" Agricultural Labor and Development: Facts and Theories

The purpose of this article is to review several strategic assumptions regarding labor productivity and unemployment in the context of economic development theories, and to test these assumptions against some recently available statistical data. The results described below throw in doubt some fundamental conclusions derived from models using the concept of surplus labor and disguised unemployment, and indicate the need for a more thorough use of available statistics in the analysis of development problems.

The focus of the discussion is Indian agriculture which is frequently cited as a classic case of disguised unemployment.[1] The data are largely drawn from the extensive 17-volume *Farm Management Studies* of Indian agriculture, which covered on a sample basis, but in considerable depth, the six main agricultural regions of India.[2] Since the data are at variance with a few previous studies on labor productivity in underdeveloped areas [38], [39], [51], [53], it is important to note that the Farm Management data were not based on a small local survey but involved 2,962 holdings covering the major crops grown in India. A large number of specially trained field inspectors observed farm operations in the sample districts and prepared cost-accounting records for the farms on a regular, almost daily, basis for a three-year period. In scope and refinement the *Farm Management Studies* easily surpass all previous field studies of the economics of peasant agriculture.[3]

What light does this material throw on the question of surplus agricultural

[1] There are many theoretical articles, but very few empirical studies, on surplus labor and disguised unemployment. The literature is briefly reviewed in [32]. A carefully done empirical study for Greece [44] yields results which partly coincide with the conclusions drawn here. For India, Schultz's recent work [47] uses historical data to support the view that the marginal productivity of labor is not zero, but this is based on evidence of some forty years ago, when India had a population about 180 million less than at present, and therefore is not directly relevant to the current controversy on surplus labor and development policy. However, Schultz's pointed criticism of the surplus labor hypothesis is fully borne out by the present study.

[2] Madras, Uttar Pradesh, Madhya Pradesh, West Bengal, Bombay, and Punjab. The Madhya Pradesh survey did not start until 1955-56, a year after the others.

[3] Under this plan 1,096 farms or 37 per cent of the total holdings were studied intensively by the cost-accounting method, and the remaining 63 per cent by a less costly procedure referred to as the survey method. Only the cost-accounting data were used here, but on all the issues discussed in this article, the survey method gave substantially similar results. The first-year volumes (1954-55) of the *Farm Management Studies* represented a "breaking in" period for the staff and the survey techniques employed and may be considered less reliable than the second- and third-year studies for each area. Therefore, the latter have been more heavily relied on here. Although the general plan underlying the studies is similar, there is considerable variation in the detailed statistics reported. Hence conceptually consistent data are frequently not available for all six states or for all three years.

labor and population? If surplus labor is interpreted to mean either (1) labor working, but not adding to output (disguised unemployment), or (2) labor chronically unemployed, which labor (in both categories) could be removed for a whole year without affecting output, then the data show that there is no significant surplus of labor for Indian agriculture as a whole. Static conditions are initially assumed, i.e., no major reorganization of agriculture is considered to accompany the removal of labor. Subsequently, the effect of reorganization on the demand for labor will be discussed. Local unemployment as well as local labor shortages may nevertheless exist, but since theoretical discussion of this issue focuses on *the* marginal productivity of Indian agricultural labor as a whole, it must be assumed by those who accept the surplus labor thesis that intra-agricultural transfers of labor would not appreciably affect the arguments supporting the doctrine of zero productivity and surplus agricultural labor. The movement of labor *out* of agriculture rather than shifts *within* agriculture is the typical policy implication. In fact, it is generally believed that rational shifting and reorganization of the labor force within agriculture would generate a still larger surplus of labor [5, pp. 116 ff.]. The present paper attempts to show that large-scale opportunities for additional employment exist within agriculture, and that the output of the current labor force could be increased by redistribution of labor within the sector. Chronic or disguised unemployment of family labor, presumably because of small landholdings, will also be called into question as a generalization applicable to farms in the smaller size classes. Finally, the common assumption that economic development will produce a large exodus of labor from agriculture is shown to be in need of drastic revision.

I. *Farm Size and Output per Acre*

One of the most surprising results of the *Farm Management Studies* is the relationship found between farm size and output per acre. In a large proportion of the districts, and for most crops, output per acre declined with farm size. This is shown in Table 1 where value of agricultural output (in rupees), classified by size of holdings, is presented for five states. It is difficult to avoid coming to the conclusion that the larger farms in India (over 10 acres) are markedly underutilized in terms of economically feasible levels of intensity of cultivation. The average yield per acre on these large farms is consistently below the yield of the small farms. The reasons for this will be examined in a moment, but here it may be noted that in this underutilized resource there is an unexpected safety valve, for as population increases and the average size of holding goes down, output per acre can be expected to increase, insofar as the poorer yield per acre of the large holdings is attributable to low motivation and lower labor inputs rather than to land of lesser intrinsic fertility. This then is the first question to be raised: are differences in fertility the reason for the observed variations in output? Is the size of holding likely to be negatively correlated with fertility? Natural fertility, not the result of improvements, is difficult to measure and is apt to be confused with productivity per acre. However, even in the absence of direct measurement there is evidence that variation in fertility is not a likely explanation for the observed

TABLE 1—AVERAGE INPUT AND OUTPUT PER ACRE BY SIZE OF FARM[a]

(Values in Rupees)

West Bengal				Madras				Madhya Pradesh				Uttar Pradesh				Punjab			
Size-group (acres)	Inputs		Out-put	Size-group	Inputs		Out-put	Size-group	Inputs		Out-put	Size-group	Inputs		Out-put	Size-group	Inputs		Out-put
	A	C			A	C			A	C			A	C			A	C	
0.01– 1.25	128.6	186.5	202.1	0–2.5	217.9	291.8	219.9	0–5	69.9	99.9	143.3	0–5	241.8	259	277	0–5	126.9	219.4	176.5
1.26– 2.50	110.9	159.2	194.8	2.5–5.0	119.6	174.8	205.2	5–10	57.8	87.8	115.1	5–10	192.2	209	240	5–10	107.6	181.3	170.5
2.51– 3.75	125.2	178.1	175.9	5.0–7.5	95.3	140.4	157.4	10–15	56.3	86.3	118.9	10–15	168.6	185	204	10–20	108.5	178.6	169.8
3.76– 5.00	103.2	146.9	181.0	7.5–10	90.4	137.9	134.8	15–20	46.5	76.5	98.5	15–20	157.6	173	200	20–50	77.6	138.0	142.6
5.01– 7.50	101.3	152.8	186.9	10–15	62.0	93.2	76.8	20–30	42.4	72.4	107.9	20 & over	130.7	145	205	50 & over	74.2	131.1	159.6
7.51–10.00	79.5	136.2	172.0	15–20	47.1	72.9	67.7	30–40	45.1	75.1	106.1								
10.01–15.00	73.1	98.2	149.1	20–25	50.0	74.4	73.3	40–50	52.0	82.0	98.7								
15.01 & over	96.2	116.3	141.9	25 & over	39.3	61.0	82.4	50 & over	47.8	77.8	107.3								
Average	106.1	151.2	178.1	Average	76.5	113.4	118.3	Average	48.5	78.5	107.8	Average	168.5	185	220	Average	90.8	156.0	153.3

[a] Data for all States refer to the year 1955–56 except Madhya Pradesh, 1956–57.
Source: Farm Management Studies [15–20] [22] as indicated: (1) *West Bengal*, pp. 47–48, 174; (2) *Madras*, pp. 58, 91; (3) *Madhya Pradesh*, pp. 40–41; (4) *Uttar Pradesh*, pp. 30, 33, 38, 39; (5) *Punjab*, p. 59.

differences in output per acre. Since the sample districts selected were fairly homogeneous with regard to general agricultural conditions within each district, while the districts range over a wide area of Indian agriculture, it seems quite unlikely that the larger farms, district by district, had consistently poorer soils than the smaller farms within the same district, though the possibility cannot be ruled out.

However, fertility may be rejected as a strategic element in the problem since differences in yield can be accounted for without introducing fertility as a variable. Differences in labor and material inputs per acre explain a large per cent of the variance in yield. Willingness to use additional inputs, rather than intrinsic differences of soil quality, seem to be mainly responsible for the observed results. Looking at Table 2, we see that the small farms consistently have a higher average intensity of cultivation than the larger farms in all the states for which data are available. Later on in Table 4 further evidence on the inverse relationship between farm size and intensity of cultivation is offered: labor inputs per acre measured in either standardized eight-hour days (Uttar Pradesh), or in rupees (the remaining states) show more labor per acre on the small farms. In this table, family labor of men, women, and children is given an imputed value equivalent to the market wage appropriate to each category.

Returning to Table 1, total inputs per acre are given (in rupees) in terms of two cost concepts. Input "A" refers to all cash outlays for hired labor, seed, etc., except for rent paid by tenant farmers. To this has been added depreciation of equipment and the imputed value of family labor. The difference between input "A" and value of output equals rent and profit earned per acre. Cost "A," while not including rent (which isn't a cash outlay for the large majority of farmers), has the virtue of avoiding the very difficult problem of imputing rent to the many fragments making up the typical In-

TABLE 2—INTENSITY OF CROPPING[a] ACCORDING TO SIZE OF FARMS

(Average for 1954–56)

Size of Farms (acres)	Intensity of Cropping		Size of Farms (acres)	Intensity	Size of Farms (acres)	Intensity
	Uttar Pradesh	Madras		West Bengal		Punjab
0–2.5	1.47	1.38	0.01–1.25	1.13	0–5	1.57
2.5–5.0	1.51	1.30	1.26–2.50	1.08	5–10	1.52
5.0–7.5	1.44	1.18	2.51–3.75	1.03	10–20	1.36
7.5–10	1.39	1.23	3.76–5.00	1.03	20–50	1.24
10–15	1.31	1.02	5.01–7.50	1.06	50 & over	1.06
15–20	1.29	0.89	7.51–10.00	0.96		
20–25	1.31	0.80	10.01–15.00	1.00		
25 & over	1.19	0.98	over 15.00	0.90		
Average	1.33	1.13	Average	1.03	Average	1.29

[a] Gross cultivated area divided by area available for cultivation. Areas double-cropped enlarged the gross total; fallow acreage may reduce the index below 1.00.

Source: *Farm Management Studies* [10–20].

dian farm. Input "C" gives us full cost including rent, paid and imputed; the difference between "C" and output represents entrepreneurial profit per acre. While "C" has the virtue of including all costs, it is less firmly based insofar as the imputation of rent is likely to be a task sufficient to tax the abilities of even a competent survey worker.[4] Both input series establish the inverse relationship between farm size and the intensity of cultivation, and indicate that higher outputs per acre generally result from higher inputs. Hence, the reason for the inverse relationship between size of farm and output per acre is found in the inverse correlation between size of farm and inputs per acre: the smaller farms average higher inputs and outputs than the larger farms. Despite the structural weakness of the small farms, their fragmentation, lack of credit, etc., they make up for this by intensive application of family labor and more inputs of complementary factors.

II. *Input-Output Functions and "Surplus" Labor*

The input-output data for 1,096 sample holdings in all five states using the cost "A" inputs, have been plotted in Figure 1. A linear regression yielded a correlation coefficient of .91 and the equation $Y = 65.4 + .908X$, but the basic relationship was better expressed by a curvilinear function $Y = -276 + 223 \log X$ which raised the correlation coefficient to .94, thus accounting for 88 per cent of the variation in output, as against 83 per cent with the linear equations.[5] A similar fit ($r = .91$) was obtained using cost "C" inputs—cf. Figure 2. It should be recalled that both sets of cost data contained a full and careful estimate of all imputed family labor valued at the market wage. This constituted a sizable per cent of the inputs, and if the marginal productivity of a large segment of family labor were zero, as has been claimed, it is doubtful that so high a correlation between inputs and outputs would have resulted. In fact, the supposed lack of correlation between inputs and outputs per acre in some earlier very limited studies [38] [53] was used as the basis for an extensive literature assuming an absolute surplus of agricultural labor with zero marginal productivity.

The difference between the regression curve and the 45° straight line (Figure 1) represents rent and profit per acre. While practically all the sample farms covered cost "A" (which includes imputed labor), a significant percentage failed to cover all the imputed costs in "C" as indicated in Figure 2. However, it is of interest to note that low-intensively cultivated large farms fell in this category as well as highly intensive small units. In practice many of the farms which failed to fully cover cost "C" can and do remain in operation because imputed costs do not have to be paid out (most Indian farmers own their own land) and the total family income from all sources is enough to cover the necessities of life.

Let us now examine the possibility that a significant part of the labor inputs

[4] The "C" series were taken from the *Farm Management Studies* directly. The above "A" series is not equivalent to the cost "A" concept used in the *Farm Management Studies*. Imputed family labor was derived by subtracting the *FMS* "B" cost data from cost "C". This was then added to the *FMS* "A" data to arrive at the "A" series used here.

[5] Both coefficients were highly significant at the .01 level.

used have zero marginal product. The disguised unemployment assumption requires that the least productive workers be removed from agriculture with no noticeable effect on output. Where are such workers likely to be found? Probably not on the medium and large farms because these farms have relatively low labor inputs, and furthermore they hire a sizable per cent of the labor used. Disguised unemployment is typically suspected on the small farms which have high inputs of labor per acre and depend more heavily on family labor. From the data presented in Tables 1 and 2, it is apparent that these small farms are the intensively cultivated ones. Referring to Figure 1, it is clear that more intensive cultivation via increased doses of capital and labor (land fixed) results in additional output. Those who postulate a large surplus of labor with zero productivity must argue that the same increase in output per acre would occur with a significant portion of the labor on the small farms removed. Now if the proportion of labor to capital inputs on the small farms were very high compared with the proportion found on the larger farms, then the disguised unemployment thesis might receive some encourage-

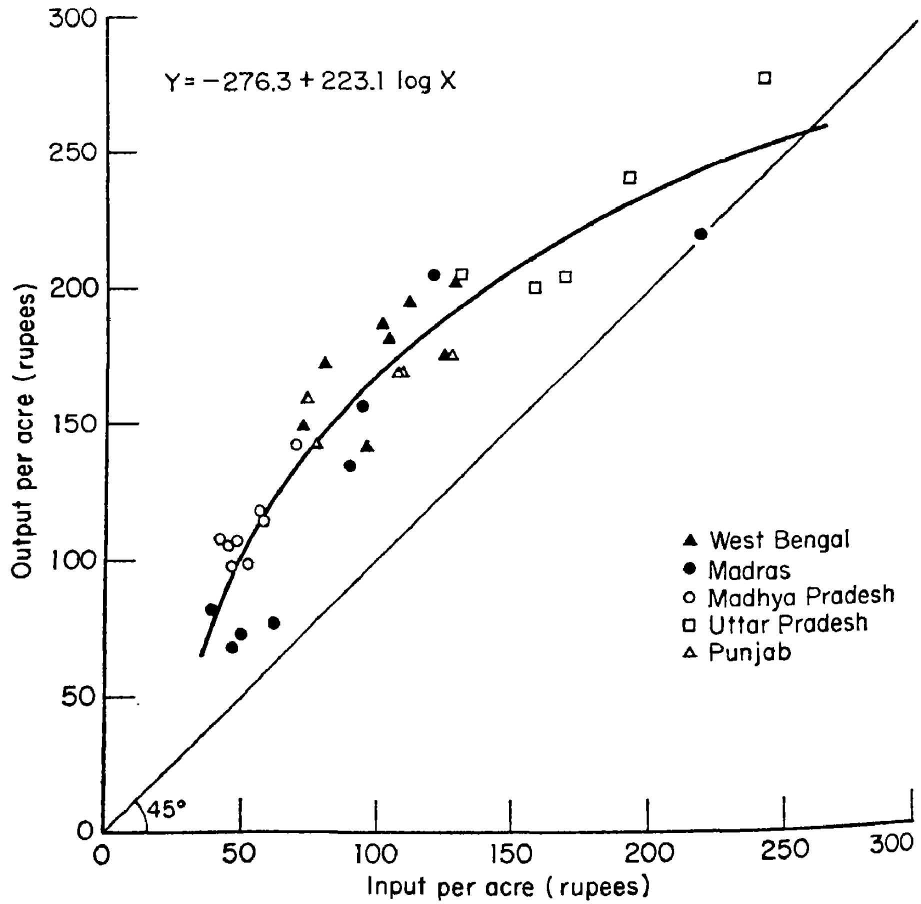


FIGURE 1. INPUT-OUTPUT FUNCTION USING COST "A" DATA

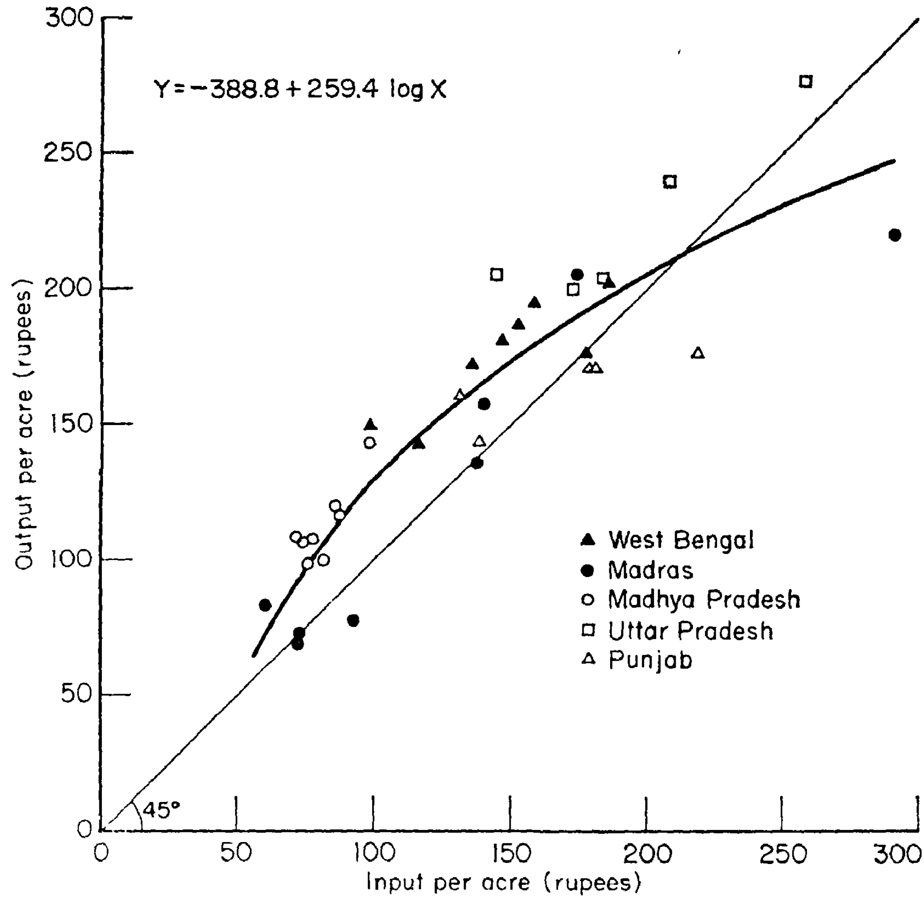


FIGURE 2. INPUT-OUTPUT FUNCTION USING COST "C" DATA

ment. But the data presented in Table 3 cut the ground from under this hypothesis. Family and hired labor inputs as a percentage of inputs "A" (working capital, depreciation of fixed capital, labor, etc.) is fairly stable over the size-groups within each state, even though inputs of labor per acre are highly variable by size of farm (Table 4). In Madhya Pradesh, Uttar Pradesh, and Punjab, the data in Table 3 show that each unit of labor on the small farms has *more* material inputs to work with than a unit of labor on the larger farms. Only in Madras is the highest percentage of labor found in the smallest size-group, and given the expected variation in factor proportions, this percentage appears quite normal. In general, on the small farms, land-saving techniques have resulted in a substitution of capital and labor for land, but the evidence indicates that labor has not been extensively substituted for capital. The small farmers use more bullock labor, fertilizer, and irrigation inputs per acre as well as more family labor—all related to the greater use of double-cropping indicated in Table 2. Given the basic similarity of agricultural techniques on the large and small farms, it should be clear that the

TABLE 3—TOTAL LABOR INPUTS PER ACRE AS A PER CENT OF INPUTS "A" BY SIZE OF FARM

West Bengal		Madras		Madhya Pradesh		Uttar Pradesh		Punjab	
Size-group (acres)	Labor as Per Cent of Cost "A"	Size-group (acres)	Labor as Per Cent of Cost "A"	Size-group (acres)	Labor as Per Cent of Cost "A"	Size-group (acres)	Labor as Per Cent of Cost "A"	Size-group (acres)	Lab or as Per Cent of Cost "A"
0.01– 1.25	76.0	0–2.5	31.8	0–5	46.2	0–5	26.5	0–5	41.9
1.26– 2.50	72.8	2.5–5.0	27.4	5–10	47.7	5–10	29.0	5–10	47.6
2.51– 3.75	70.9	5.0–7.5	27.4	10–15	45.7	10–15	31.7	10–20	43.3
3.76– 5.00	71.1	7.5–10	24.3	15–20	50.9	15–20	30.7	20–50	44.0
5.01– 7.50	73.2	10–15	24.8	20–30	50.9	20 & over	30.1	50 & over	47.3
7.51–10.00	77.0	15–20	27.4	30–40	50.4				
10.01–15.00	70.1	20–25	30.6	40–50	51.3				
15.01 & over	62.4	25 & over	29.0	50 & over	47.6				
Average	70.5	Average	27.5	Average	49.0	Average	29.8	Average	44.8

Sources: For Madras, Madhya Pradesh, and Punjab, Tables 1 and 4. West Bengal labor inputs are based on the weighted average of two districts [20, pp. 168, 171], Uttar Pradesh [19, p. 35].

TABLE 4—CLASSIFICATION OF TOTAL LABOR INPUTS PER ACRE AND PER CENT HIRED, 1955–56
(Labor input in days or value terms as indicated)

Uttar Pradesh			Madras		Bombay[a]			Madhya Pradesh[b]			Punjab		
Size-group (acres)	Total Labor Per Acre (days)	Per Cent Hired	Total Labor Per Acre (rupees)	Per Cent Hired	Size-group (acres)	Total Labor Per Acre (rupees)	Per Cent Hired	Size-group (acres)	Total Labor Per Acre (rupees)	Per Cent Hired	Size-group (acres)	Total Labor Per Acre (rupees)	Per Cent Hired
0–2.5	58.6	11	69.4	30	0–5	42.63	19	0–5	32.29	57	0–5	53.13	20
2.5–5.0	63.5	10	32.8	53	5–10	30.86	15	5–10	27.57	52	5–10	51.22	16
5.0–7.5	62.4	18	26.1	36	10–15	24.64	20	10–15	25.72	55	10–20	46.96	28
7.5–10	53.2	20	22.0	40	15–20	34.42	53	15–20	23.68	51	20–50	34.16	39
10–15	50.2	29	15.4	47	20–25	15.32	17	20–30	21.58	66	50 & over	35.13	76
15–20	45.3	36	12.9	37	25–30	15.88	44	30–40	22.73	62			
20–25	42.4	34	15.3	36	30–50	26.14	50	40–50	26.70	81			
25 & over	36.7	49	11.4	40	50 & over	10.95	33	50 & over	22.74	88			
Average	47.1	28	21.0	37	Average	21.13	36	Average	23.75	71	Average	44.12	36

[a] Ahmednagar District only.
[b] 1956–57
Source: Farm Management Studies [15–20, 22].

labor on the small farms is just as essential as the labor on the large ones, and hence there is little reason to believe that the higher labor inputs on the small farms represent disguised unemployment.

Another common variation of the disguised unemployment thesis runs as follows: the typical Indian landholding is so small that there is not sufficient land to employ the family workers productively, and hence chronic underemployment or hidden (disguised) unemployment is the result.[6] The supposed lack of work possibilities outside the family farm is of course crucial to the argument, yet available statistics show that the extent of outside work is quite considerable in many areas.[7] The data presented in Table 4 throw further doubt on the generalization of disguised unemployment. Here we have inputs of total labor per acre (family and hired) and per cent hired, by size of holding. It can be seen that even the farms in the smallest grouping use a significant per cent of hired labor, though as expected, a smaller percentage than the larger farms. Apparently then, for the smallest group as a whole, the marginal product of family labor must be positive, otherwise it would make no sense to hire outside workers. Most economists would interpret the existence of hired labor as proof per se that disguised unemployment was not present [44, pp. 33-34] [52, p. 18] [41, p. 33]. But Paul Wonnacott has argued that although it is "difficult to see why an individual employer would hire workers whose marginal product is zero," one should "open the door to the possibility of disguised unemployment by assuming that the 'typical' employer is persuaded by social and political pressures to hire his share of the labor force whether he wants to hire this number or not" [53, p. 284]. He then develops a diagrammatic model to illustrate this hypothetical situation. Such explanations seem pointless, at least on the basis of the present data which show clearly that hired labor adds considerably to output, and the employer hires this labor not because of social pressure but because it is profitable. The problem which must be explained in terms not adequately described by the competitive market mechanism is why the large farmers do not hire *more* labor (e.g., in Madhya Pradesh) when it would increase profits to do so.

Let us consider the potential productivity of additional labor inputs. These inputs should be added where their marginal productivity would be the highest, namely on the large farms which seem to use too little labor per acre to achieve optimum results. Of course it may be objected that the additional workers would require more inputs of the complementary factors, and a

[6] Leibenstein states in support of this hypothesis: "What can be observed . . . is the existence of exceedingly small holdings and the belief that in other areas a man can cultivate much larger holdings" [33, p. 59]. Given the law of variable proportions, this proves nothing as far as the zero marginal product hypothesis: Farmers might get more product if they are given more land; they also might get additional product cultivating their existing plots more intensively through double-cropping, etc. The data presented here point this up quite strongly.

[7] All agricultural laborers (both with and without their own land) averaged 29 work days on nonagricultural employment in 1950-51 and 27 days in 1956-57. Owners of small farms also work on larger farms as well as in the nonfarm sector. The data are summarized in [49, p. 268] and analyzed in detail in [27].

dearth of the latter may be the reason why the larger farms are underutilized. This is not a particularly weighty objection. The credit open to the larger farmers is certainly superior to that of the small farmers who cultivate so much more intensively. As to the supply of factors, one of the most important complementary inputs is irrigation, and the disappointing utilization of major irrigation projects, frequently even when the government offers the water free for the first three years, should create doubt as to whether the limitation is lack of associated inputs.[8] About 25 per cent of the available irrigation potential is currently wasted [30, pp. 381-82]. The farmer has to dig the field channels to utilize the water, but this just requires labor which is supposedly the factor in surplus.

However, even assuming that no additional capital inputs are available, a strong case can be made that some additions to the labor force could be productively employed, and there is little doubt that removal of a sizable number of agricultural workers would result in a significant decrease in output. *The Farm Management Studies* computed production functions for specific crops in sample districts; where good fits and statistically significant results were obtained, the marginal product of labor was clearly positive [15, pp. 168-69, 198] [19, pp. 49-55] [23, pp. 152-54] [26, pp. 123-24]. This evidence is further supported by the field reports which point to the considerable opportunities for productive application of more labor. While additional labor inputs are usually accompanied by other complementary inputs, there is much room for use of labor without capital. Mellor and Moorti in a field study in Uttar Pradesh found that "differences in yields (per acre) seem to be largely due to differences in the use of the fixed, low opportunity-cost inputs, in particular, family labour. . . . Farm to farm differences in soil were evenly distributed as between farm groups and so do not bias the results" [36, p. 48]. Examples abound in which additional labor can be used effectively. The true Japanese paddy method, now gradually being introduced into India, requires much more labor per acre than most Indian rice farmers presently use. Planting and transplanting the seedlings in rows is a highly labor-intensive activity which pays off in terms of greatly expanded output. While a small labor force can harvest a given area of paddy in a longer than optimum time (say 30 days), the quality and quantity collected will be inferior to a harvest carried out by a larger work force in a 15 day period [4, p. 31]. Weeding is a very

[8] An Indian observer notes: "Although irrigation is thus the most powerful instrument for increasing production, the lag observed between the creation of irrigation potential and its actual utilization by farmers is a disheartening phenomenon." Two policies have been suggested to correct this: (1) A flat tax per acre for water, whether used or not, might provide a monetary incentive. (2) Holdings in dry areas are large. It might be necessary to reduce the size of holdings such that the farmers would still find irrigation an attractive proposition as compared to dry cultivation on a larger holding. This would release land for settling more farmers in the area [43, p. 54]. John P. Lewis and others have confirmed the view that "Lack of outside capital is not the principal impediment to rapid expansion in production." The foreign exchange and scarce capital costs of eliminating such shortages as chemical fertilizers are "comparatively modest." "Most of the other capital formation that agriculture needs is highly labor-intensive; this too, in the last analysis is largely an organizational matter. And the rural credit problem, viewed in real terms, is entirely so" [34, p. 155].

TABLE 5—SEASONAL EMPLOYMENT PER ADULT MALE FARM WORKER AND TOTAL ANNUAL EMPLOYMENT IN UNITS OF EIGHT HOURS, 1956–57[a]

1956–57	West Bengal	Bombay Ahmednagar	Bombay Nasik	Uttar Pradesh	Punjab
March	23.4				
April	18.9				
May	19.1	23.8	27.4		
June	24.4	23.7	26.8	17	
July	23.1	21.6	25.9	19	24.0
August	22.6	25.2	27.6	22	22.6
Sept.	25.3	23.1	26.9	18	24.6
Oct.	18.6	25.1	29.5	20	24.1
Nov.	19.4	23.4	29.0	24	24.6
Dec.	28.9	27.9	29.0	26	21.6
Jan.	22.5	28.8	27.3	26	18.2
Feb.	20.3	26.6	25.9	22	18.5
March		26.8	27.4	25	20.7
April		23.3	26.2	25	29.9
May				26	33.2
June					24.6
Total	266.5	299.3	328.9	269	286.6

[a] Includes work on crop production, cattle maintenance, and "social, family and business affairs." The latter is typically defined as "Labour spent working on flour mill, repair of buildings, attending conferences, going to markets for sale of agricultural commodities or purchase of household requirements, etc" [12, p. 52]. The figures for Uttar Pradesh include work on crops and cattle only. The figures for "social and family labor" are as follows: Punjab, 21.1 days; West Bengal, 137.3 days; Ahmednagar, 30.6; Nasik, 29.3.

Source: Farm Management Studies: [26, pp. 201, 28] [24, p. 45] [21, pp. 345, 349] [25, p. 21]

important factor in getting a good jute crop and this involves nothing but labor [14, p. 111].

Apart from the obvious intensive employment of all available labor in the peak periods, the seasonal idleness commonly assumed to be so much in evidence is markedly overdrawn. Coale and Hoover not only write of large absolute surpluses of labor (in terms of man-years), but state that the Indian farmer works for only three months out of the year [5, p. 116].[9] This is effectively refuted by the data in Table 5 which indicate considerably greater year-round employment than is usually believed. In Indian discussions of unemployment it is customary to consider a man available for employment 365

[9] This otherwise soundly reasoned volume places undue credence in experts (some nameless) who believe that huge surpluses of labor are apparent in Indian agriculture. A number of the "experts" are obviously thinking in terms of a highly unlikely mechanization of agriculture which is clearly not a prospect for India in the near future [5, pp. 116 and ff.]. In contrast to the usual quotes describing surplus labor in agriculture, some of the *Farm Management Studies* field reports are instructive, e.g.: "The problem of supply of hired labour in some of our sample villages is gradually becoming acute due, on the one hand, to the increased demand for it as more stress is being laid on agriculture and on the other to the continuous depletion in the labour rank. . . . But the acute shortage of labour brings into use some amount of female labour . . . drawn to meet the heavy demand of labour which cannot be fully met by the male labour available in the villages" [14, p. 18].

days a year; any employment less than this results in time "unemployed." Fortunately, unemployment for want of work is usually separated from "unemployment for other reasons," so that the extent of true unemployment can be judged [27, p. 98]. By Western standards of a *full* work year (260 days) the average Indian agricultural employment level appears reasonable. For the sample data, the range was from 266.5 days for West Bengal to 328.9 days in Nasik (Bombay State). This applied to all adult male farm workers, both family operators and hired laborers. A work day was defined as equal to eight hours of labor. Hence work days may exceed calendar days in the month.

Finally, there is a concept of surplus labor based on work units rather than workers. Leibenstein [33, pp. 62-76] in developing this point of view reasons that if workers in an underdeveloped economy are given a higher wage and an improved diet, their level of health and efficiency would improve. Fewer workers could then perform the same tasks previously undertaken by the larger, less vigorous work force and produce the same output as before. The workers who could under these conditions be disposed of without reducing output are then considered "surplus" labor.[10] However, it makes sense to refer to these workers as surplus only if there is no productive employment for them. The above definition assumes this by postulating a given amount of useful work to be done and no more. As we have seen from the data presented, this is far from the truth, at least for India. If the *ceteris paribus* conditions are removed and it is assumed that the laborers and cultivators become healthier and more energetic, it seems reasonable to postulate a corresponding improvement in incentives. More intensive cultivation of the larger farms, use of available irrigation potential, and a shift to better seeds and methods—all easily possible within the framework of small-holding peasant agriculture—would result in large additional demands for labor (cf. Table 6). In this case the greater number of work units associated with the more energetic labor force would be used to enlarge total output, thus negating the surplus labor hypothesis.

III. *Land Use and Labor Requirements*

Why is there underutilization of land on many of the larger holdings? The field reports are particularly revealing. Time and again they indicate the disappointing performance of the larger farms: their reluctance to hire outside labor in sufficient quantity to produce satisfactory yields, and their lack of interest in utilizing irrigation water as a regular input to raise the intensity of cultivation, rather than as a stopgap measure in times of drought.[11] Since the

[10] "At very low wages there may be a labor deficit . . . but at higher wages the units of work per man increase so rapidly that a labor surplus is created" [33, p. 69].

[11] A typical comment from the Madras study: "Per acre results are much better on small farms below 5 acres in size. Even small farms of this size can be efficiently managed and made to yield a profit on the total position—this is established beyond a doubt. On the other hand it is seen that the large farms are a bit disappointing in the results they give. . . . Intensity of cultivation is much less on them, possibly because the permanent farm workers are fewer and the inputs are too thinly applied, especially human labour" [17, p. 129]. Similar observations are also in [20, p. 18]. Mellor and Moorti confirm this in their study of farms in the Agra District of Uttar Pradesh: "It appears that the general standard of crop husbandry is low among those farmers with a greater than average amount of land per worker" [36, p. 49].

TABLE 6—AVERAGE LABOR REQUIREMENTS PER ACRE FOR IRRIGATED AND UNIRRIGATED CROPS FOR SELECTED REGIONS, 1954–56
(In eight-hour man-days)

Crops	Unirrigated Land	Irrigated Land
	(Man-days per acre)	(Man-days per acre)
1. Wheat	18.65	38.51
2. Jowar (great millet)	12.94	43.64
3. Bajra (pearl millet)	15.51	39.70
4. Cotton	22.70	48.57
5. Gram	14.36	27.48
6. Rice	—	70.53
7. Sugar cane	—	68.23
Weighted average of first five crops	15.99	38.59
Weighted average of all seven crops	—	58.69

Source: Farm Management Studies [10–20].

farmer with a relatively large holding can eke out a moderate income without the trouble of hiring a high per cent of nonfamily labor, or the risk of borrowing additional working capital for other inputs associated with intensive cultivation, he frequently seems to prefer the low-effort, low-risk, low-output package to the higher-risk, higher-profit, higher-output combination. A theoretical explanation in terms of a low-aspiration model has been suggested by J. W. Mellor [37], and a revealing description of the range of attitudes toward work and profits found among Indian farmers is contained in the work of Kusum Nair [40]. The reasons for the poor utilization of some of the larger holdings need not be further elaborated here, but the implications for labor-force utilization are worth noting: limited incentives in many areas of Indian agriculture, rather than a critical man-land ratio, may be the more important bottleneck to increased production and employment. However, insofar as population growth leads to smaller average holdings through a breakup of the larger holdings, and to the extent that current government land policies produce a more equitable distribution of land, the bottleneck is relaxed and a safety valve, effective at least in the short run, is brought into play. It is true that many dwarf farms are below optimum size and would benefit from a program of consolidation. But what is also true, and usually unrecognized, is that many of the larger farms (above 10 or 15 acres) are too large for optimum results, *given the prevailing technology and incentives.*

The potential effect of more intensive cultivation of the larger farms on total output can be realized by looking at the distribution of land holdings: farms of 10 acres and over constitute about two-thirds of the total farm land and are operated by 15 per cent of the cultivators [29, p. 18], but these large farms yield much less per acre than the smaller holdings. In the five states cited in Table 1, the small and medium farms averaged about one-third more output per acre than the farms above 10 or 15 acres. Furthermore, in Madras and West Bengal, the farms in the 2.5- to 10-acre groups averaged much higher rent and profit per acre than the farms in the size-groups of 10 acres

and over, while in Madhya Pradesh the farms under 5 acres earned the highest rent and profit per acre of all the farms in that state. The large farms constitute a source of unexploited land resources that can eventually be used (along with unused irrigation water) to expand agricultural output and provide additional employment. Currently, shifts of labor from the very small farms below 2.5 acres to farms of over 10 acres (e.g., in Madras) would result in higher total output. Increasing returns to labor are still present in some areas of agriculture. The Indian government recognizes this and is attempting to transfer population to parts of Madhya Pradesh and Mysore in order to foster economic development—a policy which would be difficult to explain in terms of most of the development models usually applied to India.[12] It is equally difficult to reconcile the above findings with the often-repeated remark that Indian agriculture is so oversaturated with labor that the marginal productivity of additional workers must perforce be zero.[13]

How far can increased intensity of cultivation go as an offset to the growth of the agricultural work-force? Will a further reduction in the size of holdings, brought on by population growth, necessarily be critical? If Japan, which has poor soils, small fragmented holdings, and a peasant agriculture, is compared with India, we get a rough idea of what *might* be feasible in India. The average-size holding in Japan is 2 acres compared with India's 5.4 acres, and Japan has far less cultivated land per farm worker. Yet Japan obtains higher yields per man, and three to four times as much output per acre (in wheat and rice) as India.[14] The comparison serves to point up the fact that India's land resources need not be considered the main barrier to increased agricultural employment.

So far, the analysis of cross-section data has thrown considerable doubt on the assumption of surplus labor in Indian agriculture (in terms of man-years). However, it is reasonable to ask what are the employment possibilities in agriculture, given the expected growth in the population and labor force. Although the projection of specific labor requirements in agriculture will be the main theme of a subsequent paper, it is appropriate here to examine briefly the expected trend of agricultural employment and contrast this with a number of current development models.

IV. *Agriculture's Share of the Labor Force*

So much intellectual capital has been invested in models which postulate surplus labor in agriculture available without social cost to the industrial sector [8] [35] [45] [46] that the growing evidence against this hypothesis has

[12] See Kusum Nair [40, pp. 50-51] for a discussion of this point. Other Indian sources agree: "There are states like Madhya Pradesh which need an influx of manpower for the development of agriculture and industry . . ." [1, p. 117].

[13] Arthur Lewis (quoted in [5, p. 116]) states: "Indian economists estimate conservatively that a quarter of the rural population is surplus, in the sense that its removal from the land would make no difference to agricultural output. This is equivalent to having some 20 million people permanently unemployed." See also footnote 9, above.

[14] B. F. Johnston, in explaining the striking improvements in Japanese agriculture, also identified labor-intensive operations in preparatory tillage and interculture as strategic elements in producing higher yields [31, p. 229]. Comparative data on yields per acre are in [50], and on size of holdings in [28, pp. 51-52].

been largely neglected. Yet a good case can be made for the proposition that agriculture in the next decade will absorb and use productively almost its full share of the expected increase in the labor force.

From 1951 to 1961 the population of India increased by 21.5 per cent while the labor force employed in agriculture increased by 33 per cent, due to higher labor-force participation rates, especially of women [9, pp. xxi-xxxi].[15] (This latter increase may be partly definitional.) Despite a supposedly stagnant agriculture, an approximate 33 per cent increase in the labor force was accompanied by an increase in agricultural production of 46 per cent [28, p. 88]. For the decade, per capita agricultural output went up 20 per cent.

If one takes seriously the descriptions of huge surpluses of idle labor existing in Indian agriculture in the 'forties and 'fifties [5, pp. 116ff.], and standard explanations that rationalization and improvement of techniques will create no new jobs in agriculture,[16] and eliminate many old ones, it would seem quite difficult to explain the statistical data of the past decade—viz., how did Indian agriculture absorb 33 per cent more workers? Some important facts about the nature of Indian agriculture have not been adequately evaluated in much of the development literature. Table 6 suggests in part an explanation. It shows the labor requirements per acre for crops produced on irrigated and nonirrigated land. If we compare the first five crops produced under both irrigation and nonirrigation, then it can be seen that irrigation, and the improvements that go with it, increase labor inputs per acre 2.4 times. (The weighted average was based on the acreage devoted to each crop.) However, when irrigation is newly introduced in many areas, there is a shift from jowar and bajra (millets) to crops such as rice and sugar cane, which could not be produced with dry-farming techniques. If we include these crops as typically occupying a significant place in the cultivation pattern prevailing in irrigated areas, then the weighted average of labor requirements for crops grown under irrigation increases to 58.69 days per acre or over 3.6 times the average labor used on the nonirrigated land.[17] On the basis of similar data, a study of the

[15] Labor-force participation rates of women in industry have declined [9, pp. xxv-xxxi], owing perhaps to welfare legislation which makes their employment more expensive.

[16] Enke [7, p. 568] remarks: "Most investments in rural areas. . . do not create jobs. Many of the large investments that increase agricultural output . . . directly create employment in *non-rural* areas. . . . During 1961-66 existing underemployment will worsen, despite an estimated migration of almost one-and-a-half million job seekers from country to town, the increase in rural underemployed possibly being nine million." Enke fails to consider the many labor-intensive improvements which are currently so important in agricultural development.

[17] On an establishment basis the effects of irrigation are similarly striking: Irrigation per farm, and labor inputs per acre, in days, for sample holdings in Madras are as follows [23, p. 73]:

Farms	Labor (days per acre) Family	Total
No Irrigation	15.8	24.5
20%-30% Irrigated	35.9	47.2
45%-55% Irrigated	51.2	65.1
70%-80% Irrigated	78.3	99.2

effects of the Chambal Valley Irrigation Project [2] predicted regional labor shortages by 1970 if the expected natural rate of population increase, with no in-migration, occurs.[18]

V. *Implications for Development Models*

The data reviewed above throw doubt on the validity of the dual economy models which assume a free transfer of labor from agriculture to industry without loss of agricultural product. The evidence also negates those formulas which suggest a maximization of output via the maximization of the average productivity of capital, since these prescriptions typically assume that labor is so plentiful as to be redundant even after the most labor-intensive known production functions have been utilized [3, pp. 95 ff.]. Indian agriculture has a long way to go before arriving at this outer limit of the most labor-intensive production function. The Japanese experience and the Indian cross-section data indicate that Indian agriculture can, and probably will, move in the direction of a more labor-intensive production function for some time to come. Japanese rice-growers, for example, currently have a higher *proportion* of labor costs to total costs than the less developed, lower output Indian farms, and it is the Japanese paddy method and similar improvements that are being extended in India.

To point out the growing employment possibilities in Indian agriculture and to deny the surplus labor hypothesis are not, however, equivalent to advocating population growth, as Ranis and Fei seem to imply.[19] If the population continues to grow at the present rate, a larger labor force can, and probably will, be productively employed in agriculture, rather than being shunted largely to the cities. If, however, population growth is brought under rapid control, the trend toward more labor-intensive agriculture will make possible a greater improvement in per capita income. Labor will shift from the worst land (the extensive margin) to better land (the intensive margin). The poorest land will go out of cultivation as the returns to labor and capital on the intramarginal lands go up. Ricardian analysis will suffice to show that a more labor-intensive production function in agriculture need not be linked to a demand for population growth!

The "safety valve" reduction in the size of the large holdings which is beneficial in the short run, given the technical conditions and incentives prevailing in Indian agriculture, will perhaps make reorganization on the basis of mechanization and advanced technology eventually more difficult. The latter requires larger units, not smaller.[20] However, there is no evidence that India,

[18] These "shortages" assume no change in the structure of peasant agriculture in which a man and his family can cultivate only a few acres, nor does the author consider the possibility of the poorest land going out of crop production. The first assumption seems fairly realistic, but the second is clearly an alternative to the conclusion of a labor shortage.

[19] "If Oshima is right, what we really face here is a shortage of agriculture labor. . . . Under such assumptions . . . the correct policy conclusion is the encouragement of further population increases" [46, p. 453].

[20] Schultz [47, pp. 122-24] has cautioned against the assumption that effective mechanization necessarily requires very large units.

given her present resource-mix, is destined to take this road in the decades immediately ahead. Rather, the Japanese example of highly productive, small-unit peasant farming holds out the most promising prospect.

Finally, on the basic issue of the zero marginal product of labor and disguised unemployment, the evidence offered here supports the position taken by Viner [52], Schultz [47] [48], and Oshima [42], against the more generally accepted views of Lewis [35], Wonnacott [53], Ranis and Fei [45] [46], Eckaus [6], Coale and Hoover [5], Nurkse [41], and many others that Indian agriculture suffers from a redundant labor force with a zero marginal value product. The data indicate not only a positive marginal product of labor, but also lend support to the hypothesis that the rationalization and improvement of agricultural techniques, generated by development efforts, will exert a strong upward pull on the demand for agricultural labor.

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Lifetime Income and Economic Growth

The purpose of this article is to show that the usual procedure for estimating lifetime income based on cross-section surveys tends to produce underestimates because of the failure to take future growth into account. Estimates based on cohort analyses produce quite different results. Census data show that the differences in real income for a given age cohort in two successive decennial censuses (e.g., men 25 to 34 years old in 1950 and 35 to 44 years old in 1960) are far greater than those obtained for men in the same age groups at a given point in time. The main reason for the difference is that the income measures obtained ten years apart reflect economic growth which is entirely excluded from the cross-section surveys. The use of income averages by age based on the cross-section surveys therefore produces lower values than would be obtained by the use of averages based on successive censuses. Moreover, the impact of growth appears to be greater for young men than for those past the prime working years. This fact suggests an additional source of downward bias in the currently available estimates, since the discounting procedures used to convert estimated lifetime income to present values attach greater weight to incomes expected early in life than to those expected later on.

These conclusions are, of course, subject to the limitation that they are based on only two points in time. Several decennial censuses would be needed to verify whether the rates of change in average income obtained are stable over time. The rates for any one decade could be affected by factors such as differences in the size of the cohorts. Almost all of the labor force in 1950 and 1960 were born in an era of declining birth rates. Hence one might expect a tight labor supply in the younger age groups. This might be reversed by

70-80 when the younger cohorts will be far more numerous relative to older es.

I. *Shortcomings of Cross-Section Estimates*

The procedure generally used to estimate the present value of income reved during a working lifetime for a given subgroup in the population can summarized as follows [2, p. 47]:

$$V_{18} = \sum_{n=18}^{75} \frac{Y_n W_n P_n}{(1 + r)^{n-18}} .$$

In this formula Y_n is the average income at age n; W_n is the proportion of sons at age n with income; P_n is the probability of surviving at least one ar at age n; r is the discount rate; and n is the working lifetime span, here ined as 18-75 years. The income averages generally come from household veys conducted with a cross section of the population at a given point in ne. Since the average incomes based on these surveys are generally tabulated ly for age groups (e.g., 35-44 years) rather than for single years of age, it assumed that the average income for the entire age group applies to each of e single years of age within the group.

Perhaps the major shortcoming of the model is that it assumes no future reases in average income.[1] The model recognizes that each individual may pect his own income to rise as he gains in experience, seniority, and other tors that produce income differences among age groups at a given point in ne. It fails to recognize, however, that "in a growing economy every indiiual may expect an upward trend in his own earning superimposed on the oss-sectional pattern for a given year" [1, p. 27].

So long as the model is used as a standardization procedure with the age stribution of the population held constant, it can yield meaningful comparins among subgroups in the population. Very often, however, the results are eated as actual approximations of lifetime income rather than as statistical stractions. That is, the figures are used to suggest the actual amount of inme in current dollars, discounted to represent present values, that might sult from a given investment. In such cases, the figures are interpreted as if

[1] This model has several other shortcomings which are not discussed in the present port. First, it assumes that there will be no future increases in life expectancy during e working years. This is a rather doubtful assumption in the light of historical evince, but it is probably not critical because the likelihood of surviving an additional ar during the prime ages of working life is already quite high. More serious is the fact at it assumes the same discount rate for all socioeconomic groups. This procedure may valid if the purpose is to provide a single estimate of lifetime income from an over-all andpoint. In that case, we would wish to use the rate that would best reveal the present lue from a single standpoint. If, however, the purpose is to show the estimate that dividuals or particular groups may be considering when they make their decisions, fferent discount rates for different socioeconomic groups may be appropriate. Studies of verty show that one of the major problems low-income families have is their inability plan for the future, to recognize the future implications of present actions. It is very kely, for example, that college graduates discount the future at a far lower rate than gh school graduates. If this is the case, the present value of a future stream of income likely to be far higher for them than for the high school group.

they really represent the best estimate of the present value of the stream of income that a given subgroup might expect to receive over a lifetime. Under these circumstances, a case can be made for including the returns associated with economic growth as part of the total because they represent income that the individual may realistically expect to receive. The fact that their receipt is independent of any action on his part is unimportant in this case because the objective is to measure the returns *associated* with the investment, and not only those *caused* by the investment.

Under other circumstances, however, the decision as to whether or not to include such returns does become more complicated. This is particularly true when returns to different types of investments are being considered or when comparisons are made between different groups. For example, in one recent study the cost of preventing school dropouts was compared with the estimated amount by which the lifetime income of school dropouts might be increased if they were induced to stay in school [3]. In this kind of application it could be argued that returns to lifetime income associated with growth should not be included unless it can be demonstrated that these returns are *caused* by education or that they accrue differentially to the various education groups and therefore affect comparisons that may be made among them.[2] If the income measure associated with education were free of all such extraneous factors as differences in innate ability, quality of schooling and discrimination, it would be reasonable to exclude income gains associated with economic growth. But, if the income figure for each education group shows only the *association* between income and education, with other factors included, there is little reason to exclude income associated with economic growth. Where global comparisons are made and the growth factor is omitted, the results are misleading because the estimating procedure understates the present value of lifetime income.

II. *Differential Impact of Economic Growth on Age Groups*

Empirical evidence based on the 1950 and 1960 Censuses, which is presented below, suggests that the relative increases in income associated with economic growth appear to be greater in the early years of working life than they are once the age of peak income is passed. Since the procedure generally used to estimate lifetime income counts income received early in life more nearly at its full value than income received in the later years, failure to include increases due to growth provides an additional source of downward bias in the estimates. Caution must be exercised in interpreting the data because they are based on experience during a single decade. Nevertheless, the figures do suggest that all groups do not share equally in economic growth and that the young tend to benefit more than others. The reasons why this should be so are not entirely clear. However, young workers are more mobile

[2] Table 2 suggests that economic growth affects higher education groups more than the lower ones; but, even if all education groups were equally affected by economic growth, we might still want to include the contribution caused by growth in those cases where the cost of investing in an additional year of education is compared with the amount of return associated with that investment.

than others in the labor force. They are not as much tied down by family responsibility, home ownership, seniority, and other factors which reduce mobility. Therefore, they are more likely to move into new areas and new industries where wages tend to be higher and opportunities for rapid advancement are greater. Conversely, having been in the labor force a relatively short time, they are less likely to be trapped in declining industries where incomes may be dropping despite the over-all growth in the economy. The newer entrants into the labor force may also be better trained than those who have been employed for a decade or more, having been exposed to more modern methods of education and training. It is also possible that employers prefer younger workers even when they have the same ability as older workers because of their interest in potentials for growth. For these and other reasons, the evidence regarding the differential impact of economic growth for different age groups, based on the experience during the 'fifties, seems reasonable. In view of this fact, it might be reasonable to modify the estimating procedure described at the outset along the following lines:

$$V_{18} = \sum_{n=18}^{45} \frac{Y_n W_n P_{n(1+x)}}{(1+r)^{n-18}} + \sum_{n=46}^{75} \frac{Y_n W_n P_{n(1+y)}}{(1+r)^{n-18}} .$$

The symbols are used here with the same meaning they had at the outset with the following exception. Since gains in income associated with economic growth appear to be greater up to age 45 than they are later on (see Table 1) the estimating procedure is broken into two parts, one representing incomes received till age 45, and the other, incomes received from age 45 to 75. The growth factors x and y represent the gains in income associated with economic growth for age groups 18-45 and 46-75.

As previously noted, the model assumes that all relationships which exist at the time the estimates are made will remain unchanged in the future. If, for example, the supply of college graduates is increased more rapidly than the demand for them, their incomes relative to others might be changed and, as a result, their expected lifetime incomes will change. Changes in technology, tastes, international relations, and many other factors could alter the results. Since these potential changes cannot be predicted, it is assumed that, on balance, their effect is neutral.

III. *Graphic Presentation of Cohort and Cross-Section Data*

Some of the basic issues involved in this paper can perhaps best be demonstrated by use of a numerical example. If the lifetime income of college graduates were estimated on the basis of the experience of 1949, using the formula described at the outset, $4,891 would have been used as the average for the 25-34 year age group and $8,595 would have been used for the 35-44 year group (see Table 1). These values are based on the cross-section data on income received during 1949 that were collected in the 1950 Census. They represent differences in income associated only with age (i.e., experience, seniority, and similar factors) and are independent of changes in the economy over time. In other words, on the basis of the cross-section data for 1949 that were

TABLE 1—CHANGE IN MEAN INCOME FOR SELECTED COHORTS OF MALES, BY YEARS OF SCHOOL COMPLETED, COLOR, AND REGION, FOR THE UNITED STATES: 1949 AND 1959

(Mean income in 1959 dollars. The Consumer Price Index was used as the price deflator. Figures shown for 1950 represent income in 1949; figures shown for 1960 represent income in 1959.)

Color, region, and years of school completed.	Cohort of 1915-1924			Cohort of 1905-1914			Cohort of 1895-1904			Cohort of 1885-1895		
	25-34 in 1950	35-44 in 1960	Percent Change	35-44 in 1950	45-54 in 1960	Percent Change	45-54 in 1950	55-64 in 1960	Percent Change	55-64 in 1950	65-74 in 1960	Percent Change
United States												
All Classes												
Total	$3,556	$ 6,212	75	$4,396	$ 6,136	40	$4,540	$ 5,522	22	$4,008	$3,415	-15
Elem: Less than 8 years	2,295	3,564	55	2,719	3,625	33	2,938	3,501	19	2,776	2,202	-21
8 years	3,077	4,705	53	3,660	4,854	33	3,925	4,644	18	3,662	2,967	-19
High School: 1-3 years	3,440	5,465	59	4,154	5,681	37	4,535	5,614	24	4,268	3,965	- 7
4 years	3,920	6,379	63	4,868	6,697	38	5,702	6,746	18	5,531	4,527	-18
College: 1-3 years	4,121	7,654	91	6,035	8,679	44	6,852	8,817	29	6,277	5,386	-14
4 years or more	4,891	11,088	127	8,595	11,590	35	9,853	11,039	12	9,292	8,123	-13
White												
Total	3,705	6,508	76	4,627	6,449	39	4,782	5,802	21	4,172	3,571	-14
Elem: Less than 8 years	2,527	3,918	55	3,011	3,961	32	3,218	3,781	17	2,959	2,327	-21
8 years	3,154	4,845	54	3,738	4,958	33	3,986	4,718	18	3,699	2,994	-19
High School: 1-3 years	3,537	5,650	60	4,254	5,820	37	4,627	5,729	24	4,326	4,035	- 7
4 years	3,971	6,494	64	4,939	6,807	38	5,794	6,867	19	5,611	4,602	-18
College: 1-3 years	4,181	8,023	92	6,140	8,835	44	6,976	9,001	29	6,370	5,463	-14
4 years or more	4,939	11,263	128	8,716	11,747	35	9,992	11,181	12	9,411	8,226	-13
Nonwhite												
Total	2,123	3,465	63	2,240	3,113	39	2,157	2,674	24	1,854	1,650	-11
Elem: Less than 8 years	1,730	2,590	50	1,887	2,539	35	1,869	2,272	22	1,609	1,431	-11
8 years	2,244	3,416	52	2,449	3,358	37	2,522	3,126	24	2,334	2,084	-11
High School: 1-3 years	2,384	3,728	56	2,644	3,618	37	2,700	3,357	24	2,402	2,243	- 7
4 years	2,710	4,299	59	2,998	4,090	36	2,983	3,753	26	2,707	2,466	- 9
College: 1-3 years	2,732	4,625	69	3,289	4,527	38	3,273	4,114	26	3,046	2,624	-14
4 years or more	3,133	6,366	103	4,371	6,803	56	4,613	6,297	37	3,985	4,850	+22
North and West												
All Classes												
Total	3,786	6,603	74	4,748	6,606	39	4,886	5,926	21	4,281	3,627	-15
Elem: Less than 8 years	2,828	4,246	50	3,321	4,321	30	3,459	4,066	18	3,166	2,465	-22
8 years	3,235	4,920	52	3,813	5,054	33	4,060	4,814	19	3,761	3,056	-19
High School: 1-3 years	3,567	5,690	60	4,303	5,935	38	4,675	5,859	25	4,423	4,083	- 8
4 years	3,965	6,520	64	4,893	6,818	39	5,740	6,832	19	5,577	5,069	- 9
College: 1-3 years	4,168	8,023	92	6,074	8,823	45	6,886	8,901	29	6,338	5,490	-13
4 years or more	4,869	11,395	134	8,702	11,851	36	9,997	11,309	13	9,435	8,322	-12

Table 1 (continued)

Color, region, and years of school completed.	Cohort of 1915-1924 25-34 in 1950	35-44 in 1960	Percent Change	Cohort of 1905-1914 35-44 in 1950	45-54 in 1960	Percent Change	Cohort of 1895-1904 45-54 in 1950	55-64 in 1960	Percent Change	Cohort of 1885-1895 55-64 in 1950	65-74 in 1960	Percent Change
North and West (cont'd.)												
White												
Total	$3,848	$ 6,776	76	$4,853	$ 6,774	40	$ 4,993	$ 6,072	22	$4,342	$3,693	-15
Elem: Less than 8 years	2,905	4,409	52	3,440	4,476	30	3,570	4,184	17	3,220	2,501	-22
8 years	3,272	5,004	53	3,856	5,117	33	4,097	4,861	19	3,782	3,072	-19
High School: 1-3 years	3,628	5,817	60	4,371	6,034	38	4,736	5,943	25	4,468	4,133	- 7
4 years	4,003	6,609	65	4,947	6,902	40	5,811	6,932	19	5,643	5,154	- 9
College: 1-3 years	4,205	8,169	94	6,154	8,942	45	6,980	9,056	30	6,398	5,545	-13
4 years or more	4,898	11,530	135	8,768	11,940	36	10,083	11,409	13	9,503	8,390	-12
Nonwhite												
Total	2,711	4,309	59	2,921	4,002	37	2,834	3,531	25	2,589	2,216	-14
Elem: Less than 8 years	2,460	3,548	44	2,645	3,494	32	2,547	3,159	24	2,330	1,985	-15
8 years	2,633	3,905	48	2,832	3,849	36	2,866	3,556	24	2,743	2,362	-14
High School: 1-3 years	2,695	4,242	57	2,966	4,116	39	3,066	3,885	27	2,732	2,582	- 5
4 years	2,932	4,731	61	3,217	4,530	41	3,261	4,151	27	2,932	2,660	- 9
College: 1-3 years	3,060	5,047	65	3,494	5,117	46	3,608	4,569	27	3,581	2,797	-22
4 years or more	3,355	7,061	110	4,975	8,161	64	4,935	7,142	45	4,514	5,147	+14
South												
All Classes												
Total	3,033	5,255	73	3,573	4,978	39	3,639	4,434	22	3,189	2,641	-17
Elem: Less than 8 years	1,970	2,991	52	2,208	2,869	30	2,236	2,598	16	2,009	1,696	-16
8 years	2,634	4,042	53	3,081	4,085	33	3,290	3,868	18	3,081	2,581	-16
High School: 1-3 years	3,132	4,825	54	3,750	4,917	31	4,132	4,843	17	3,721	3,618	- 3
4 years	3,737	5,901	58	4,776	6,281	32	5,541	6,455	16	5,331	4,275	-20
College: 1-3 years	3,986	7,372	85	5,922	8,246	39	6,750	8,567	27	6,109	5,114	-16
4 years or more	4,956	10,270	107	8,086	12,504	55	9,349	10,191	9	8,799	7,577	-14
White												
Total	3,327	5,778	74	4,011	5,535	38	4,126	4,973	21	3,577	2,934	-18
Elem: Less than 8 years	2,229	3,418	53	2,532	3,266	29	2,575	2,970	15	2,285	1,899	-17
8 years	2,786	4,304	54	3,248	4,296	32	3,421	4,022	18	3,184	3,218	+ 1
High School: 1-3 years	3,300	5,141	56	3,919	5,143	31	4,296	5,033	17	3,844	3,738	- 3
4 years	3,841	6,096	59	4,917	6,475	32	5,719	6,649	16	5,470	4,383	-20
College: 1-3 years	4,109	7,602	85	6,095	8,507	40	6,964	8,837	27	6,292	5,244	-17
4 years or more	5,063	10,547	108	8,370	12,973	55	9,660	10,445	8	9,088	7,760	-15
Nonwhite												
Total	1,725	2,585	50	1,802	2,349	30	1,720	1,930	12	1,435	1,305	- 9
Elem: Less than 8 years	1,516	2,132	41	1,614	2,031	26	1,575	1,732	10	1,319	1,163	-12
8 years	1,858	2,699	45	1,979	2,613	32	1,992	2,305	16	1,697	1,692	- 2
High School: 1-3 years	2,003	2,900	45	2,212	2,819	27	2,187	2,441	12	2,006	1,764	-12
4 years	2,241	3,304	47	2,549	3,188	25	2,364	2,772	17	2,141	2,054	- 4
College: 1-3 years	2,248	3,657	63	2,951	3,512	19	2,724	3,188	17	2,386	2,437	+ 2
4 years or more	2,900	5,313	83	3,748	5,586	49	4,287	5,273	23	3,609	3,979	+10

Source: Unpublished data of the Bureau of the Census.

collected in the 1950 Census it could be said that the increase in average income between age 30 (the average for 25-34) and age 40 was $3,704, representing a gain of 76 per cent for the decade or 7.6 per cent per year. The variation of income with age based on cross-section data has been measured annually during the past 15 years and has been found to be quite stable. The relationship does not appear to be affected in any appreciable way by cyclical changes in the level of economic activity. Therefore it could be deduced that the relationship between income and age reflected in the cross-section data provides a measure of the extent to which experience, seniority, loss of work due to illness, and similar factors associated with age have an impact on individual income.

An alternative way of estimating the income change between ages 30 and 40 is to compare the incomes (measured in constant dollars) of college graduates 25-34 years old in 1949 ($4,891) with those of college graduates 35-44 years old in 1959 ($11,088). This procedure provides a more valid measure of the change in income over time than the one described above because it permits the comparison of average incomes for the same group of men at two different points in time whereas the cross-section data permit only a comparison of two different groups of men at the same point in time. On the basis of the cohort approach, it could be said that the average income of college graduates increased by $6,197, representing a gain of 127 per cent for the decade or about 12.7 per cent per year. This increase consists of two separately identifiable elements. One of these elements is the increase in income due to experience, seniority, and other factors associated with age. On the basis of the cross-section data previously described, it might be roughly estimated that about 7.6 percentage points of the total increase for this age group is due to the variation of income with age. This factor, incidentally, is taken into account in the traditional method of estimating lifetime income because it is reflected in the averages used in the formula described at the outset. There is, however, a second component of 5.1 per cent per year which is not taken into account in the traditional method of estimating lifetime income. This component represents the increase in average income over time for this cohort due to changes in productivity, in the industrial and occupational mix of the labor force, in the geographic distribution of the labor force, and similar factors that are associated with changes in the economy as a whole.

A visual presentation of the problem discussed above is presented in Figure 1. According to the formulation described at the outset, the values used to estimate lifetime income are based on the cross-section data shown at points *A* ($8,595) and *C* ($9,853). The difference between these two points represents the variation due to age (i.e., experience, seniority, etc.) that the economy is willing to pay for a given resource (college graduates) at a given point in time (1959). Another way of looking at this problem is to see how much the economy is willing to pay for the same resource at two different points in time. This information is provided by points *A* ($8,595) and *B* ($11,590). If it is assumed that the conditions of supply were unchanged between 1949 and 1959, it can be concluded on the basis of this evidence that because of the increased experience of this group of college graduates *and* because of the

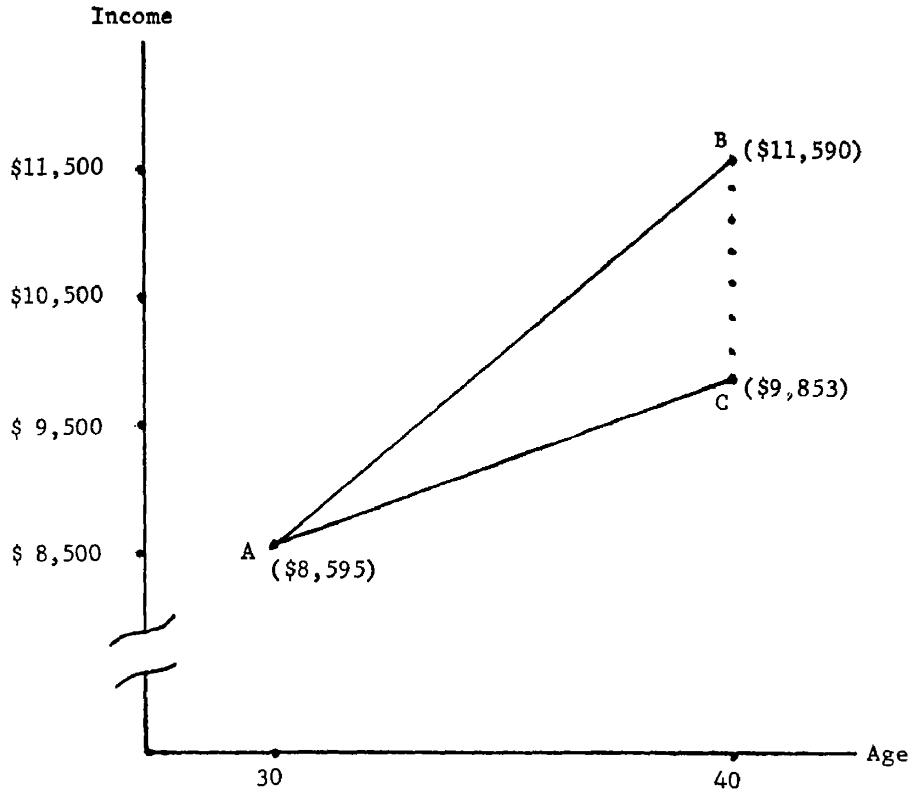


FIGURE 1

change in the entire economy during the decade, the annual income for this group increased from $8,595 to $11,590. The difference between points *A* and *B* ($2,995) represents the *total change* due to both the increase in experience and the growth in the economy, whereas the difference between points *A* and *C* ($1,258) represents the increase due to age alone. Therefore, the difference between points *B* and *C* ($1,737) represents the increment associated with economic growth. The age component of the increase is included in the current techniques for estimating lifetime income. The component identified as being associated with economic growth is not included.

IV. *Summary of Components of Income Change by Age Groups*

A summary of the components of change in income for various subgroups in the population is shown in Table 2. The key factor to note here is that for each color and education group, the greatest gains in income are experienced by the younger age groups. Thus, for example, to continue with the illustration of college graduates previously used, it can be noted that the cohort of men born between 1915-24 (i.e., those who were 25-34 years old in 1950 and 35-44 years old in 1960) had annual increases in income of 12.7 per cent, whereas men who were 10 years older had annual increases of only 3.5 per

TABLE 2—COMPONENTS OF CHANGE IN MEAN INCOME FOR SELECTED COHORTS OF MALES, BY COLOR AND YEARS OF SCHOOL COMPLETED, FOR THE UNITED STATES, BY REGION: 1949 AND 1959

Color, Region, and years of school completed.	Annual rate of increase in income between age 25-34 and age 35-44			Annual rate of increase in income between age 35-44 and age 45-54			Annual rate of increase in income between age 45-54 and 55-64		
	Based on cross-section data (change assoc. with age) (1)	Based on cohort data (total change over the decade) (2)	Difference (2) - (1) (change due to economic growth) (3)	Based on cross-section data (change assoc. with age) (1)	Based on cohort data (total change over the decade) (2)	Difference (2) - (1) (change due to economic growth) (3)	Based on cross-section data (change assoc. with age) (1)	Based on cohort data (total change over the decade) (2)	Difference (2) - (1) (change due to economic growth) (3)
United States									
All Classes									
Total	2.4	7.5	5.1	0.3	4.0	3.7	-1.3	2.2	3.5
Elem: Less than 8 years	1.8	5.5	3.7	0.8	3.3	2.5	-0.6	1.9	2.5
8 years	1.9	5.3	3.4	0.7	3.3	2.6	-0.7	1.8	2.5
High School: 1-3 years	2.1	5.9	3.8	0.9	3.7	2.8	-0.6	2.4	3.0
4 years	2.4	6.3	3.9	1.7	3.8	2.1	-0.3	1.8	2.1
College: 1-3 years	4.6	9.1	4.5	1.4	4.4	3.0	-0.9	2.9	3.8
4 years or more	7.6	12.7	5.1	1.5	3.5	2.0	-0.6	1.2	1.8
White									
Total	2.5	7.6	5.1	0.3	3.9	3.6	-1.5	2.1	3.6
Elem: Less than 8 years	1.9	5.5	3.6	0.7	3.2	2.5	-0.9	1.7	2.6
8 years	1.9	5.4	3.5	0.7	3.3	2.6	-0.8	1.8	2.6
High School: 1-3 years	2.0	6.0	4.0	0.9	3.7	2.8	-0.7	2.4	3.1
4 years	2.4	6.4	4.0	1.7	3.8	2.1	-0.3	1.9	2.2
College: 1-3 years	4.7	9.2	4.5	1.4	4.4	3.0	-1.0	2.9	3.9
4 years or more	7.6	12.8	5.2	1.5	3.5	2.0	-0.6	1.2	1.8
Nonwhite									
Total	0.6	6.3	5.7	-0.4	3.9	4.3	-1.6	2.4	4.0
Elem: Less than 8 years	0.9	5.0	4.1	-0.1	3.5	3.6	-1.6	2.2	3.8
8 years	0.9	5.2	4.3	0.3	3.7	3.4	-0.8	2.4	3.2
High School: 1-3 years	1.1	5.6	4.5	0.2	3.7	3.5	-1.2	2.4	3.6
4 years	1.1	5.9	4.8	-	3.6	3.6	-1.0	2.6	3.6
College: 1-3 years	2.0	6.9	4.9	-	3.8	3.8	-0.7	2.6	3.3
4 years or more	4.0	10.3	6.3	0.6	5.6	5.0	-1.6	3.7	5.3
North and West									
All Classes									
Total	2.5	7.4	4.9	0.3	3.9	3.6	-1.4	2.1	3.6
Elem: Less than 8 years	1.7	5.0	3.3	0.4	3.0	2.6	-0.9	1.8	2.7
8 years	1.8	5.2	3.4	0.6	3.3	2.7	-0.8	1.9	2.7
High School: 1-3 years	2.1	6.0	3.9	0.9	3.8	2.9	-0.6	2.5	3.1
4 years	2.3	6.4	4.1	1.7	3.9	2.2	-0.3	1.9	2.2
College: 1-3 years	4.6	9.2	4.6	1.3	4.5	3.2	-0.9	2.9	3.8
4 years or more	5.5	13.4	5.5	1.5	3.6	2.1	-0.6	1.3	1.9

Table 2 (continued)

Color, Region, and years of school completed.	Annual rate of increase in income between age 25-34 and age 35-44			Annual rate of increase in income between age 35-44 and age 45-54			Annual rate of increase in income between age 45-54 and 55-64		
	Based on cross-section data (change assoc. with age) (1)	Based on cohort data (total change over the decade) (2)	Difference (2) - (1) (change due to economic growth) (3)	Based on cross-section data (change assoc. with age) (1)	Based on cohort data (total change over the decade) (2)	Difference (2) - (1) (change due to economic growth) (3)	Based on cross-section data (change assoc. with age) (1)	Based on cohort data (total change over the decade) (2)	Difference (2) - (1) (change due to economic growth) (3)
White									
Total	2.6	7.6	5.0	0.3	4.0	3.7	-1.5	2.2	3.7
Elem: Less than 8 years	1.8	5.2	3.4	0.4	3.0	2.6	-1.1	1.7	2.8
8 years	1.8	5.3	3.5	0.6	3.3	2.7	-0.8	1.9	2.7
High School: 1-3 years	2.0	6.0	4.0	0.8	3.8	3.0	-0.6	2.5	3.1
4 years	2.4	6.5	4.1	1.7	4.0	2.3	-0.3	1.9	2.2
College: 1-3 years	4.6	9.4	4.8	1.3	4.5	3.2	-0.9	3.0	3.9
4 years or more	7.9	13.5	5.6	1.5	3.6	2.1	-0.6	1.3	1.9
Nonwhite									
Total	0.8	5.9	5.1	-0.3	3.7	4.0	-0.9	2.5	3.4
Elem: Less than 8 years	0.8	4.4	3.6	-0.4	3.2	3.6	-0.9	2.4	3.3
8 years	0.8	4.8	4.0	0.1	3.6	3.5	-0.4	2.4	2.8
High School: 1-3 years	1.0	5.7	4.7	0.3	3.9	3.6	-1.2	2.7	3.9
4 years	1.0	6.1	5.1	0.1	4.1	4.0	-1.1	2.7	3.8
College: 1-3 years	1.4	6.5	5.1	0.3	4.6	4.3	-0.1	2.7	2.8
4 years or more	4.8	11.0	6.2	-0.1	6.4	6.5	-0.9	4.5	5.4
South									
All Classes									
Total	1.8	7.3	5.8	0.2	3.9	3.7	-1.4	2.2	3.6
Elem: Less than 8 years	1.2	5.2	4.0	0.1	3.0	2.9	-1.1	1.6	2.7
8 years	1.7	5.3	3.6	0.7	3.3	2.6	-0.7	1.8	2.5
High School: 1-3 years	2.0	5.4	3.4	1.0	3.1	2.1	-1.1	1.7	2.8
4 years	2.8	5.8	3.0	1.6	3.2	1.6	-0.4	1.6	2.0
College: 1-3 years	4.9	8.5	3.6	1.4	3.9	2.5	-1.0	2.7	3.7
4 years or more	6.3	10.7	4.4	1.6	5.5	3.9	-0.6	0.9	1.5
White									
Total	2.1	7.4	5.3	0.3	3.8	3.5	-1.5	2.1	3.6
Elem: Less than 8 years	1.4	5.3	3.9	0.2	2.9	2.7	-1.3	1.5	2.8
8 years	1.7	5.4	3.7	0.5	3.2	2.7	-0.7	1.8	2.5
High School: 1-3 years	1.9	5.6	3.7	1.0	3.1	2.1	-1.2	1.7	2.9
4 years	2.8	5.9	3.1	1.6	3.2	1.6	-0.5	1.6	2.1
College: 1-3 years	4.8	8.5	3.7	1.4	4.0	2.6	-1.1	2.7	3.8
4 years or more	6.5	10.8	4.3	1.5	5.5	4.0	-0.6	0.8	1.4
Nonwhite									
Total	0.4	5.0	4.6	-0.5	3.0	3.5	-2.0	1.2	3.2
Elem: Less than 8 years	0.6	4.1	3.5	-0.2	2.6	2.8	-1.9	1.0	2.9
8 years	0.7	4.5	3.8	0.1	3.2	3.1	-1.7	1.6	3.3
High School: 1-3 years	1.0	4.5	3.5	-0.1	2.7	2.8	-0.9	1.2	2.1
4 years	1.4	4.7	3.3	-0.8	2.5	3.3	-1.0	1.7	2.7
College: 1-3 years	3.1	6.3	3.2	-0.8	1.9	2.7	-1.4	1.7	3.1
4 years or more	2.9	8.3	5.4	1.4	4.9	3.5	-1.9	2.3	4.2

cent during the same period; and those who were 20 years older had annual gains of only 1.2 per cent. If, instead of using cohorts, cross-section data had been used, they would have revealed that differences associated with age accounted for an annual increase of 7.6 per cent between the average ages of 30 and 40; 1.5 per cent increase between the average ages of 40 and 50; and a decrease of about 0.6 per cent between ages 50 and 60. When the age component of the total increase is taken into account, it appears that economic growth accounted for a 5.1 per cent annual increase in income between ages 30 and 40; a 2.0 per cent increase between ages 40 and 50; and a 1.8 per cent increase between ages 50 and 60. Since the latter element is not taken into account in traditional measures of estimating lifetime income, it appears that its inclusion would add to the expected income gains of younger men and would therefore have an important bearing on the estimates of expected lifetime income.

A second point of interest in Table 2 is the fact that income gains associated with economic growth appear to be somewhat greater for nonwhites than for whites. Over all, the income gains associated with growth were 5.7 per cent per year for nonwhites as compared with 5.1 per cent for whites between the ages of 30 and 40. The corresponding differences between ages 40 and 50 were 4.3 per cent for nonwhites and 3.6 per cent for whites; and between ages 50 and 60 the gains were 4.0 per cent for nonwhites and 3.6 per cent for whites. Most of these differences are eliminated, however, when the figures are examined separately for the North and West and for the South, suggesting, therefore, that the apparently greater gains for nonwhites associated with economic growth are largely due to their migration from the South.

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A Note on Textbook Pricing

Economists have long been active in writing textbooks. Publishers have had much experience in publishing works on economic theory. Yet there seems to be a gap between the theory inside the books and the policies used in selling them. The purpose of this note is to suggest a change in pricing policy for

textbooks that may lead to larger profits for publishers, larger royalties for authors, and lower textbook costs for students. It is suggested that relating price to the age of the book may accomplish these objectives.

It is well-known economic theory that the profit-maximizing price for a product with no close substitutes is higher than the profit-maximizing price for that same product when close substitutes are available.

It is a well-known fact of the publishing business that the fixed costs (editorial work, typesetting, etc.) of producing a single copy of a book are large compared to the marginal costs of printing additional copies of the book. That is, it may cost $20,000 to set up production of a book which could then be printed at a marginal cost of $1.00 per copy.

What is not well known in the publishing business is the elasticity of demand for textbooks. Publishers seem to operate on assumption that potential adopters of a textbook are faced with a choice among many books which are only moderately differentiated from one another. The price of a new textbook must be somewhere in the neighborhood of competing books because the instructor may be reluctant to require his students to purchase a book costing substantially more than other texts which are similar in content and quality. While a difference in price of, say, 25 cents or 50 cents is not likely to be a significant factor in the decision of an instructor as to which text to adopt, a difference of $3 to $4 would probably be given heavy weight. No publisher, regardless of how good he thinks his book is, is likely to price a new *Principles* text at, say, $12.00 when existing books are selling for $8 to $9. Once the publisher determines the price he will charge for his new book,[1] under present practices, the price generally remains the same throughout the life of the edition.

There appear to be advantages in reducing prices as the book gets older. Instead of pricing a book at $8.00 when it is published, and maintaining that price regardless of changes in demand, the book could be priced at, say, $10.00 when published, $8.00 a year later, $7.00 the following year, and $6.00 in subsequent years.

Once an instructor has adopted a new book, his students have little choice but to buy the book, regardless of price. While demand is not completely inelastic—two or more students may share a book, a student may rely on a library, and I have seen students whose examinations provide no indication that the text was ever looked at at all—nevertheless, it is clear that the number of

[1] It is not clear to me exactly how prices are determined but certain rules of thumb seem to play a large role. Publishers are reluctant to carry inventories, and thus a first printing is usually no larger than one year's estimated sales. It seems to be virtually an article of faith that each printing (including the first) should pay its own way. A price must therefore be chosen which will be roughly in line with that of existing books and which, when multiplied by estimated first-year sales, will more than cover the full costs of producing the books. Since other publishers have followed the same rules, this is usually possible. If this cannot be done because of small expected sales, the book will not be published. If this cannot be done because of unusually high costs (a book with large numbers of tables, illustrations, or complex mathematical symbols), an attempt may be made to reduce costs by cutting material (if the author is agreeable) or printing the book where production costs are lower (England or Japan).

books sold will be quite close to the number of students in classes for which the book has been adopted as the text.

The preceding paragraph is correct only for the first year of a book's life. By the second year an excellent substitute for the book is available—a used copy. While instructors will give no more (or less) regard to price in the second year, students will have a choice between purchase of a new copy of the book or a used one. Clearly the elasticity of demand is higher in the second year than in the first year, the level of demand for new books is lower, and profit-maximization thus calls for a lower price in the second year than in the first.

Publishers are fully aware of the impact of the used-book market on the sale of new books. The used-book market is an efficient, well-developed one, with books being transferred rapidly from one section of the country to take advantage of changes in adoptions (shipping costs for books are low as a matter of social policy). Given the prices of new books, the used-book market provides a socially useful function in allowing students to minimize book costs.[2] Many textbooks (perhaps the majority) have their largest sales in their first year. In subsequent years the number of schools adopting the book may increase but part of this demand is satisfied by used copies.

To meet the competition from used books, publishers resort to frequent revisions of their books.[3] With the appearance of a new edition, all used copies of the old edition become immediately obsolete and virtually worthless in the used-book market. Even a cursory examination of sequential editions of popular textbooks reveals that many books are revised more out of concern for the competition of used books than because the author has discovered a new theory of comparative advantage or law of supply and demand.[4]

The suggested policy of reducing prices as the age of a book increases will have effects on both the supply of used books and the demand for them. Let us look first at the demand side. We have noted that the student's choice between new and used books is likely to be highly price-sensitive. The student who may choose a used copy at $6.00 in preference to a new one at $8.00 may prefer a new one at $7.00 to a used one at $5.50.

The modest price sensitivity of the instructor will also be brought into

[2] Economists would not agree with the statement made to me by an editor of a large publishing house that "used book dealers are parasites, creating nothing useful, who should be put out of business."

[3] I do not mean to imply that this is the only reason for revising textbooks. There is a need for bringing statistical data up to date. There is also a competitive problem in that, quality and price being equal, an instructor will tend to prefer the most recent book. It is possible, however, that a lower price for the older book may compensate for this disadvantage.

[4] The analogy with so-called "planned obsolescence" of automobiles is obvious. It should be stressed that I intend no criticism of motives or ethics. In a free-market economy authors, publishers, and automobile manufacturers are entitled to seek maximum profits. My point is that relating textbook prices to the age of the edition may result in larger profits with a reduction in the social waste of resources devoted to rearranging paragraphs or pages in a "revised edition." While the per-unit costs of frequent revisions are negligible for books selling tens of thousands of copies per year, most textbooks sell only a few thousand copies per year, and costs of revisions become substantial.

play. Given a choice between a new book at say, $10.00, and an equally good older book at, say $7.00, he may adopt the older book, whereas if the books were roughly the same price he would choose the newer.

The effect of this proposal on the supply of used books should also be substantial. The student's decision to retain or sell his books is highly price-sensitive. The price of a used book is directly related to the price of new copies. A hypothetical example may help here. Suppose used books are sold by dealers for 75 per cent of the new copy price and that the dealer mark-up on used books is 50 per cent. A book with a list price of $8.00 will be sold by used-book dealers for $6.00, and they will be willing to pay $4.00 for used copies of the book. The student who bought a new book for $8.00 must decide, at the end of the year, whether to retain it or sell it for $4.00. Consider the alternative pricing system whereby a new book is priced at $10.00 the first year, $8.00 the second year, and $7.00 the third year. The student who bought a new book for $10.00[5] must decide, at the end of the year, whether to retain it or sell it for $4.00. He is more likely to retain the book in the latter situation than in the former (even if he has read the chapter in the book on sunk costs). Consider, further, the student who bought a new copy the second year for $8.00. At the end of the year he will be able to sell this book for only $3.50,[6] and thus he is more likely to retain it than if it could be sold for $4.00.

The over-all effect of this technique, then, would be to encourage the sale of new rather than used copies of books, encourage the retention of books by students, and thus lengthen the life of textbook editions. Reducing the need for frequent revisions should reduce publisher's costs and allow a somewhat lower average price level for textbooks. The more uniform year-to-year sales pattern that would result should yield printing and inventory economies. Subsequent printings of a given edition now tend to become smaller in accord with the pattern of sales, despite the economies of larger printing runs. The lower publishing costs mean greater profits which would be shared to some extent with authors. Even apart from any additional royalties, authors would save time and effort from the reduced need to revise their books. Students would benefit from a lower average price level for books, and the fewer revisions would reduce the number of times a student would buy a new book and find it worthless on the used-book market at year-end because a new edition has come out. This gain to students should outweigh the loss to those students who would have to pay higher than present textbook prices for new copies of new books. It is possible that even the used-book dealers would benefit by the longer life of editions. Only the typesetters would seem to be losers from this proposal.

While the results of this policy would be in the direction I have indicated, it is possible that the magnitude of such changes would be small. If there are significant operational problems in instituting such a policy (need to give re-

[5] He had no choice since used copies were not available the first year.

[6] Since the price the following year for new copies will be $7.00, used-book dealers will only be able to get $5.25 for used copies and will be willing to pay only $3.50 for used copies.

bates to bookstores carrying inventory over to the next year, etc.), these costs may outweigh the possible gains. However, discussions with persons more knowledgeable than I in the mechanics of the publishing business have not turned up any insurmountable obstacles.

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"Neutral" Technological Change and the Isoquant Map

The concept of "neutrality" in technological change that has been found most useful in models of economic growth is that offered by Harrod [4, p. 23]. According to this view technological progress is neutral if it leaves the capital-output ratio unaltered at a given rate of profit. In principle this differs from the concept of neutrality offered by Hicks [5, p. 121], in which the capital-labor ratio remains unaltered at a constant ratio of factor prices.

Since each classification has been widely used, it is only natural that the connection between the two has been analyzed.[1] The basic result for one-sector models in which capital goods are produced is that Hicks and Harrod neutrality come to the same thing only if the elasticity of substitution between capital and labor is unity. The situation is slightly more complicated in multisector models, as the *value* of the capital-output ratio in sectors not producing the capital good depends in part on the price ratio between the output of that sector and machines.

The purpose of this article is to show how relationships involving Harrod neutrality can be illustrated and proved by use of the isoquant diagram. Although such a diagram offers a natural means of illustrating the Hicksian classification of technological change, it does not seem to have been used heretofore to discuss the Harrod definition.[2] It turns out to be surprisingly simple to employ the isoquant map to illustrate Harrod neutrality in single and multisector models and thus also to illustrate certain key relationships in a neoclassical model in which growth proceeds at a constant rate of profit.

As a preliminary step I demonstrate how it is possible to calculate the elasticity of substitution of an isoquant diagrammatically.[3] Consider Figure 1, where K and L represent quantities of capital and labor. The initial equilibrium is at A with the ratio of wages to rents given by the (absolute value of the) slope of line (1) and the capital-labor ratio by the slope of (3). In moving to point B the capital-labor ratio increases to the slope of line (4), while the

[1] Cf. Meade [10, Ch. 5], Uzawa [13], Kennedy [7], Asimakopulos [3], and Amano [1].

[2] Meade [10, p. 41] uses a diagram showing total output varying with capital, with a fixed quantity of labor. Joan Robinson [11] used the average and marginal product curves of capital in discussing the definitions. This is the kind of diagram also used by Asimakopulos [3]. In more recent work [12, p. 419], Mrs. Robinson employs a diagram showing output/man against the real capital ratio (capital in wage units). Kennedy [6, p. 293] and Asimakopulos and Weldon [2] have also used this diagram.

[3] An alternative representation is given by Lerner [8].

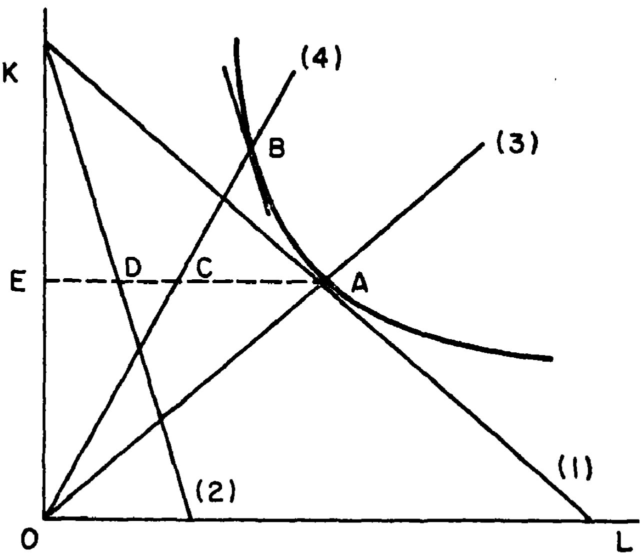


FIGURE 1

wage-rental ratio increases to the (absolute value of the) slope of the isoquant at B. Line (2) is drawn with the same slope. The percentage change (reduction) in the labor-capital ratio is AC/AE, while that in the rental-wage ratio is AD/AE. By definition the elasticity of substitution is the ratio of these two expressions, AC/AD. The isoquant in Figure 1 has elasticity of substitution less than unity.[4]

If production functions are linear homogeneous, as I assume, all the information about production is conveyed by the unit isoquant. Consider a one-sector economy producing machines (M). In Figure 2 the initial situation is shown at point A, where the unit isoquant is tangent to line DF, whose slope represents the ratio of factor prices (w/r). Distance OC represents a_{KM}—the amount of capital required to produce a unit of machines at point A. Similarly, the unit labor requirement at A, a_{LM}, is given by OB or CA. Since w/r equals DC/CA, DC is wa_{LM}/r. Therefore distance OD represents $1/r \cdot (wa_{LM} + ra_{KM})$ which, in a competitive model, is the inverse of the rate of profit, since the price of machines, p_M, is given by $(wa_{LM} + ra_{KM})$, the unit cost of production.[5]

Technological progress is represented by an inward shift of the unit isoquant. The movement from M to M' in Figure 2 represents Harrod-neutral progress, for at a constant rate of profit ($1/OD$) the capital-output ratio (OC) remains constant. Furthermore, the ratio AE/EC represents the im-

[4] For finite changes, as shown in the diagrams, assume the curves are of constant elasticity of substitution. Otherwise the graphical measure yields the arc elasticity of substitution.

[5] By a similar argument the horizontal intercept of the factor price line (OF) is the inverse of the real wage (in machine units). Also, capital's distributive share is shown by OC/OD or AF/DF.

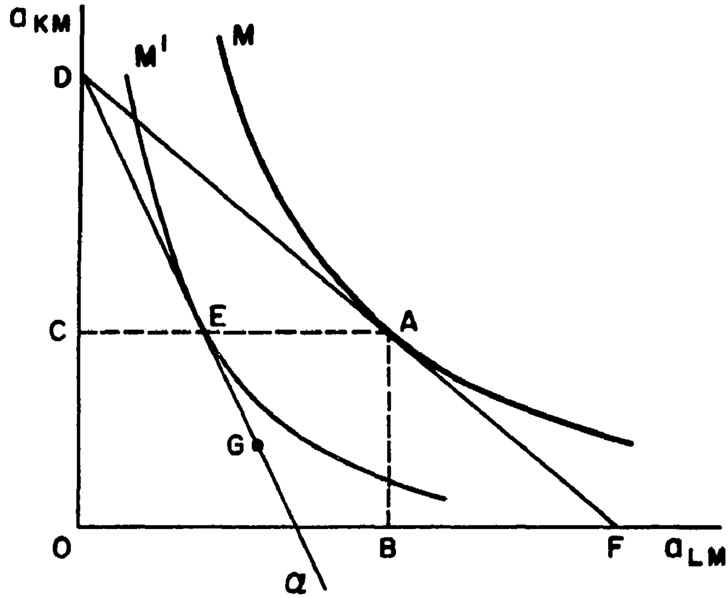


FIGURE 2

provement in output per man resulting from technological progress at a constant capital-output ratio.[6]

If technological progress is Harrod-neutral at the same rate for all points on the unit isoquant, it is clear that the old unit isoquant, M, could be obtained from the new by a renumbering of labor units in efficiency terms. A uniform expansion of the a_{LM} axis would put M' into M. This feature of Harrod-neutral change was noted by Joan Robinson [11] and proved by Uzawa [13]. If before technological change the production function is $M = F(K,L)$, it can be represented, after the change, by $M = F(K,\lambda L)$. In the case shown in Figure 2, the multiplicative factor λ is $AC/EC = 1 + (AE/EC)$, or one plus the rate of improvement of output per man.

It is well known that Hicks neutrality is shown by a radial shrinking of the unit isoquant towards the origin. The slope of the isoquant is preserved along any ray from the origin. Figure 3 illustrates the case in which technological change is both Hicks- and Harrod-neutral. Hicks neutrality is reflected in identical slopes at B and C. Harrod neutrality is shown by the fact that C and A represent the same capital-output ratios for the same rate of profit.

[6] This measure of technological progress, the improvement in output per man at a constant capital-output ratio, must be distinguished from the improvement in output/man at a constant rate of profit if technological change is not Harrod-neutral. For example, suppose the isoquant M' had been tangent to the α-line at point G in Figure 2. This would represent a Harrod capital-saving change, and output per man at a constant rate of profit would not rise by as much as at a constant capital-output ratio precisely because the capital-output ratio would be reduced. AE/EC would still represent the improvement in output/man at a constant capital-output ratio (except for a second-order small).

Points D and C *of* Figure 1 merge (in Figure 3's point C), which implies that the elasticity of substitution is unity.[7]

It may be useful to think of the analogue in terms of demand theory. In Figure 3 envisage a series of isoquants representing successive degrees of Harrod-neutral improvements. The "price consumption" curve would be the horizontal line AC, representing demand for labor per unit of output that would be of unit price elasticity. If the isoquants are Hicksian-neutral as well, they represent unit income elasticity. An indifference map exhibiting unit price and income elasticity underlies the Graham demand function,[8]

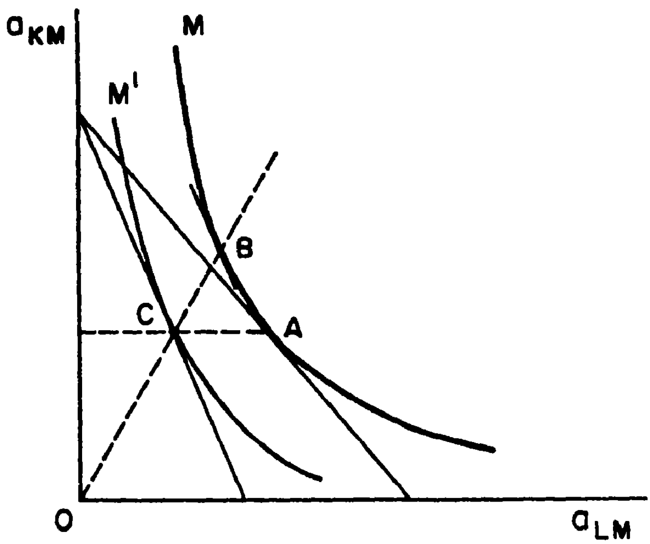


FIGURE 3

which, as is well known, is characterized by unit elasticity of substitution. (The constant-savings assumption often made in growth theories is equivalent to Graham demand functions for consumption and investment.)

In the one-sector model just described the capital-output ratio in physical terms is the same as the ratio in value terms. And the rate of profit is the ratio between the rental on machines and the price of the good produced in that one sector. Now consider the addition of a sector producing, say, food (the consumption good). Suppose the original isoquant for food and the nature of technological change are as shown in Figure 2, with a_{KM} and a_{LM} replaced by a_{KF} and a_{LF}. In the special case pictured by Figure 2, the physical capital-output ratio in the food sector, a_{KF}, would remain constant if the "own" rate of return to capital in the food industry (r/p_F) remained con-

[7] Thus the assumption of unit elasticity of substitution leads to strong conclusions. A radial shrinking of the isoquant towards the origin is equivalent to a shrinkage of the labor units or, indeed, to a shrinking of the capital units. This can be seen directly by the multiplicative form of the Cobb-Douglas function.

[8] Let q be quantity demanded, Y money income, and p price. Then $q = \alpha \cdot Y/p$ in the Graham function. For example, see McKenzie [9, p. 149].

stant. Call this a case of "own" neutrality, a definition with the advantage that it is symmetrical as between sectors and requires only information as to the nature of the change in the production function in that sector.

Only in special cases will "own" neutrality and Harrod neutrality coincide. Harrod neutrality in the F-sector obtains if the *value* of the capital-output ratio in that sector $[(p_M/p_F) \cdot a_{KF}]$ remains constant at a constant rate of profit (r/p_M). It is clear from the definitions that the two versions would coincide if the relative commodity price ratio (p_M/p_F) were to remain constant at a constant rate of profit. Otherwise it is impossible to state whether the technological change occurring in the F-industry is Harrod-neutral without knowing the extent of technological progress in the M-industry.

The concept of Harrod neutrality in the consumption-goods sector (F) of a two-sector model can be illustrated in Figures 4 and 5. Let physical units be chosen so that initially both commodity prices are unity. I choose M as numeraire so that p_m is identically unity. The price of F may change as a consequence of technological progress. Assume, for convenience, that M is capital-intensive.[9] The unit isoquants M and F in Figure 4 are then unit-value isoquants in the initial situation, with the factor price ratio shown by the slope of DG, and the rate of profit by $1/OD$.

Suppose technological progress takes place in the M-industry—shifting the unit isoquant to M'. As illustrated in Figure 4 progress is of the Harrod capital-saving variety in the M-industry. As we shall see, this is immaterial to the question of whether technological change is Harrod-neutral in the F-industry. What is relevant to that question is the *extent* of technological progress in M—the increase in output/man at a constant capital-output ratio. This is given by EG/OE.

Suppose technical improvement also takes place in the F-industry. The new unit F-isoquant may (1) still lie outside line α, (2) be tangent to line α, or (3) cut line α. In case (1) the rate of increase in output/man (at a constant capital-output ratio) in the F-industry falls short of the improvement in the M-industry. In case (2) they are equivalent and in case (3) the extent of technological progress (in Harrod's sense) in the F-industry exceeds that in the M-industry. Clearly it is only in case (2) that both goods can continue to be produced in a competitive equilibrium at the same rate of profit *without a change in the commodity price ratio.* In the case illustrated, where F' shows a smaller improvement in output per man than does M', the price of F must rise if both commodities are to be produced with $1/OD$ the rate of profit.

The argument shifts now to Figure 5. The α-line is given as in Figure 4, and is determined by the extent of the improvement in the M-industry. For F to be produced the price of F must rise. Consider deflating a_{KF} and a_{LF} by p_F. As p_F rises, this would have the effect of shifting the F' curve radially towards the origin; p_F must rise until such a shifted F' curve becomes tangent to the α-line (shown by F''). Alternatively phrased, as p_F rises, the F-isoquant showing unit-*value* becomes a radial shrinking of F'. The F'' curve in Figure

[9] Absolutely nothing in the subsequent argument depends on this.

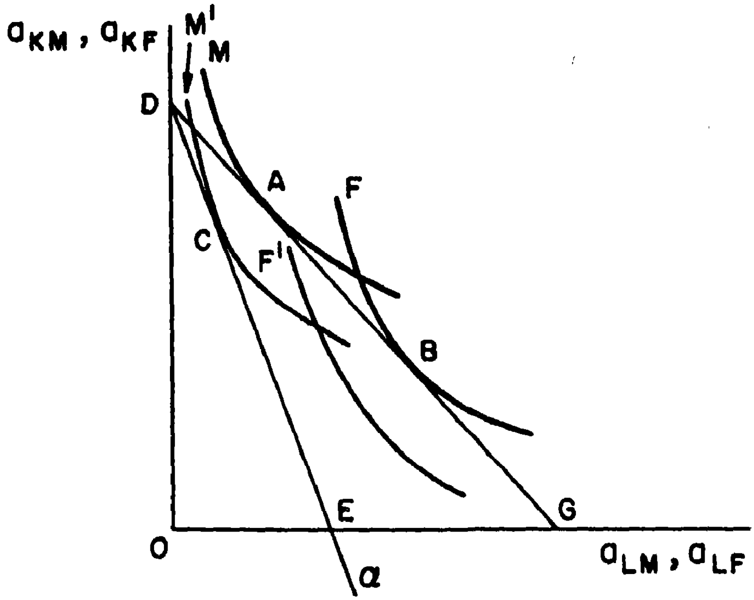


FIGURE 4

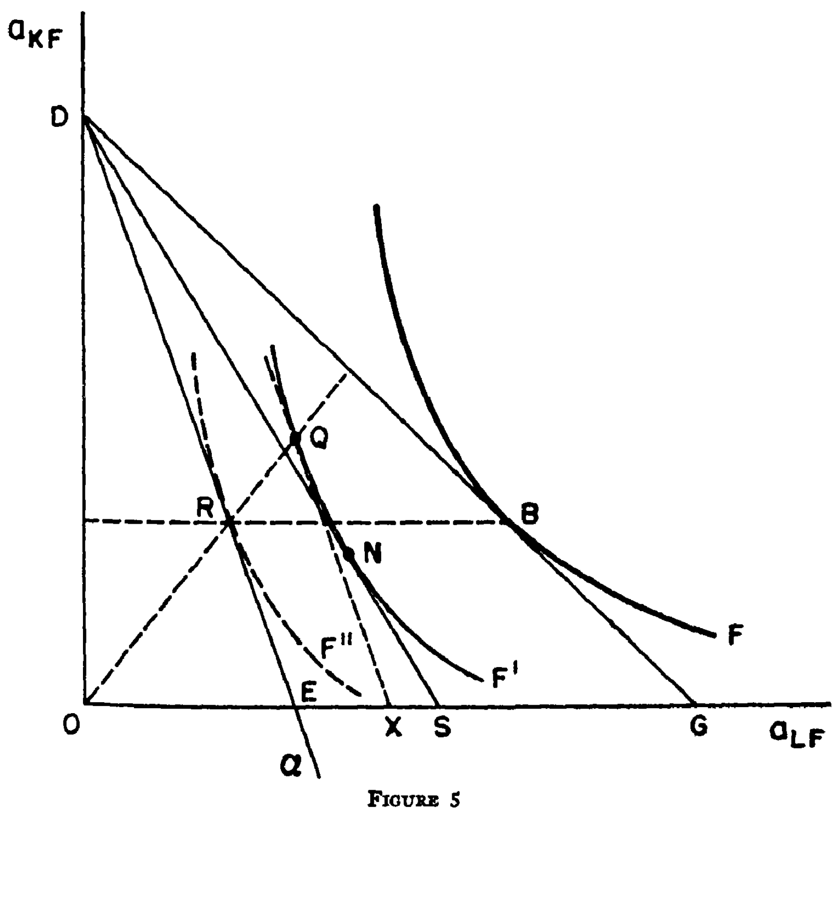


FIGURE 5

5 is pictured as being tangent to the α-line at point R, where the *value* of the capital-output ratio is the same as at B. This shift from F to F' thus exhibits Harrod neutrality. The capital-output ratio in quantity terms is shown at Q, and is higher as the price of F is higher. To recapitulate, draw a ray from the origin such that it intersects the α-line (showing the new factor-price ratio at the old rate of profit) at the old capital-output ratio. If the slope of F' at the capital-labor ratio shown by this ray equals the slope of α, the change is Harrod-neutral (and the appropriate radial expansion or shrinking of F' will bring F'' to a point of tangency with the α-line at the old capital-output ratio).

Several comments are suggested by this construction. First, suppose technological change is Harrod-neutral in the F-industry and of different rates in the two industries. It can then be what I have called "own" neutral in the F-industry only if the elasticity of substitution in F is unity. (In Figure 5 point N would have to lie on RB.) Secondly, consider the case in which no technological change takes place in the M-industry. The α-line then corresponds to DG. If technological progress takes place in the F-industry, the unit isoquant is shifted inwards. At a constant rate of profit the price of F must fall in order that both goods be produced in a competitive equilibrium. This serves to make F'' (the unit-value isoquant) a radial expansion of F'. If F'' is to be tangent to DG (the α-line in this case) at the same value of the capital-output ratio as initially, the original movement from F to F' must have been a radial shrinking towards the origin. That is, technological change in the F-industry must be Hicksian-neutral if it is to be Harrod-neutral and no technological progress takes place in the industry producing capital goods.

Finally, consider the changes in the factor-price ratio (w/r) and the real wage (w/p_F) associated with technological progress at a constant rate of profit. The rate of increase in the wage-rental ratio is given by EG/OE, which is the rate of increase of output per man in the machine sector. At this new higher relative wage rate the real wage is given by the reciprocal of OX, where QX is parallel to α and tangent to F' at Q. Expressed in percentage terms, the rate of increase of the real wage is given by XG/OX. The rate of increase of output per man in the machine sector is approximately EG/OX, and in the other sector is approximately SG/OX.[10] Therefore the rate of increase in the real wage is a weighted average of the rates of increase of output per man in the machine sector and in the consumption (F) sector, the weights given by XS/ES and EX/ES, respectively. These weights represent the capital and labor distributive shares in the consumption sector.[11]

It may prove useful to conclude by summarizing these results: (1) Harrod neutrality in the one-sector model, and "own neutrality" in the multisector model, lead to a change in the production function represented only by introducing a multiplicative factor for labor (the production function in

[10] For small changes OE, OX, and OS are approximately equal. The formulae become exact for infinitesimal changes.

[11] XS/ES is approximately equal to NS/DS, capital's share in the F-industry. (See footnote 5.)

"efficiency" units remains unchanged). (2) Harrod neutrality in the one-sector model, and "own neutrality" in the multisector model, are equivalent to Hicksian neutrality if and only if the elasticity of substitution is unity. (3) The commodity price ratio in a two-sector model rises in favor of the consumption good, at a constant rate of profit, if and only if the rate of increase in output/man at a constant (physical) capital-output ratio is smaller in the consumption sector than in the capital-goods sector (4) If the commodity-price ratio changes at a constant rate of profit any two of the following implies the third: (a) technological change in F is Harrod-neutral; (b) technological change in F is "own" neutral; (c) the elasticity of substitution is unity. (5) If no technological progress takes place in the sector producing capital goods, Harrod neutrality in any other sector is equivalent to Hicksian neutrality in that sector. (6) At a constant rate of profit, technological progress raises the real wage. The rate of increase in the real wage is the sum of the increase in output per man in the machine sector weighted by capital's share in the consumption sector and output per man in the consumption sector weighted by labor's share in the consumption sector. Should technological change occur at the same Harrodian rate in each sector, the real wage and the wage-rental ratio both increase at this rate.

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The Demand Curve for a Factor of Production: Comment

R. R. Russell's "proof" [1] that the demand curve for a factor of production cannot slope upward under conditions of perfect competition in all markets is invalid for a number of reasons, even though his conclusion is valid. The validity of the conclusion is itself of questionable usefulness, for it is dependent on incompatible assumptions. I shall discuss these criticisms in turn, and then develop an alternative proof of the proposition.

Russell's argument is in three stages: 1. When the price of factor A falls, marginal cost falls, and the firm expands output. If it is then to buy less of A, the adjustment path[1] must be backward-sloping. 2. Since the *VMP* curve of A is downward-sloping, the firm can buy less of A when its price falls only if the *VMP* curve falls. 3. A fall in the *VMP* curve is incompatible with the backward-sloping adjustment path. The first stage assumes that marginal cost falls, but it is possible that marginal cost might rise when the price of one factor falls, in which case the firm would contract output. Thus to show that a fall in the *VMP* curve is incompatible with a backward-sloping adjustment path does not prove that the demand curve cannot be upward-sloping, since the backward-sloping adjustment path is not shown to be a necessary condition for an upward-sloping demand curve.

The third stage of his argument rests on the spacing of the isoquants in his Figure 2a, which is reproduced as my Figure 1. Russell says, ". . . the horizontal distance between Y_1 and Y_8 . . . is greater when $B = Ob_1$ than when $B = Ob_2$" [1, p. 732]. From this he infers that VMP_A is greater when $B = Ob_2$ than when $B = Ob_1$, which is incompatible with the fall in the price of A. While his argument on the spacing of the isoquants is valid, the inference drawn from it is not. What the relative spacing shows is that the average VMP_A over the range a_0 to a_2 is greater than the average VMP_A over the range a_3 to a_6, and as Figure 2 shows, this is not incompatible with the VMP_A at a_2 ($B = Ob_2$) being less than the VMP_A at a_3 ($B = Ob_1$). Thus he does not prove stage three of his argument, and if he did, it would be inconclusive in establishing his proposition.

My second criticism concerns Russell's choice of assumptions. He assumes that the firm can expand output when the price of one factor falls, although the product price remains constant. But if all markets are competitive, the factor price must fall for all firms in the industry, and if the product price is to remain constant when all firms expand, the market demand curve must be perfectly elastic. This is a theoretical impossibility. A general proof of the proposition concerning the downward slope of the demand curve for a factor should take account of a consequent change in product price, and should also include the long-run case as well as the short run. The following analysis attempts to cover all cases. However, while changes in product price are considered, because product price necessarily changes, all other possible changes caused by the change in factor price are assumed away, in order to keep the

[1] The term adjustment path refers to the locus of equilibrium points achieved as the price of A changes.

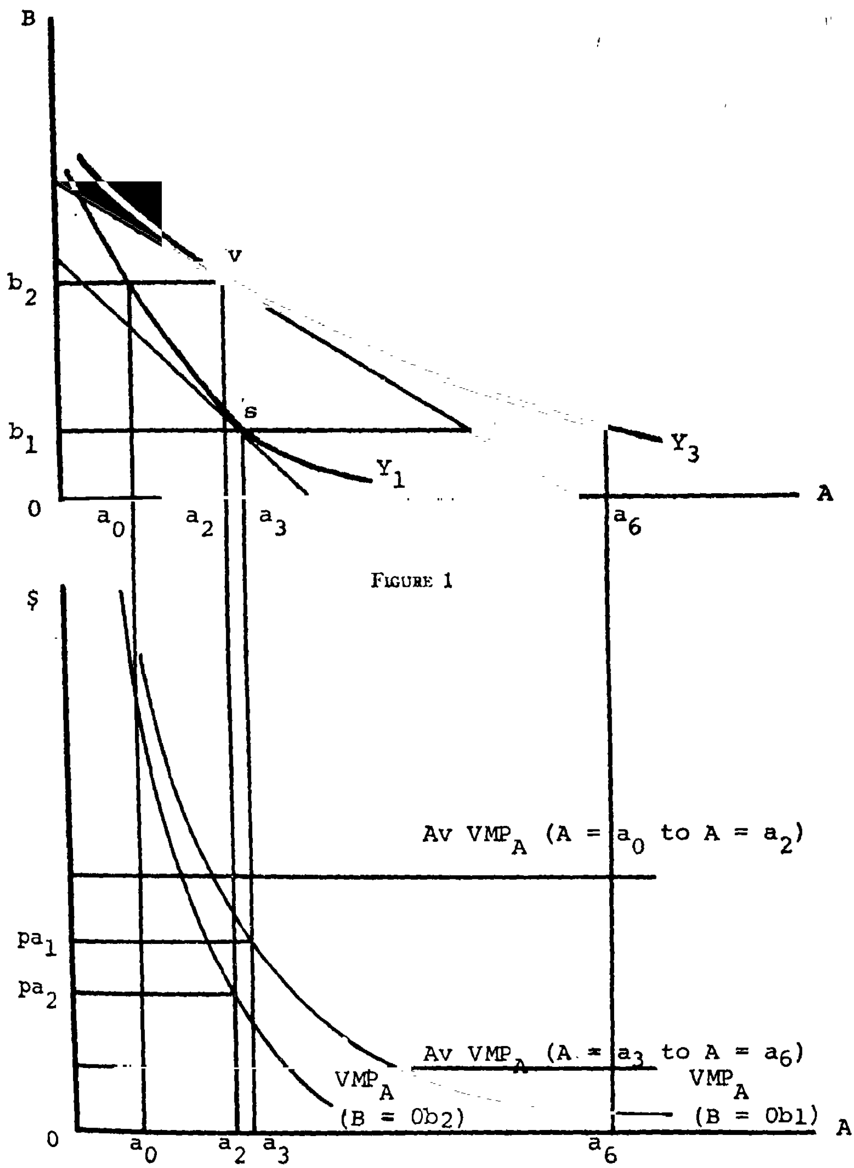


FIGURE 1

FIGURE 2

FIGURES 1 AND 2

problem within the confines of partial equilibrium analysis. In particular it is assumed that no other factor price changes, and that no other product price changes.

The Short Run

In the short run the number of firms is fixed. If we can show that no firm can have an upward-sloping demand curve for a factor, then the same must be true of the industry's demand curve. Figure 3 shows a case where the marginal cost falls as a result of a fall in the price of factor A from Pa_2 to Pa_1. The additional cost of the increase in output from Y_1 to Y_2 can be expressed in terms of B, the price of which is constant. If the rise in output from Y_1 to Y_2 is one unit, marginal cost at Pa_2 is b_2b_4, and at Pa_1, b_1b_3; $b_2b_4 > b_1b_3$, therefore $b_3b_4 > b_1b_2$, and the point s must lie to the right of r. When Pa falls, marginal cost falls, and if the product-demand curve is either perfectly elastic or downward-sloping, all firms will expand output, the expansion of the firm under analysis being, say, from Y_1 to Y_2, that is, from t to v. But v must lie to the right of s, s to the right of r, and r to the right of t, from which it follows that the quantity of A purchased must increase when Pa falls. This shows that Russell's case [1, Figure 2a] is impossible for he assumes that marginal cost falls, but v lies to the left of t. If, however, the demand curve for the product is upward-sloping, output of the industry will fall when M.C. falls, the firm under analysis reducing output from, say, Y_2 to Y_1, that is, from x to y. It is then possible, though not necessary, that quantity of A demanded will fall when Pa falls, and the firm's demand curve for A be upward-sloping.

If marginal cost does not change when Pa falls, the firm's output will not

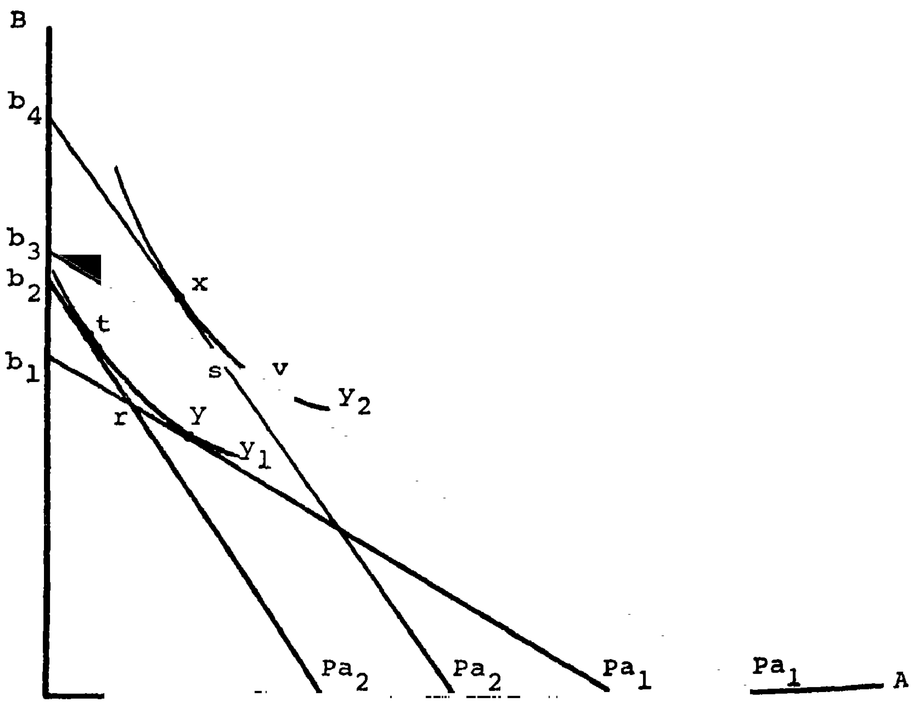


FIGURE 3

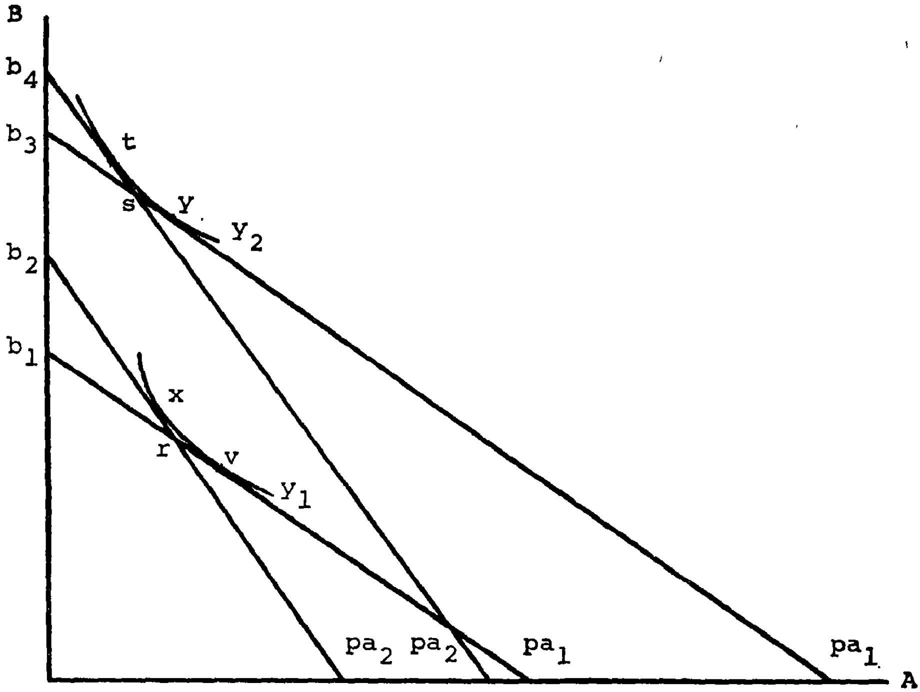


FIGURE 4

change, no matter what slope the product-demand curve has. The firm then remains on the same isoquant, and the demand curve for A must be downward-sloping.

Figure 4 shows a case where marginal cost rises as a result of the fall in Pa. In this case $b_2b_4 < b_1b_3$, therefore $b_3b_4 < b_1b_2$, and s must lie to the left of r. If the demand curve for the product is perfectly elastic or downward-sloping, the firm will reduce output when Pa falls and M.C. rises. The firm under analysis might reduce output from Y_2 to Y_1, that is, from t to v. But v must lie to the right of r, r to the right of s and s to the right of t. Again the quantity of A purchased rises when the price of A falls and the demand curve for A is downward-sloping. However, if the product-demand curve is upward-sloping, output will increase when Pa falls, from x to y in Figure 4, for example. In this case it is again possible, though not necessary, that the demand curve for the factor might be upward-sloping.

Thus the conclusion in the short run is that the demand curve for a factor of production must slope downward in all cases, except where the product-demand curve is upward-sloping, in which case it is possible, though not necessary, that the demand curve for the factor could be upward-sloping.

The Long Run

The long run differs from the short run in two ways. The firm's equilibrium is established not merely where price equals marginal cost, but where price equals minimum average cost, and the number of firms is variable. The long-

run adjustment of a single firm to a fall in the price of factor *A* involves reference to the expansion path shown on the isoquant diagram, Figure 5. The *B* axis represents all other factors, the prices of which are assumed to remain constant. It is assumed that the isoquants have the normal characteristics, with smooth curvature, and that the average cost curve has a unique minimum point. The last assumption is necessary for the firm to have a determinate output.

Figure 5 shows the isoquant for that level of output at which average cost

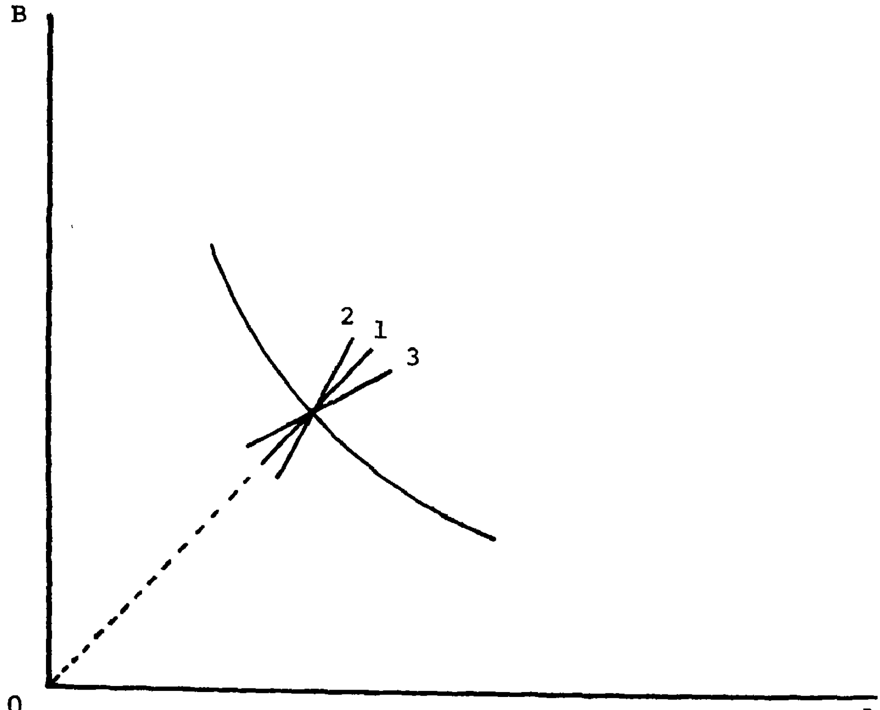


FIGURE 5

is minimized, and three possible slopes for the expansion path at that point. When the price of factor *A* falls, the firm increases its use of factor *A* relative to *B* for any output, but this alone does not establish that the *A*:*B* ratio rises unless possible changes in the equilibrium output are taken into account. In case 1 the expansion path has the same slope as the vector, as output changes with constant factor prices, the *A*:*B* ratio remains constant. An infinitesimal fall in *Pa* will therefore move the average-cost curve down by a constant proportion in the vicinity of the minimum point, and the output at which average cost is minimized will not change. The firm stays on the same isoquant and the *A*:*B* ratio rises. In case 2 the *A*:*B* ratio falls as output rises through the optimum output. An infinitesimal fall in *Pa* will therefore cause the A.C. curve to fall by a greater percentage at outputs below the optimum than at greater outputs, and the optimum output will fall. This fall in out-

put will itself raise the $A:B$ ratio. Thus the $A:B$ ratio rises both because the price ratio changes and because output falls. In case 3 the $A:B$ ratio rises as output rises through the optimum. A fall in Pa reduces A.C. by a greater percentage at greater outputs and the optimum output rises, again intensifying the tendency for the $A:B$ ratio to rise as Pa falls. We can therefore conclude that, whatever adjustment takes place in the output of a single firm, the $A:B$ ratio will rise for all firms when Pa falls. Since in a context of perfect competition the same technology is available to all firms, extant and potential, it follows that the $A:B$ ratio rises for the industry as a whole.

The long-run supply curve is a locus of equilibrium points at which price equals minimum A.C. When Pa falls, the supply curve must fall. Equilibrium output must then rise unless the demand curve is upward-sloping. If output rises and the $A:B$ ratio rises, the quantity of A used must rise, and the demand curve for A must be downward-sloping. However, if the product-demand curve is upward-sloping, output will fall, in which case the quantity of A used might either rise or fall.

Thus we find that in both the short run and the long run an upward-sloping demand curve for a factor when all markets are perfectly competitive can arise only if the demand curve for the product is upward-sloping.

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Notes on Marxian Economics in the United States: Comment

Professor Bronfenbrenner's note (in the December 1964 issue of this *Review*) on the absence of Marxists in U.S. universities cannot be allowed to pass without at least a protest. As a refugee from American higher education, one who lost his position although he was on tenure for refusing to answer questions about his politics, I may be pardoned for feeling strongly on the subject.

An attentive reading of Bronfenbrenner's paper reveals the following chain of thought:

There are now no outstanding Marxists on the economic faculties of American universities. This fact is not to be attributed to the fact that Marxism has been proved wrong: "Marxists like [the late Paul] Baran may [have been] more right than the rest of us." Indeed, American economics training is defective in that it does not include adequate exposure to Marxism. Rather the lack of Marxists is due firstly to the brainwashing to which candidates for academic posts must submit during their graduate years, not so much in the way of anti-Marxist indoctrination as in exclusive concentration on mathe-

matics and statistics. Second, the absence of Marxists is due to the fact that academic economists prefer promotion and pay to a genuine search for truth. Not only do they adopt this code voluntarily themselves but they impose it on "junior, pre-tenure colleagues."

A more crass defense of opportunism it would be impossible to find.

I will not attempt to argue principles with Bronfenbrenner, who is concerned about moral standards not for their own sake but "only to refurbish the international repute of U.S. liberalism." For Bronfenbrenner it is axiomatic that an American university teacher of economics accepts the values of the rulers of the society in which he lives. *"Cuius regio eius religio"* is the motto of his paper. As contrasted with the Austro-German academician, who has academic freedom "Fascism and revolution aside" (!), "we" have "permitted some erosion . . . of academic freedom in the conventional sense." You can say that again!

But Bronfenbrenner's account is factually less than perfect, nowhere more than in his defense of Harvard. He duly records that Paul Sweezy was refused promotion at Harvard, and was thereafter supported for "no desirable academic assignments whatever." Did Harvard in failing to support Sweezy for a job elsewhere have no responsibility for his being purged from academic life? No, this charge is "palpably false," Bronfenbrenner assures us. He leaves the impression that there is no pattern at Harvard of denying promotion to left-wingers. He has of course not heard that in the 1930's there was an insistent demand that Harvard make a policy statement on this very point —apropros of the Walsh-Sweezy case, which involved Alan Sweezy, brother of Paul—and that Harvard declined to do so. He also does not know about the notorious "visiting committee" which was set up by conservative Harvard graduates *with the acquiescence of the administration* to make a spot check of alleged heresies in the social science departments.

Bronfenbrenner overrates the importance of tenure as such. Tenure did not protect the profession in the purges of the 1950's, and it is to the discredit of academicians that they permitted the brutal assault on this supposedly vital institution to pass unchallenged. They even condoned it. I recall presenting a resolution against the witch-hunt to a meeting at the Midwest Economics Association. It received just six votes!

Bronfenbrenner argues that the conservatives have lost out in their battle for the minds of the students because "a good deal of Marxism" is included in the left-Keynesian position, which he thinks (rather inconsistently) may be the dominant school at present; and because Marxists do appear sporadically on campus for visits. Note the "sporadic." He even quotes Ayn Rand as having given up on modern intellectuals because they are too "left." He might have quoted Elizabeth Bentley, who thought that any Ph.D. was suspect. It is preposterous to say, as he does, that "conservatives complain of their exclusion from contact with economics students." What is the typical economics class but a contact between the conservative teacher and his mentees?

The left-Keynesians, again on Bronfenbrenner's own showing, are "at least as Keynesian as they are Marxist." The epigrams really slay the reader. Are

any Keynesians Marxists? Was even Paul Sweezy a Marxist when he was denied promotion, or was he seeking to act as marriage broker, to bring together "left" Keynesians and Marxists in an unnatural marriage? Bronfenbrenner shows no appreciation of the many variations on the Marxist theme which are seriously discussed abroad, and which in the nature of the case can have only inadequate attention in a higher educational system committed to "fighting Communism."

Of the external pressures on the universities, which determine the attitude of the cynical majority of professors, Bronfenbrenner has nothing to say. "Let us overlook the ownership and control of mass communications," he says; and he similarly overlooks the ownership and control of U.S. higher education. He makes no analysis of McCarthyism; instead he refers with suitable vagueness to "the U.S. tradition of liberalism." This oversight, which is probably deliberate, robs his discussion of all semblance of reality. It is not enough to say that economics students get the wrong kind of training. Economics was at one period known as the "dismal science" because academic economists had all but eliminated discussion of moral issues. These issues were reintroduced in England and the United States under the rubric of sociology. But sociology too is conservative today; and Bronfenbrenner's advice to the universities to hire more, or rather some, Marxists (where will they find them?) for the sake of refurbishing America's image abroad will run foul of the prejudices of a ruling class going all out for world domination.

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Notes on Marxian Economics in the United States: Reply

A McCarthy-era purgee, Professor Davis is justifiably angry with the American academic Establishment, which he is attacking over my shoulder. Unfortunately, he has selected the wrong shoulder. My actual position is closer to Davis' own than to the one which, in his anger, he attributes to me.

In Davis' view, I have committed two crimes. One crime was failure to discuss many matters which he wanted discussed. I plead *nolo contendere*, mitigated by lack of space. The other crime was offering *two* explanations for the parlous state of Marxian economics in America. I tried to summarize the view which Davis supports, and called it "alarmist." I also summarized the view which he opposes, and called it "complacent." Perhaps I went too far to make the "complacent" case plausible, but my biases, on balance, are *not* on that side. In any case, does not complacency too deserve a hearing?

As a minor count, I also stand accused of proposing the hiring of occasional innocuous Marxian economists to refurbish America's overseas image. This is a less-than-half truth. My main point is that some Marxists may be nearer the truth than most academics; image-polishing is a minor consideration.

If more universities opened academic competition to Marxian candidates on

even terms, Davis wonders where we could find competent candidates. (So do a number of conservative economists!) I myself anticipate no shortage of aspiring Marxist missionaries of quality. Most would be foreigners, no doubt. A few, concerned about McCarthy's Ghost and the Birch Society, would understandably limit themselves to temporary appointments at the outset.

Davis makes a great deal of the difference between Keynes and Marx, as he must to prove that our students are being misled. My claim is that some sects of the two religions overlap, so that what is served up as "lib-lab" Keynes in America sometimes includes substantial Marxist infusions, not always acknowledged or digested.

On the importance of academic tenure in the problems of free thought for Marxian and other dissidents, I'm statistically right in concluding that things go fairly well once tenure is achieved. (Paul Baran was a case in point.) This does not deny cases like Davis' own, where tenure could not save witches from hunters, or periods of "Fascism and revolution" like Germany in 1933 or China in 1957, when tenure meant little or nothing.

I lack either inclination or competence to sort out truth from falsehood in Davis' charge against Harvard University. To one who knows only the general outlines of the incidents Davis mentions, they sum to something short of Harvard's finest hour. They also fall short of Davis' picture of Fascist purges of scholars by alumni on the banks of the Charles. I should instead like to correct four lapses from scholarship, one involving Harvard, which readers[1] have found in my December note. (1) Although Russian-born, Baran received Polish citizenship as a child. He never held Soviet citizenship. (2) Baran's dissidence from Stalinism in the 1930's did not fit neatly into the "Bukharinist" pigeonhole into which I classified it. (3) Paul Sweezy was never denied a tenure appointment at Harvard. Rather, a close decision to promote to tenure rank another candidate (the distinguished labor economist, John Dunlop) was interpreted generally as foreclosing Sweezy's chances. Sweezy himself interpreted the outcome in this way and resigned from Harvard before the expiration of his contract. (4) My reference to "Labor Research Associates" should have read "Labor Research Association."

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[1] Readers who pointed out one or more errors are George Garvy, Gottfried Haberler, Victor Perlo, Nicholas Spulber, and Paul Sweezy.

ERRATA

In the communication by Paul Samuelson, "A Catenary Turnpike Theorem Involving Consumption and the Golden Rule" (*American Economic Review*, June 1965, pp. 486-96), many subscribers' copies contained a number of typographical errors which resulted from the breaking-off of parts of certain characters as the printing of the issue proceeded. Thus the dots over letters denoting time rates of change were omitted from a number of the equations. The omis-

sions occur in equations (1), (3), (4), (5), the statement of the theorem (p. 491), (8), definition of U (top of p. 494), and (13). These should read:

$$\text{(1)} \qquad \underset{\{k(t)\}}{\text{Max}} \int_0^\infty u[c(t)]dt = \int_0^\infty u[f(k(t)) - \dot{k}(t)]dt$$

$$\text{(3)} \qquad H(\dot{k}, k) = \dot{k}\,\frac{\partial u[f(k) - \dot{k}]}{\partial \dot{k}} - u[f(k) - \dot{k}] \equiv \text{constant.}$$

$$\text{(4)} \qquad \underset{\{k(t)\}}{\text{Max}} \int_0^T u[f(k) - \dot{k}]dt \quad \text{for} \quad k(0) \leqq k^0,\ k(T) \geqq k^T \geqq 0.$$

$$\frac{d}{dt}\frac{\partial u}{\partial \dot{k}} - \frac{\partial u}{\partial k} \equiv 0 \quad \text{or}$$

$$\text{(5)} \qquad -\frac{d}{dt}u'[f(k) - \dot{k}] - u'[f(k) - \dot{k}]f'(k) \equiv 0$$

$$u''[f(k) - \dot{k}][\ddot{k} - f'(k)\dot{k}] - u'[f(k) - \dot{k}]f'(k) = 0.$$

$$\underset{\{k(t)\}}{\text{Max}} \int_0^T u[f(k) - \dot{k}]dt \quad \text{for} \quad k(0) = k^0,\ k(T) = k^T$$

$$\text{(8)} \qquad c_1 + \dot{k}_1 = f(k_1, k_2, \cdots; c_2 + \dot{k}_2, \cdots)$$

$$\underset{\{c_1,\cdots,c_n\}}{\text{Max}} u[f(k_1, k_2, \cdots; c_2 + \dot{k}_2, \cdots) - \dot{k}_1, c_2, \cdots] = U(k_1, k_2, \cdots; \dot{k}_1, \dot{k}_2, \cdots)$$

$$\text{(13)} \qquad c + \dot{k} = f\left(\frac{K}{L}\right) - g\,\frac{K}{L} = f(k) - gk \equiv \psi(k)$$

In the book review by Joseph A. Hasson in the June 1965 issue, page 566, paragraph 2, the third sentence was incorrectly printed. This sentence should be: "Such statements are implicitly predicated on static analyses and overlook that lines among classes of goods are not clear-cut; that increases in incomes have little or no effect on demand patterns and elasticities; that development leaves factor endowments, their relative prices and productivities, and technological levels almost unaffected."

BOOK REVIEWS

General Economics; Methodology

"Positive" Economics and Policy Objectives. By T. W. HUTCHISON. Cambridge: Harvard University Press, 1964. Pp. 200. $5.75.

This book is a contribution to economic methodology and doctrinal history by a writer who has made a name for himself in both fields. It contains a thoughtful and amply documented examination of two ranges of questions, one relating to the distinction between positive and normative elements of thought, and the other to the formulation of policy objectives and policy preferences. Each of these topics is treated in two chapters, one reviewing its doctrinal history and the other investigating the matter more systematically.

Few matters can provide a better illustration of the cunning of reason, or of the importance of the unintended consequences of any one's thought or action, than Hume's distinction between what is and what ought to be. When Hume originally set forth his ideas about the distinction, he did so without relating it directly or indirectly to economic thought. In the course of time, however, the distinction has served as a potent device to establish the scientific character of economics, and with the writings of Mill, Cairnes, Robbins, and Friedman it has become part of the mainstream of the tradition of our science.

While the distinction is often facilely drawn, its application is beset by a host of problems. No wonder then that a more skeptical attitude to the dichotomy has become noticeable in recent decades. Myrdal, who had endorsed it in the original edition of his *Political Element in the Development of Economic Theory* of 1930, blaming those writers with whose findings he found fault for not observing it, has since changed his mind and in the preface to the English edition of 1953 denied the existence of a body of scientific knowledge acquired independently of all valuations, branding the contrary belief as naïve empiricism.

Taking account of this and other skeptical voices, T. W. Hutchison attempts to break a path that avoids the panideologism of those who see valuations everywhere and deny the possibility of objective truth, as well as the opposite extreme, the exponents of which tend to cover up the many problems inherent in the distinction between what is and what ought to be by exhortations calling for the strict observation of the dichotomy. In tracing his way, Hutchison adopts Popper's demarcation criterion, according to which scientific propositions differ from valuations in that they can be intersubjectively tested or refuted by experience. Hutchison then goes on to classify valuations. He distinguishes (1) inevitable or prescientific ones which relate to the choice of the problems to be studied and the adoption of criteria or rules of procedure by which the problems are to be studied; (2) ethical or political valuations

regarding the objectives of policies—inevitable if policy recommendations are being put forward; (3) biased subjectivity in positive empirical statements, where the valuation is neither inevitable nor legitimate. These three classes of valuations are judiciously examined with all but complete references to the relevant economic literature.

The second part of Hutchison's book is specifically devoted to the type of valuation relating to the objectives of economic policy. His discussion includes a critical review of technical welfare economics as an aid in the formulation of policy objectives. He cites Little, according to whom welfare theory has been persistently used by all sides as a political weapon, and arrives at the conclusion that "the monistic, welfare-maximizing formulation of policy objectives obscures clashes of objectives and the need for choices and valuation relating to the objectives of economic policy. His discusison inchoices and valuations, Hutchison would prefer the listing of a plurality of goals such as Bentham's four "objects" of economic policy—subsistence, security, abundance, and equality—which he compares with a variety of examples taken from modern sources.

In a concluding chapter, Hutchison appraises the relevance of the questions of valuations and policy objectives in the light of the economist's professional ethics. He advises that, where a clear distinction between normative and positive statements can be maintained, it should be maintained, but that, while the two should be distinguished when possible, the economist need not be precluded from making the former. As for policy recommendations, these will invariably require either the postulation or the assertion of valuations.

The book is derived from four lectures which the author gave at the University of Virginia in 1960. His work offers a reliable guide to past thought on the epistemological foundations of our science as well as a fair résumé of the present state of the debate.

HENRY WILLIAM SPIEGEL

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Decision and Choice—Contributions of Sidney Siegel. Edited by SAMUEL MESSICK and ARTHUR H. BRAYFIELD. New York: McGraw-Hill, Inc., 1964. Pp. 298. $8.95.

Nearly forty years ago, J. M. Clark wrote in the *Journal of Political Economy:* "If the economist borrows his conception of man from the psychologist, his constructive work may have some chance of remaining purely economic in character. But if he does not, he will not thereby avoid psychology. Rather he will force himself to make his own, and it will be bad psychology" [1, p. 30].

Clark, of course, was here merely expressing again the insecurity that generations of economists have felt regarding the preconceptions of their science. It would be well to base a behavioral science on a sound concept of human behavior. Thrash about as we may, however, to replace the dubious rationalism of Bentham's Felicific Calculus, we seem unable to do so. We either assert that psychology is beyond our realm of interest, as did Davenport, or

else, as Mitchell suggests, "in our prefaces and introductory chapters some of us repudiate hedonism and profess volitional psychology or behaviorism . . . in the body of our books, however, we relapse into reasonings about behavior that apply to creatures that are essentially reasonable" [3, p. 181].

Decision and Choice—the Contributions of Sidney Siegel is a notable book in light of this inability of economists to borrow successfully from the psychologist. In this book the shoe is on the other foot. A psychologist has borrowed the economists' concept of man's behavior and subjected it to investigation in the psychological laboratory. The concept borrowed is utility maximization. This collection of articles, which mostly appeared originally in psychological journals between 1956 and 1961 (when Siegel died at the age of 45), suggests that the marriage of the behavioral sciences may yet take place; that the matchmakers may be von Neumann and Morgenstern, and the consummation will be some form of what we presently call "decision theory."

Siegel's conception of utility is developed from that suggested by Friedman and Savage in their 1948 article on "The Utility Analysis of Choice Involving Risk." That is, given a person's subjective perception of the probability that an event will occur, and given the utility of the consequences of the event to him, it is assumed that he will choose between alternatives so as to maximize *expected* utility, or utility, as it were, discounted for risk. From this, it deductively follows that if in an experimental situation you can control the subject's perception of the probability that an event will occur, and you can observe the choices he makes, it is possible to infer relative quantitative measures of utility associated with these choices.

In these articles, the economist who is familiar with the Mosteller and Nogee verification of the Friedman-Savage hypothesis can find numerous similarly ingenious experiments described. Using a special nonsense syllable die he invented to assure a perceived probability of occurrence-nonrecurrence of 50-50, Siegel experimented with students gambling for grades in a course to determine utility levels associated with each. He experimented with graduate students, offering different books as the pay-off. In one truly imaginative series, he gambled with penitentiary prisoners for cigarettes, the medium of exchange in that closed economy. In each instance scales of measured utility were developed and tested for predictability at various intervals on the achievement scale. The results, simply presented in articles that are notable for their brevity and clarity, will be of interest to all economists concerned with economic motivation, economic behavior, and considerations of "utility and all that."

Out of his experimental work with utility, Siegel developed a way to measure "levels of aspiration," a concept with a long bibliography that traces its origins to Lewinian Field psychology and presently enjoys the attentions of students of such varied aspects of human behavior as abnormal psychology, management science, elementary education, the economics of underdevelopment, and many more. As Kurt Lewin originally defined level of aspiration, it denoted the "degree of difficulty of a goal towards which a person is striving" [2, p. 181]. Employing the concepts of expected utility maximization, Siegel demonstrates that a quantitative utility measure may be applied to aspira-

tion. He finds that the goal that will be most meaningful in motivating activity, be it economic or other, is that which brings the greatest marginal utility to the subject—a finding that will be viewed by most economists with some relief. Given a methodology that can locate aspiration levels, it is to be hoped that the psychological laboratory will be able to investigate the dynamics of changing goals and tastes, thereby giving greater insight into entrepreneurial behavior or permitting the economist to discuss expectations, relative income effects, or long-run consumption functions with greater certainty.

Siegel, in collaboration with economist L. E. Fouraker, subsequently applied his techniques directly to an important economic problem, the effects of learning and experience on behavior in a bargaining situation. Their efforts resulted in a book that won the 1959 $1,000 award of the American Academy of Arts and Sciences, *Bargaining and Group Decision Making: Experiments in Bilateral Monopoly*. Excerpts from this book, as well as further comment by Fouraker, appear in *Decision and Choice*.

Certainly the plethora of literature in each specialty and subspecialty of economics makes it difficult to keep abreast of developments in one area, and nearly impossible to read in depth in many. For this reason most economists doubtless missed these articles as they originally appeared in the psychological literature. The editors of *Decision and Choice* have again called the tragically truncated work of Sidney Siegel to the attention of the world of the behavioral sciences. It is well worth noting.

Rexford Tugwell wrote, "it is only the study of human nature that can force the great closed door of economic theory" [4, p. 332]. Siegel's experiments will certainly not force the great closed door open, but they may help to put it a bit ajar.

STANLEY KAISH

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University Economics. By ARMEN A. ALCHIAN AND WILLIAM R. ALLEN. Belmont, California: Wadsworth Publishing Company, 1964. Pp. xv, 924.

The distinguishing feature of this strong new entrant into the elementary textbook field is best set out in its preface: "The fundamental principles and theorems of analysis are included and stressed and repeatedly applied." Throughout a long book (now the 900-page barrier is broken), a few principles lead to a flood of applications.

It is easy enough to say that the elementary course should avoid doing all things in once-over-lightly fashion. But Professors Alchian and Allen, in this first edition at least, have enough of the courage of others' convictions to resist sacrificing depth for breadth through fully two-thirds of their book. The remaining one-third makes the appropriate bows to macroeconomics, to international trade, and to economic development abroad. But these latter pages will not move the text into the forefront of a crowded market; they are too cursory, too uneven in difficulty for a student who has been led by firm hands through microeconomics in the first 30 chapters and too abstracted from current policy problems to warrant more than a passing glance.

The text stands or falls then on its detailed examination of how markets operate as resource allocators. Here, Alchian and Allen are virtually without rivals. The market mechanisms have not been so thoroughly, clearly, and imaginatively developed in other texts. Where other books often leave the student wondering why it is that the intersection of supply and demand is not the determinant of so many real world prices, these two authors extract every ounce of teaching value from just such cases. The distinction made between price-searchers' markets and price-takers' markets is but one useful aid to this end.

The longer one works in the elementary field, the more he realizes how everlastingly difficult it is to get across a thorough understanding of the role of prices in an economy; the parroting of a few key concepts is easy enough, but enough mastery of these concepts to permit easy and automatic use of them to unfolding situations is rare. Alchian and Allen have thus chosen a critical target for their efforts. Their method is a slow and precise building of the tools and then detailed examples of their use. They take little for granted as they fight some of the more popular misconceptions in economic thought. If the instructor will share the authors' patience and self-discipline in keeping the tools simple and the examples plentiful, the student must emerge from this beginning course as a more rigorous thinker.

Certainly a student who learns his lessons from this book will be in no doubt about the cautionary questions to ask before he invites more interferences, public or private, in market places. Alchian and Allen will argue here that their purpose is simply to force the student to separate economic analysis from value judgments. That this purpose is partially thwarted from the outset is no criticism of this book, for the break between analysis and values can never be quite as clean as the writers assume. And the textbook market can well afford an entrant so committed to libertarian values.

A feature of the book to which the authors point with special pride is the section of questions and, in some cases, of answers accompanying each chapter. No mere discussion-starters, these questions are core parts of the teaching strategy. The device is both unusual and uneven. At their best, the questions are effective and exciting extensions of the line of analysis developed in the chapters. They carry the separation of analysis and emotion as far as it can reasonably be carried. At their worst, which is fortunately rare, the questions and answers are trivial, obscure, or just plain maddening. What, for example, is a teacher to do when he encounters, " 'Don't be selfish, Jane,' said Mother.

What did Mother mean?" Or how is he to treat such "answers" in the back of the book as, "Your guess is as good as ours," or "We asked the question first. You answer it"?

The authors' search for liveliness in economics leads them along similarly uneven paths. There are abundant examples in the book to remove students' doubts about the usefulness of the economists' tools in analyzing the world around them. The Rose Bowl game, nonraiding agreements among the campuses of the University of California, coffee futures markets, subsidized dinner parties for employees—they are all here, not as casual entries but as detailed illustrations of critical points. What is needed for the next edition is some discipline in the liveliness. A book as good as this one needs neither cuteness ("And that, gentle readers and young scholars, is the end of the book. What did you expect? A pot of gold?"), nor cynicism ("After a few more tries, we'll get the hang of it," as a concluding comment on U.S. experience in financing wars), nor churlishness (the word "nonsense" applied freely to many an almost-sensible statement) to win favor.

It is no perfunctory gesture to welcome this book to the market. In its concentration, its style, and its thoroughness, it broadens choice for the elementary course.

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Simulated Economic Models—A Laboratory Guide to Economic Principles of Market Behavior. By JOHN HALDI AND HARVEY M. WAGNER. Homewood, Ill.: Richard D. Irwin, Inc., 1963. Pp. xii, 172. $5.00.

The reviewer of a work whose primary purpose is to introduce an innovation in teaching technique, rather than an innovation in economic theory or a contribution to empirical knowledge in some area, faces a difficult task. The only fair and reasonable method of judging the work is to test it on students in the classroom, and compare their understanding and retention with students who have been introduced to economics by more conventional methods. Unfortunately, this course of action was not open to the reviewer: hence, judgment must be passed on the basis of simply reading the text.

The preface to *Simulated Economic Models* states:

> *Simulated Economic Models* is an innovation in teaching modern microeconomic theory. It is written for liberal arts, business, and engineering students who are learning economic principles. In a series of controlled laboratory experiments students demonstrate the scientific validity of current economic theory of market behavior by enacting managerial business decisions. In an understandable and lively way, students see:
>
> How economic theory can be applied in a variety of realistic and complex situations.
>
> How a free enterprise system functions to determine what shall be produced, how, and for whom.
>
> How dynamic forces guide a competitive market economy toward equilibrium.

How factors of uncertainty and risk must be taken into account when making economic decisions.

The authors recognize that the chief defect of such "management games" is that they reveal much about the players, but little about the real-world situations they claim to portray. Hence, much time has been spent in designing competitive situations that clearly illustrate the importance of specific economic generalizations and principles. In their first model, two manufacturing firms with identical cost schedules compete for the business of two retailers who sell to separate markets with identical demand curves. This situation serves as the vehicle for introducing the concepts of the role of prices in a market system, demand elasticity, derived demand, and the notion of market equilibrium. Model two is identical to model one, except that the demand curve shifts while the game is played, introducing the players to the distinction between movement along, and a shift in, a demand schedule.

Successive games are designed to introduce, by a series of progressive modifications to the game structure, the distinctions among total, fixed, and variable costs, the use of marginal cost and marginal revenue in price and output decisions, the effects of competition at the retail level, economies of scale, and last but not least, the virtues and vices of collusion in restraint of trade.

All in all, this book would seem to be most useful as a supplementary text in a small introductory course. Unlike most management games, no computer is needed; hence the book could be used in schools where such facilities are unavailable. The authors recommend that each of the simulations in the six laboratory sessions should last from 30 to 75 minutes, depending on the decision as to how many time periods each game will be played. If one adds to this the time the authors recommend be taken in discussing the results, the whole set of six sessions should consume about 18 hours of class time.

The crucial question, of course, is: is it worth devoting 18 hours of class time to such an endeavor, given that this represents around ⅓ of the total contact time available in an average introductory micro theory course? If, as the authors suggest, the trade-off is one simulation for every 10,000 well-chosen words, the answer is probably yes: any marked diminution in this ratio, and my guess is that the answer is probably no.

F. T. SPARROW

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Stabiles Geld—Stetiges Wachstum, Jahresgutachten 1964/1965. Sachverständigenrat zur Begutachtung der gesamtwirtschaftlichen Entwicklung. Stuttgart and Mainz: W. Kohlhammer Verlag, 1965. Pp. 208 + charts and tables. DM 10.80.

A West German law of August 14, 1963, established a Council of Economic Experts (Sachverständigenrat) with the task of submitting an annual report to the government on the state of the economy and a forecast of future developments. In particular, the Council is charged with investigating how, within the general framework of a market economy, internal and external

monetary stability, full resource use and a high level of sustained economic growth can be achieved. Additionally, problems of income and property distribution are to be investigated. The report of the Council need not be unanimous; there may be a minority opinion. However, the law provides that the federal government must take a position on the Council's recommendations within eight weeks after its report is submitted. All reports are to be published.

The report falls into two parts. By far the larger is a review of West German economic development. This part is amply documented and will be an addition to the existing literature on this subject, even if in many places it is somewhat aphoristic. In its historical analysis the Council (like so many other students of the German economy) was evidently handicapped by lack of relevant data in important areas. For example, the data on income and property distribution are spotty or not available, while the fiscal data do not allow an estimation of the leverage effect of government budgets on the economy.

The second part of the report gives a forecast for the first six months of 1965. It predicts continued full employment, somewhat lower export surpluses with improving terms of trade, and a further rise in the price levels at an annual rate of 3 to 3.5 per cent.

The report is titled, "Stable Money—Continuous Growth," and the problem of how the money can be kept stable is indeed the main concern of the Council. West Germany has seen in the last years a rising price level. It is the Council's opinion that this is essentially due to an "imported inflation." The obstacle to price stability is not so much present fiscal or price and wage policies as exchange-rate policy.

It is argued—especially by F. A. Lutz and E. Sohmen who prepared a special incisive report for the Council which is part of the book—that with the present DM exchange rate and a domestic monetary and fiscal policy aiming at price level stability, West Germany will continue to be a cheap country and thus have recurrent large export surpluses leading to inflationary pressures. The Council believes that the preferable remedy is the introduction of a flexible exchange rate because this would give West German economic policy the necessary freedom of action to cope successfully with domestic price increases. It is said that the danger of extreme fluctuations in the exchange rate is low among countries which try to maintain internal monetary stability. If necessary, the Central Bank could interfere to counter speculative onslaughts.

The West German government in its reply to the report, also part of the book, flatly rejected the Council's main proposal, the introduction of a flexible exchange rate. It argued that present international obligations vis-à-vis the IMF and the EEC make this legally impossible. Yet, even if such legal obstacles did not exist, it would not favor flexible exchange rates. The government holds that fixed exchange rates are a precondition for further economic integration in the EEC as well as the world economy. It is optimistic that other countries will eventually establish enough monetary discipline to make

the system of fixed exchange rates work. The Council is more pessimistic on this issue.

There are probably quite a few economists who feel with the Council and Lutz and Sohmen that at present the DM is still undervalued and that something should be done about it. If not a flexible exchange rate, further revaluation may be required. To advocate revaluation one would have to know the effect of the 1961 change in the exchange rate. Unfortunately there is currently little evidence. The Council states in one place that the adjustment was effective in reducing the balance-of-payments surplus. Other studies indicate, however, that the West German market share in important export markets did not change and that the bulk of the 5 per cent revaluation was domestically absorbed. Interestingly enough, the Council finds in this context that the decrease in the capital share in national income was significant after the revaluation. This would confirm a speculation that, in the present West German situation, exchange-rate adjustments have a great impact on income distribution. The latter may partly explain why the government wishes to adhere to the present exchange rate and why it is adamantly against any flexibility. If capital, because of extreme labor shortage, is likely to bear most of the real burden of an exchange-rate adjustment, the government may opt for moderate domestic inflation under which capital may carry a smaller real burden. Income-distribution considerations and the resulting political pressures could very well make a government reluctant to seek changes in an exchange rate.

Yet, if the market forces were to determine the exchange rate, no individual or political party could legitimately be blamed for any changes entailed in income distribution. In the present West German situation, this may be the strongest case for flexible exchange rates.

The first report of the new Council deals with economic problems of great import, and one may expect future reports to do the same. In view of this fact it would be desirable if the West German Council could be persuaded to publish an English edition of its report.

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Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

The Theory of Exchange. By PETER NEWMAN. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1965. Pp. 201. $7.95.

This succinct, well-written little volume can be highly recommended as a simple, rigorous, and mathematically almost self-contained introduction to the theories of individual preference, revealed preference, bilateral exchange, demand, and group preference. It employs the axiomatic method in clear but uncompromising terms, illustrating the argument at every step of the way with a wealth of diagrams. The substance of the book introduces the student

to the fundamental problems of these fields, and the extensive notes which follow the chapters provide limited but excellent references to the classical and current literature.

In his treatment of the first set of problems, Peter Newman specifies the existence of a complete weak ordering over a field of choice containing nonnegative commodity baskets (Axioms 1 and 2), the consumer's choice of a maximal position in his attainable set (Axiom 3), nonsatiation in all goods (Axiom 4), and the existence of indifference functions which are strictly convex and continuous (Axioms 5, 6, and 7). His discussion of the implications of these axioms includes the problems of boundary as well as interior solutions, dominance, lexicographic rankings, and the existence of a continuous representation of preferences.

One may quarrel with the statement (p. 8) that the definition of the choice domain as a set of goods baskets implies that the consumer's *preferences* are influenced only by goods baskets enjoyed by himself, and that such environmental factors as changes in prices or in others' choices cannot affect his preferences. Since the choice field need not be the only set of factors in the domain of preferences, this does not seem a sufficient axiomatic basis to rule out, for example, the possibility of interdependent consumer preferences or Veblen effects.

Even more questionable, however, is his unfortunate definition of the convexity of a function (p. 33): "Definition 20. A curve in the plane . . . is convex if, given *any* two points . . . on the curve, the chord joining [them] touches the curve only at these points." This is merely a sufficient condition for nonlinearity, and is likely to confuse the student equipped with the usual definition of the convexity of a function. Further, the definition compounds confusion since it is meant to denote a *strictly* convex function in the usual sense, as Newman notes. The confusion arises because Newman does not make clear that Definition 20 implicitly assumes the prior statement of Axiom 6—which specifies increasing rates of substitution for indifference curves—so that the nonlinearity "bends" the curve in the desired direction. Perhaps the motivation for the separation of these two statements was that in dealing with two sets of strictly convex indifference curves in an Edgeworth-Bowley box the inverted set appears to be strictly concave, so the definition may have been separated to avoid confusion in this presentation (see, for example, p. 73).

Newman slips later on his own terminology when he asserts (p. 71) that Axiom 6 guarantees a *unique* point of tangency (or support) of indifference curve and budget line, when in fact it is this axiom plus the implicit assumption of Definition 20 that he was dependent upon.

Chapters 3, 4, and 5 contain an excellent presentation of the theory of bilateral exchange, generalized briefly to more than two commodities and more extensively to more than two traders. The dependence of the equilibrium ratio of exchange between two goods upon the initial distribution of goods, the possibility of boundary solutions to such exchange, the existence of one or more equilibria, and the devices to achieve Pareto efficiency of solutions which are

sequential through time are well handled. Particularly well done is the modern extension of Edgeworth's exchange analysis. It adds to the theory the dimension of coalition-formation and the characteristic of a trade being an element or nonelement in the core-set of an exchange economy. The core-set contains all trades that cannot be subverted by potential coalitions. The enrichment of welfare theory such analysis has introduced, by showing that market prices in the usually assumed ideal environment yield not only feasible (zero excess demand) and efficient (in the sense of Pareto) trades, but also bring about a core allocation which cannot be upset by coalitions, is well developed by Newman.

Some important omissions can be noted in this discussion of barter. The extremely important demonstration by Walras of the key role of a single *numéraire* in all markets in bringing about the arbitrage equilibrium in multiple exchange is one such. Also, most of the modern work on the stability of *systems* for so-called *tâtonnement* and *non-tâtonnement* adjustment processes is grounded in the analysis of exchange economies. A frequently used sufficient condition for the so-called stability of the *tâtonnement* process is that the weak axiom of revealed preference hold for excess demand functions *at the price equilibrium*. Since the weak axiom is discussed at great length by Newman, and since the restriction referred to is implied by strict gross substitutability, positive homogeneity of the excess demand functions, and Walras' Law, it is a pity that Newman did not see fit to add a discussion of gross substitutability and apply his obvious gifts as a teacher to the illustration of this convergence.

Some discussion of the convergence of the market process to a satisfactory solution is contained in these chapters. Moreover, it adds a great deal to clearing up the confusion which plagues modern treatments of these problems. It notes explicitly and more than once the need for the specification of a dynamic response pattern rather than the mere shapes of static functions before the classic stability problem can be analyzed (pp. 92-93, 105). Further, Newman distinguishes clearly between the stability of an *equilibrium* position—which concerns the return to a given acceptable stationary solution from a newly selected initial position—and the stability of a *system*, which involves the attainment of *some* satisfactory stationary state from any feasible initial position. The reviewer regards this clarity as in part a penance, since Newman in a previous work[1] confused the distinction by both assuming the new equilibrium to exist and adopting the system sense of the term "stability."

The "new" stability analysis—both *tâtonnement* and *non-tâtonnement*—is best seen as concerning itself with the *existence* of dynamic equilibria: it is an extension, therefore, of the techniques for proving existence and uniqueness of equilibria in static systems. It asks the question, given a set of feasible data (including an initial position), does the dynamic system yield at

[1] "Suppose now that there is a sudden disturbance such as a rise in the demand for the *j*th good. . . . The aim of stability analysis is to determine the conditions under which the process of adjustment will converge to the new equilibrium price vector, which we have assumed always to exist." ("Some Notes on Stability Conditions," *Review of Economic Studies*, October 1959, p. 1.)

least one solution (i.e., a path through time) which meets certain desiderata? Among these latter are usually stationarity in the sense of limiting values or at least limit points, positivity or nonnegativity of the stationary vector(s), and, perhaps, positivity or nonnegativity of all values on the entire path. Would it not be simpler if we reserved the term "stability" for the relevant characteristic of a given stationary solution, and presented the problems now treated under "stability of systems" as the dynamic analogues of the question of the existence of economically acceptable equilibria in static systems?

Newman's interpretation of Walras' *tâtonnement* process is an interesting, but not an entirely unambiguous, one. In essence, he accepts the second of the two alternative interpretations Uzawa finds in Walras.[2] The first is that of the *tâtonnement* as a simultaneous adjustment of all prices in the directions indicated by the signs of their excess demands, while the second views it as the sequential adjustment of prices most frequently associated with Walras. Newman seems to go beyond this, however, to question if this process was a recontracting procedure, asserting that Walras did not mean it to eliminate trading at nonequilibrium prices in exchange theory or (and here Newman's position is not clear) production theory.

I confess to having felt uncomfortable for some time about the interpretations of the *tâtonnement* process in Walras, including my own. Walras was so desirous of giving the appearance of relevance to real markets to his analysis that I do not believe his references to *tâtonnement* at all points are meant to denote the successive price-adjustment measures of a conceptual experiment. I am inclined to believe that he did at times refer by this term to the realistic process which followed the *prix crié au hasard,* including trade at nonequilibrium prices. This is closer to Uzawa's first interpretation, but does not include the recontracting provision. In my own writing I have followed the "bidding by chits" tradition, but I have not been entirely satisfied that recontracting was involved in Walras' thought. I do not see how much doubt can arise that the *tâtonnement* is primarily designed to cope with the problem of how the general market system arrives at a solution, as Newman urges.

Several other lesser questions arose in my reading of this second portion of the book. The role of the umpire is not clear when he is introduced on page 69: it is not until a page or two later that the explanation of infeasible trades is made by virtue of his possession of stocks of goods from which excess demands can be drawn and to which excess supplies can be added. Generally speaking, in recontracting either party can break the agreement *without* the other party's consent, as Newman's quotation from Edgeworth specifically says in his auction example (p. 68), but which Newman seems to deny immediately above. And, when Newman writes that "no other economist seems to have recognized explicitly the problem of convergence in multiple markets until Samuelson wrote in 1941," he is surely being unfair to Hicks (p. 103).

In a next-to-last presentation Newman constructs an axiom system for revealed preference which is "equivalent" to the axiom system for preferences. It

[2] H. Uzawa, "Walras' *Tâtonnement* in the Theory of Exchange," *Review of Economic Studies,* June 1960, pp. 182-94.

is a model of clarity and insightful exposition that should recommend itself to many teachers of graduate theory. My criticisms are minor. It is not clear to me why the price of savings goods must be one minus the rate of return (p. 132). Were they to be handled most simply as perpetuities yielding one dollar per period, their prices would be one *divided* by the rate of return. Also, the definition of an Engle curve on page 139 seems rather to be the collection of *all* baskets of goods on the frontier of the attainable set rather than that basket actually chosen.

Lastly, the problems of group preference are presented much too briefly, but well. The relations between individual-preference or revealed-preference structures, group-preference or revealed-preference structures, and the aggregation procedures which bridge them, are outlined quite nicely. Newman reproduces Inada's proof of Arrow's nonexistence theorem on aggregation procedures for three goods, and introduces the student to the kinds of difficulties involved. Perhaps it should be pointed out to the author for the correction of the multiple printings the book deserves that on page 187 he refers to Axiom C4 as the axiom of revealed preference instead of the proper C5.

In briefest summary, this little work is a fine introduction into classical and modern exchange theory, and deserves a wide reading in the profession.

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Inflation, Growth, and Employment. A Series of Research Studies Prepared for the Commission on Money and Credit. Contributors: JOSEPH W. CONARD, JESSE W. MARKHAM, FRANKLYN D. HOLZMAN, JOHN W. KENDRICK, DANIEL CREAMER, STANLEY LEBERGOTT, LAWRENCE R. KLEIN AND RONALD G. BODKIN, AND TIBOR AND ANNE SCITOVSKY. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1964. Pp. 470. $7.95.

This volume of "supporting papers" prepared for the Commission on Money and Credit contains eight creditable contributions on inflation, growth, and employment, squarely in line with the volume's title. The essays are not, in general, characterized by original contributions, but are surveys of issues and facts, sometimes stimulatingly opinionated, but most commonly replete with the "ifs," "howevers," and "on the other hands" that characterize the difficulties economists have in really defining policy problems and taking a stand.

In the lengthiest of the essays, Joseph Conard surveys very systematically the causes and consequences of inflation. His is no monolithic position for he finds, in an interesting and eclectic review of factors contributing to short-term inflationary movements from the end of World War II to mid-1959, evidence of a genuine role played by all serious explanations of inflation, of various demand-pull, demand-shift, and cost-push types. As to the consequences of inflation, he strikes in this opening essay a theme for the book: that moderate inflation of the sort experienced in the United States during this post-war period is probably inescapable if unemployment is to be kept at a minimal acceptable level, and that the social consequences of moderate inflation of

this degree are by no means clearly adverse. Hence, given our institutional framework, which introduces a ratchet into the general price-level mechanism if high-level employment is to be sustained, moderate inflation should be found agreeable. He does not, however, feel that moderate inflation, in contrast to price-level stability, is peculiarly conducive to economic growth, apart from its being conducive to full employment.

One of the explanations of inflation, that of market-power or administered-price inflation, is given special scrutiny in the essay by Jesse Markham. His focus is essentially on business rather than labor monopoly and entails the examination of nine industries characterized as belonging in the Administered Price Group and four industries in the Competitive Price Group. Six of the nine highly concentrated industries appear, during the period 1953-59, to have had price behavior much in accord with the dictates of supply and demand, while two industries—steel and automobiles with their unions—appear to have performed each as tandem monopolists. The tenth—the glass-container industry—is left with a question mark. In 31 pages none of this can be conclusive, but it is certainly a responsible interpretation of the facts available.

Research Study Three by Franklyn Holzman considers the use of escalation devices to combat the undesirable redistribution effects of inflation. Thus, Holzman is concerned with a problem that may or may not seem important to other authors in the volume, but a problem of some dimension nevertheless. Escalatory practices are surveyed both by form of income and internationally, and then the economic effects of escalation of wages, bonds, savings deposits, life insurance, annuities, OASI, public assistance, unemployment compensation, and minimum wages are examined (by qualitative common-sense analysis more so than by data—and with occasional uncertainty of conclusion). On wage escalation, equity considerations remain obscure, allocative effects remain obscure, and destabilizing effects are judged to be unimportant, provided (for reasons I found unpersuasive) that wage escalation is not legally compulsory and/or widespread. I felt the fascinating questions were mostly left suspended in mid-air, but if that is where our profession has left them, the author cannot be blamed for honest reporting. His essay concludes, pertinently, with some suggestions for escalating contractual arrangements in which the government has some hand.

John Kendrick's paper on economic growth is a solid survey of the growth concept, its economic causes (briefly treated), the merits of alternative measures of growth, and U.S. growth performance since 1889. I should myself have liked a more extensive discussion of the meaning and justification of economic growth as a distinct policy objective. In particular, I was bothered by the coupling of two notions that may well be incompatible: (1) that in a political democracy the appropriate rate of growth is up to the body politic, and majority rule should determine the government measures necessary to achieve the rate that the majority desires (which I assume to be a rate such that opinion is equally divided as to whether it is too great or too small); and (2) that in a "predominantly free enterprise economy, the growth rate will [here read, should] be determined on the basis of relevant decisions by millions of individuals [in the market place]." I doubt that we can have it

both ways, and upholding the notion of "the composite decisions of the community over time" only slurs over the trouble.

There is a nice survey by Daniel Creamer on capacity and capacity-utilization estimates. Especially interesting are his conjectures about the relative reliability of different published series of capacity and utilization in the United States, for all manufacturing and by industry subgroups. In this connection, he notes:

> The data deficiencies for all available measures of capacity are such that it is difficult to claim one as the best measure merely by evaluating the data and assumptions implicit in the methodology. The results of the various measurement processes must be compared and where different results obtain, appeal to other facts must be made to determine which result appears to be more reasonable (p. 328).

With due recognition of potential partisan bias, Creamer of the NICB finds the NICB estimates "somewhat more reliable than the comparable estimates of McGraw-Hill."

It is a pleasure to encounter Stanley Lebergott's contribution on "The Use of Unemployment Statistics for Monetary and Fiscal Policy," which is written in a sprightly and decisive style. His position cannot be summarized within present space, but he does attempt to answer "six primary questions from the viewpoint of those who must make monetary or fiscal policy: (1) Why consult unemployment statistics? (2) To act or not to act? (3) When to act? (4) How effective has policy been? (5) An unemployment target? (6) Better numbers?"

He assumes that the central authority should respond in a discretionary way to current indicators, finds unemployment series the best available indicators, identifies the readings most worthwhile to take, and defends the two-fold goal of holding short-term unemployment down to 3.5 per cent of the labor force and longer-term unemployment down to 0.5 per cent. Throughout, he advances worthwhile suggestions for improvements in data collection.

The final two essays, by Lawrence Klein and Ronald Bodkin and by Tibor and Anne Scitovsky, present much the same view of the complementarities and conflicts among the goals of full employment, price stability and growth —and much the same view as Conard. Their discussion converges on the following propositions: (1) a high rate of growth is desirable—though there is really little discussion of how an optimal rate of growth is to be determined; (2) full employment and price stability are incompatible, at least within the present institutional framework; (3) the amount of inflation needed to achieve full employment is not excessive; (4) full employment is essential for a greater rate of economic growth; (5) inflation is not—on the record—a necessary condition for a high rate of growth, except to the extent that it is a precondition for full employment; and (6) the rate of growth is best further increased, given full employment, not by rising prices, but by more institutional measures calculated to increase the rate of net capital formation at the expense of current consumption.

Klein and Bodkin consider estimates of the level of employment that must be tolerated for price-level stability. They are very pessimistic, citing estimates of from 4.8 million to 14 million unemployed as necessary in order to

hold wage increases to the rate of productivity increase. Not much detail about the statistical work is provided, but some questions can be raised. Their estimates are obtained from behavior relations intended to explain the determination of the wage rate, with unemployment here regarded as a sort of policy instrument. This is surely not the theoretical structure that would be used by those who would argue for price stability. There are, moreover, problems of the long- versus short-run effects of a price stabilization policy calculated to induce changes in the behavior of trade unions. Furthermore, the regression estimates rest upon an entire scatter, but are used to derive an estimate of the intercept (unemployment needed to produce wage stability) which, away from the point of means, may be sensitive to the linearity assumption. A more careful evaluation of their model and estimation procedure would be needed to dissuade the old-fashioned economist from the view that the unemployment needed to persuade organized labor not to engage in a wage-push process is endurable in both the long and the short run, especially in face of the danger that the monetary validation of administered wage increases may make the process unstable.

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Papers on Welfare and Growth. By TIBOR SCITOVSKY. Stanford: Stanford University Press, 1964. Pp. 274. $6.50.

Though they are not classified in that way, the papers reprinted in this volume fall rather sharply into two series. The first contains those which date from Professor Scitovsky's London period, having appeared for the most part in the *Review of Economic Studies;* a large proportion of the second set appeared in this *Review*. The former are the more severely analytical; the latter are more often expository, though they are distinguished from merely expository essays in that there emerges from them, somewhat by degrees, a characteristic and important point of view.

Of the former, "Welfare Propositions in Economics" (1941) and "A Reconsideration of the Theory of Tariffs" (1942) are of course outstanding. These have passed into the body of basic economic literature; if for nothing else the present volume would be worth having, in order that we might have these classic papers in a more convenient form. "Tariffs" has been slightly re-edited, by the incorporation of a short cut in the argument, taken from the "Theoretical Welfare Economics" of J. de V. Graaf. There is no doubt that Graaf's method is sufficient for Scitovsky's immediate purpose; but the former method, which did not have to assume that the position resulting from the tariff was a Pareto optimum, had in some respects a wider scope. I am myself rather sorry that we have not been allowed to have both.

The still earlier papers from the London period, "Interest and Capital" and "Capital Accumulation, Employment and Price Rigidity," though they are given pride of place in the present collection, have not won for themselves the same established position. They will have to be used with more care. They belong to the same family as Kaldor's writings of the same time (his "Speculation and Income Stability" in particular); the approach which is common

to them all, intriguing though it is, has not (so far) proved very fruitful. Maybe we shall come back to it some day, in some form or other. I do not find much trace of it in Scitovsky's later work.

What is distinguished in that later work (made more explicit here than in his other books, but in consequence illuminating those other writings) is the insistence upon the importance of "increasing returns" in the causation of economic progress—the qualitative as well as quantitative changes which the transformation needed to attain those economies entails—together with a vivid impression of the wide social costs that are involved in that transformation. One of his themes is the power of specialization—admirably represented here by the paper on "Balanced or Unbalanced Growth" (1959), connecting of course with his work on European Integration. For him, as for Adam Smith, the division of labor and the widening of the market are the things that matter. But in his work, as in Smith's, the "contrapuntal serpent's hiss" is also to be heard. What if Progress be no more than Progress into Barbarism? Nowhere, to my knowledge, have the costs of economic Progress *itself* been so well worked out as in the thirteenth essay in this book. It takes an economist to evaluate the limitations of economics. In these days, when communists and socialists (as well as capitalists) have sold their souls to the Steam Intellect Society, the Scitovsky's reminder (for this is a joint paper) is especially welcome. There is more in Welfare than economic welfare; more in civilization than economic growth.[1]

Civilization, however, is better end than means. The Swiftian pastiche (not previously published) that concludes the book is a joy to read; but it will give more pleasure to particular readers than instruction to those who are in need of a sermon on International Payments.

JOHN HICKS

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[1] May I take this opportunity of clearing up a misunderstanding about my own work, which is doubtless my own fault, but which a passage in one of these essays (pp. 180-81) may do something to encourage? I have long been aware that in the heyday of the "New Welfare Economics" extravagant claims were made for the Pareto optimum; and that my own "Foundations" paper (*Economic Journal* 1939) was one of the worst offenders. I had however thought that my "Rehabilitation of Consumer's Surplus" (*Review of Economic Studies*, 1942) from which Scitovsky quotes, marked the beginning of a retreat from that extreme position. If reference is made to the whole paragraph, instead of just the sentence quoted, it will be seen that the *long-run* tendency for "almost all" to be made better off by a consistent policy of Pareto improvement was qualified by the thought of what might happen on the way. I did already perceive (as I would continue to insist) that the issue is to a large extent an issue of time-preference. If we could wait until Domesday, everything might well come out in the wash; but indifference between present and future (here as elsewhere) is not sensible as an economic assumption.

The Economic Theory of "Managerial" Capitalism. By R. MARRIS. New York: Free Press of Glencoe, 1964. Pp. xviii, 346. $7.95.

This is both a very good and a bad book at the same time. Those with an interest in microeconomics, especially the theory of the firm, should certainly read it. The book is good inasmuch as it is a valiant attempt to introduce

many new variables and synthesize many different insights and approaches to the understanding of the firm. It is bad inasmuch as the scope is so great that much of the treatment, although suggestive, is superficial.

The eight chapters fall into three categories. The first six form a book by themselves on an approach to the study of the firm; the seventh is used to muster evidence; and the eighth endeavors to offer possible microeconomic implications of the foregoing work.

Often the basic question in economic analysis is not what is the correct answer, but what is the correct question. Tied in closely with this is what do we want to consider as an economic variable; and what questions concerning the behavior of the firm do we wish to consider as economic questions. Marris, in the opinion of the reviewer, correctly distinguishes between organizational and managerial problems and, more strictly, economics-oriented problems of the firm.

The first chapter attempts to provide a broad institutional framework for the firm, leading off with a discussion of the disappearance of the entrepreneur, the influence of the stock market, and straight into a theory of the raid for a "take-over."

The second proceeds to investigate motives and morals in the managerial structure. The central theme developed is that managers maximize the rate of growth of their firms subject to a security-level constraint. Marris stresses information from other social sciences and touches on a utility system which attempts to reflect features such as power, status, and security as well as wealth. Furthermore, attention is paid to the team aspects of management and the internal sociology of the managerial groups. He suggests the existence of "a norm of professional competence" among managers which is manifested in approval of the ability to organize and expand markets. A distinction is also made between "broad" and "narrow" economic motives where, in the former, "playing the game for the sake of playing the game" or trading money for power are important, whereas in the latter the variables are items such as income, stock options, and bonuses.

The third chapter is relatively brief and deals with concepts and methods for characterizing the firm in a dynamic behavioristic-economic intermix. In order to give structure to his model, Marris touches on the internal restraints on growth, defines balanced growth, and stresses the roles of diversification and merger. Having characterized the firm "as an autonomous organization capable of growth," he suggests that his theory applies "to typical modern developed economies, such as those of the U.S.A., USSR or Europe."

The method of analysis he utilizes is described as "comparative dynamics" where "the analyst first defines the characteristics of the solution and then deduces the implicit economic conditions" with which he varies conditions to study the new dynamic equilibrium.

Chapter 4 deals with "demand" from the viewpoint of the firm, including control of the firm over the growth of this demand via product development and diversification. Free of charge, on page 136, we are offered an explana-

tion of the storage capacity of the brain and its effect on future actions. This is a problem whose solution has to date eluded all neurologists and experts in artificial intelligence and computer theory. Apart from this aberration the rest of the chapter is both sensible and stimulating; it discusses want-creation, the role of the pioneering consumer, chain reactions in consumer behavior, and the effects of social structure. He then develops a mathematical model much in the style of the type of work which is now to be found in Operations Research analysis of marketing problems.

"Supply," which is the subject of the fifth chapter, is basically directed at the supply of finances to the firm. Here he stresses the importance of giving explicit consideration to financial restrictions and sallies into topics which usually are relegated by theorists to the penumbra of corporate finance. These include new issues, retentions, and other ways of financing.

Chapter 6 is the culmination of the work. It is devoted to the construction of two completed micro-models. The first lays stress on problems of management, and the second emphasizes questions concerning growth rate, profits, and, in general, features of more direct interest to economists.

The scope of the book and the abundance of notation is such that at the least a lengthy review article would be necessary to do a detailed exegesis of even a single chapter. Because of the scope, however, the reviewer is in a position to consider as a whole the attempt that has been made at "grand theory building." In this reviewer's opinion, the book fails to construct the grand theory of the firm; but in its attempt to do so it makes an eloquent case for the consideration of many variables and important topics left out of previous attempts by almost all economic theorists. Especially considering that this book originates in England, it is a pleasant change (though not from the point of view of "belles lettres") to see an acknowledgement of the role and importance of marketing. organization, and finance even at the cost of failing to quote "Alice-in-Wonderland" and all that.

The failure to construct the general theory, in this reviewer's opinion, rests mainly on three features. The first is that, given our current knowledge, our ability to identify relevant variables is hardly developed to the point where we can expect a successful synthesis of many diverse poorly understood topics. The second is that Marris' technique of comparative dynamics is inadequate. It is precisely to cope with this type of problem that various behavioral scientists, among them economists, psychologists, and sociologists, have begun to employ simulation techniques to manipulate mathematical models which are beyond the scope of complete mathematical analysis. The final and most important point is that Marris may be looking for his general theory at the wrong level. It is most likely that an interpretation of the firm in terms of organizational flexibility, finance, marketing, the private club of its executives, and so forth is a necessary step in the construction of any satisfactory general theory. However, it may well be that once values are assigned to basic parameters and equations written for different behaviors, the same basic theory will produce different identifiable models which will have growth of

market as a natural goal in one industry and at the same time possibly indicate several other goals for other industries.

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The Economics of Capital Utilisation—A Report of Multiple-Shift Work. By ROBIN MARRIS WITH IAN MCLEAN AND SIMON BERAU. Cambridge: The University Press, 1964. Pp. xviii, 267. $8.50.

In this book Robin Marris of the Department of Applied Economics at Cambridge presents a carefully worked out economic theory of multiple-shift work in manufacturing industries in Great Britain. He points out several times that "this study is mainly concerned with the long-run problem, that is with the determination of planned, rather than unplanned, (capital) utilisation." The question being addressed is essentially this: Given some rate of output required from the plant, what combination of amount of productive equipment and number and arrangement of shifts will (should) be chosen? The theory and empirical research deal with the factors that influence this decision.

The book is divided into two parts, the first explaining the theory and the second presenting the tests and applications. The first chapter places the question of the rate of capital utilization (i.e., annual operating hours of an item of equipment) in the larger context of economic growth. Marris states, "If an economy could gradually be reorganized to permit more intensive capital utilisation, it should be able to achieve a faster rate of progress with a given propensity to save. This means that at all future points in history consumption would be higher than would otherwise have been the case."

The second chapter, "Optimum Utilisation in a Single Process," presents the main theoretical argument, which is mathematically but simply made. The results of the theory can be summarized as follows (pp. 118-19):

> . . . firms would be more likely to decide in favour of high rates of utilisation, if any or all of the following conditions were present, provided they did not offset one another:
> (1) If the utilisation differential (e.g., premium pay) were small relative to the proportionate increase in daily rate of utilisation with which it were associated, that is if the *elasticity of utilisation* were large.
> (2) If the *"real basic wage"*—the ratio of the hourly money wage rate normally paid on single shifts without overtime to the assumed price of the product—were small.
> (3) If the *size of the market* were large.
> (4) If the "production function" implicit in the firm's technical knowledge were favourable (in the sense that labour productivity is relatively high).
> (5) If the proportion of output produced at the peak rate were low.
> (6) If shift-work would yield considerable economies in prime cost, as is the case, for example, in continuous-process industries.

Chapter 3 investigates the influence of "Many Processes" with the conclusion that the single-process analysis essentially holds in the more complex

case. Chapter 4 deals with "The Effect of Output Restraints." Here are covered some of the problems of indivisibilities of fixed capital, management's own resistance to the additional burdens of shift-work, and some comments on fluctuations in output.

This reviewer feels that this coverage of a plant's variation in output is possibly the weakest part of the book. Such variation comes under the phrase "unplanned utilisation" in the discussion, and yet the businessmen who make the scale (and therefore utilization) decision are probably not as concerned with a hypothetical equilibrium rate as they are with seasonal, cyclical, and secular patterns of utilization. Though the book does not choose this as a major source of inquiry, it is pointed out that the Census data used in the tests unfortunately can at best show actual utilization rather than planned utilization.

Part II, which presents the empirical material of tests and applications, is introduced with a discussion of the difficulties of obtaining the appropriate empirical material to test and illustrate a theory such as the one the book presents. Possibly indicative of these difficulties are the two facts that "six years after the Census (1951) was taken, the data became available in the form suitable for econometric analysis," and that the project was started in 1954, and the book was published in 1964. The empirical work consisted of a number of investigations (well-documented by charts and tables): (a) The Census of Production 1951; (b) industrial interviews; (c) the Ministry of Labour Shift-Work Inquiry, 1954; (d) inquiries by employment exchanges; (e) bus services; (f) trade unions; (g) the factory inspectorate; and (h) industrial case studies.

The book uses the Census to bear the major burden of proof. Some of the familiar problems of aggregation and surrogates are faced and partially overcome in the analysis. The theory is largely substantiated by the empirical work or, as the author puts it, "We suggest the theory has not unsuccessfully explained the observed facts."

The author concludes with one reflection about change and two about cause. "In short, as a result of five [*sic*] years' research and reflection the author no longer believes that a miraculous acceleration of economic growth could (or should) be achieved by knocking together the heads of managers and trade unionists in order to persuade them rapidly to convert us into a universal double-shift society." The two reasons which seem to explain to the author the seeming low rate of capital utilization in Great Britain are the constraint of (low) *scale of production* (partially explained by "imperfect competition") and the traditional pay discrimination against women, who work in the day-prime-shift, making the night-male-shift doubly expensive.

This book is well organized and clearly written. For those readers interested in capital utilization, an extension of production function theory, or especially a well-described interplay between theory formulation and empirical investigation, the book is well worth studying.

Edward H. Bowman

Massachusetts Institute of Technology

Models of Markets. Edited by ALFRED R. OXENFELDT. New York: Columbia University Press, 1963. Pp. 367. $10.00.

Two conferences related to market models were sponsored in 1962 by the Graduate School of Business, Columbia University. The first was titled "Appraisal of the Market Models of Price Theory," the second, "Contributions from Mathematical Models to an Understanding of Market Performance." This volume contains the papers and formal discussions of both conferences. A very distinguished list of participants attended the conferences, contributed papers, and prepared commentaries.

The first conference focused on exploring the true worth and function of existing economic models. Here, the papers and discussions made little progress beyond reviewing the dissatisfactions frequently expressed with respect to models of price theory. The second conference, which was to concentrate on new techniques useful in the study of markets and competition, was also somewhat disappointing. While the papers introduce the reader to game theory, simulation, linear and heuristic programming, there is not always a clear connection to the development of an understanding of markets. In summary, then, the conferences and this volume did not contribute significantly to our technical knowledge or our ability to analyze markets through the use of formal models.

More positively, the experiences of the conferees as documented in this volume may well direct future efforts of economists to develop models useful for many more marketing problems. The need for greater content in the treatment of the nonprice elements of demand and the influence of distribution channels were stressed as cases in point. Existing economic models are very general and would often require extensive modification to be of direct value in application. Moreover, these models are usually associated with aggregate analysis. Consequently there is some question whether they provide a very useful starting point in the construction of specialized and more complex models to meet particular needs as the editor suggested.

The message presented in the second half of the book is a brief introduction to new techniques the authors view as promising for the analysis of marketing problems. Optimism has long run high as to what is just around the corner in terms of new research findings and techniques. While optimism is probably a necessary ingredient for successful research and while several of the papers do caution the reader on some of the limitations inherent in the new techniques, the over-all tenor is more optimistic than is probably justified. The search for techniques that might be useful in describing and evaluating market behavior is likely to result in limited success without major emphasis on empirical studies. Pure research on models can provide insight to their characteristics and assist in the development of improved models, but much more significant progress might be expected if this research were coupled with a better basic understanding of market and consumer behavior. Unfortunately, as several authors point out, such empirical work is likely to lag behind theoretical work.

In conclusion, *Models of Markets* may be a useful book for anyone interested in the analysis of markets who has not followed the earlier applied literature in the field and who has not attempted to apply formal economic theory to business problems. The views and conflicts set forth by the eminent group of economists and mathematicians who contributed to this volume should provide a broader perspective needed by individuals setting out to develop marketing models or to apply models of economic theory.

ALFRED A. KUEHN
JOHN U. FARLEY

Carnegie Institute of Technology

Macroeconomics and Programming. By KENNETH K. KURIHARA. London: George Allen & Unwin Ltd., 1964. Pp. 100. 21s.

This book comprises eight loosely connected essays, six of which represent slightly revised versions of previously published papers.

Part I, entitled "Macro Analysis and Policy," consists of four chapters that purport to present "critical discussions of the *recent* debates in the related fields of income-employment, trade cycles, and general prices—with an ultimate view to extending macroeconomic analysis and policy beyond the conventional purview" (Preface, p. 7). In fact the discussion is directed to showing that certain conclusions reached by previous writers may not remain valid if the assumptions underlying their arguments are altered. Since none of the generalizations proposed involves any but trivial extensions of familiar models, the results obtained are neither novel nor interesting. Such policy blood as is contained in the $Y = C + I + G + ?$ turnip has long since been squeezed out, boiled, and spattered about in books published by other "critical" writers.

Part II, "Macro Linear Programming," is intended to suggest "various possible *macro* applications of mathematical programming techniques to optimization problems . . . with a secondary view to forwarding the synthesis of aggregative economic theory and multisectoral input-output analysis" (Preface, p.7). Unfortunately, Kurihara's discussion is jejune and platitudinous. Readers who are not already familiar with the mathematical programming literature will find Kurihara's discussion obscure, and the rest will find it silly.

The book as a whole is an excellent example of the kind of "mathematical economics" that Keynes rightly castigated in the *General Theory*—"concoctions . . . which allow the author to lose sight of the complexities and interdependencies of the real world in a maze of pretentious and unhelpful symbols." Contrary to Kurihara's suggestion, his book decidedly is not to be recommended as "supplementary reading for the students of macroeconomic theory and mathematical programming."

ROBERT W. CLOWER

Northwestern University

Economic History; Economic Development; National Economies

Railroads and American Economic Growth: Essays in Econometric History. By ROBERT WILLIAM FOGEL. Baltimore: The Johns Hopkins Press, 1964. Pp. 296. $6.95.

Challenging what he calls the "axiom of indispensability," Robert Fogel devotes this book to showing: first, that "the combination of wagon and water transportation could have provided a relatively good substitute for the fabled iron horse" (p. 219); second, that contrary to Walter Rostow's "take off" theory, the "inputs required for railroad construction [did not] induce the rise of industrial techniques and skills essential to economic growth" (p. 228).

Defense of the first proposition involves elaborate calculations and bold assumptions in order to measure how much the national product would have been reduced if waterways and roads had been utilized instead of railroads. The methods used, the data uncovered, interpolated, or assumed, and the detailed answers obtained defy brief summary. The author finds that the total savings on inter- and intraregional shipments to market from Mississippi Valley farms by railroad would have been surprisingly small as compared to the savings resulting from utilization of a system of roads and waterways. If the waterways had been extended as projected by the author (about 5,000 miles), and if, as he calculates, land within 40 airline miles of waterways could be profitably cultivated, the total value of agricultural land feasible for commercial cultivation in 1890 would have been only 7 per cent less than it actually was; the social saving due to the existence of the railroads is estimated to have been only $214 million by one method and $175 million by another, or between 1.8 and 1.5 per cent of gross national product. Feasible road improvements would have reduced this saving even further. At this point, without attempting further detailed measurements, the author assumes certain simple relationships and thereby extends his conclusions to comprehend the total traffic movement. Carefully warning that no "firm estimate" can be made, he suggests that the substitution of waterways and roads for railroads might have reduced the gross national product in 1890 by less than 5 per cent.

Admittedly, models have many uses. Their approximation to reality is less important for some purposes than for others. Fogel deliberately designs his model to determine the size of the "social saving" attributable to the railroads. Its usefulness, therefore, will depend to a considerable degree upon its closeness to reality. Certainly the critical reader will be troubled by those instances where vital considerations appear to be omitted or seemingly far-fetched assumptions made. For example, can the relative costs of two systems of transportation be profitably compared without taking into account their joint use for the transportation of persons and things? Revenue from passenger traffic amounted to more than one-third of that received from railroad freight transport in 1890. How can the cost and inconvenience of the increased time

necessary to travel by waterways and roads be measured? Over what route, for example, would persons have traveled between Boston and Minneapolis in 1890 when northern waterways were obstructed by ice?

In the model set up by the author for interregional shipments of agricultural products, the origins, destinations, and volume of internal trade, the shipments from each primary market, and the needs of each secondary one are held constant. These assumptions, the author tells us, result in an upward bias to the calculated social savings of the railroads over the waterways. The pattern of population distribution and of production and marketing of commodities under a transport system using waterways and roads would surely have developed differently and more efficiently if not confined within the particular pattern molded by railroad development. But this raises important questions as to market organization, external economies, etc., which place in doubt the meaningfulness of Fogel's whole linear programming approach. In a section of his final chapter entitled "The Derived Effects," he notes this difficulty and, in my opinion, dismisses it much too lightly.

In challenging the strategic role ascribed to the railroads in U.S. development before the Civil War, the author minimizes the part played by the railroads in the 1840's and 1850's as consumers of iron, coal, lumber, transportation equipment, machinery and manufacturers as a whole. This downgrading of the railroads as an important activating factor in U.S. economic development before the Civil War casts doubt on the role of the railroads as emphasized not only by Rostow, but also by Joseph Schumpeter, Leland Jenks, and others. Fogel subjects to especially detailed examination the assumption that rails were an important factor in the U.S. demand for iron in the years preceding the Civil War. After careful research into the statistics and technology of the industry and after the construction of some ingenious models, he concludes that rails constituted only a small part of the *total* output of the industry. His research appears to add materially to our understanding of what was happening in the iron industry.

His treatment of this problem, however, well illustrates the limitations and possible dangers of the so-called "new economic history." He conscientiously points out that his "reconstruction . . . should be interpreted not as representing the actual position of rails in the market for iron, but as a theory of that position built up from data fragments on the basis of economic principles" (pp. 148-49). He also recognizes that, even though the size of rail consumption relative to pig-iron production is determined, the significance of rails to the growth of the iron industry has not been established. This is true, of course, because the effect on growth of *marginal* additions to consumption has not been determined.

This book merits close attention by those who are interested in the method, approach, and significance of studies devoted to testing accepted historical generalizations by the use of quantitative methods involving econometric models. We should, I believe, welcome these efforts to measure everything we possibly can, to examine the validity of all numerical series and scraps of series and, where numbers are lacking, to experiment with hypothetical constructs. All this Fogel does with confidence and the enthusiasm of a metro-

phile. But unfortunately not all will read him carefully. Quoted out of context and without the necessary qualifications and reservations, Fogel's numerical values may prove more misleading than the qualitative statements they are designed to replace. For many people a number is a number is a number. I predict we shall soon be told that Fogel discovered that the social savings permitted by the railroads in 1890 over water and road transportation was less than 5 per cent of the national product and that the absence of interregional railroads in 1890 "would have retarded the development of the economy by about three months" (p. 47n.). Such unwarranted use of his hypothetical numerical constructs has indeed already begun. A publisher's brochure just received states that by 1890 the railroads "increased the amount of land devoted to agriculture by less than 5 per cent." Standing thus without modification, this statement approaches downright fraud. Model-building in economic history can be positively misleading when its hypothetical constructs are mistaken for reality.

In his final chapter, the author makes a persuasive case for the greater use by economic historians of quantitative method. "One cannot escape," he says, "the ponderous problems of measurement in economic history by embracing qualitative analysis" (p. 239). Granted. But neither can one avoid making value judgments when choosing a particular model or making an assumption. The votaries of measurement need to be reminded that Fogel's admonition may well be reversed to read: One cannot escape the necessity for qualitative judgments by embracing quantitative analysis.

George Rogers Taylor

Eleutherian Mills Historical Library

Iron and Steel in Nineteenth-Century America: An Economic Inquiry. By Peter Temin. Cambridge: The M.I.T. Press, 1964. Pp. ix, 304. $7.50.

Peter Temin's study of the American iron and steel industry, 1830-1900, provides an account which will be of particular interest to those curious about innovation and interested in the analysis of structural change. If one type of refined iron made up a larger share of output, had its demand risen faster or its supply? If supply, had technology advanced, or was there less collusion and a higher tariff? Part I deals with 1830-65 and the battles between cast iron and wrought iron, and between charcoal, coke, and anthracite. Part II shows how the Bessemer process took the lead after 1865 and then lost to the open-hearth. During these decades rails, skelp, and bars also declined compared with structural steel, wire rods, plates, and sheets.

The book's standards of historiography and economic analysis are commendably high, even uncompromising. But an untiring search of the records may exhume facts that do not fit the questions posed and may raise doubts instead of solutions. Temin feels that he cannot make a test of the effect of the tariff on iron and steel production and concedes that his conclusions here are slightly more ambiguous than the ambiguous findings of Taussig decades ago (pp. 213-14). Temin's historical integrity always keeps him from advancing a precise answer where he can only see "the smoke of battle, not its precise outlines." But in such cases something can be said for keeping the exposition un-

cluttered by terms below the level of visibility. Where shifts of schedules cannot be separated from movements along variously elastic ranges, or where a little apparent separation yields no insights that would have been hidden by a simpler account, much of our jargon can be gracefully sloughed off.

Temin has looked through all the nineteenth-century iron and steel trade journals, autobiographies, government reports on the industry, as well as at later books and articles, including unpublished dissertations. He examines the reliability of sources with care, shows exactly where interpolation seems useful, and insists that his reconstructions remain speculative. Where the sum of parts found might yield a misleading whole, he gives an inventory of details from which readers can draw their own conclusions. His explanations of blast furnaces, steel plants, rolling mills, and their auxiliary devices are exceptionally clear.

If more notice had been taken of changing styles of entrepreneurship and of labor relations, it would be a first-class, comprehensive account of the industry. Such an account was not Temin's goal; nevertheless, his explanation of changing prices and quantities calls for greater sophistication about economic behavior. Where information is lacking, as it is for the 1840's, he assumes that the model of competition fits, that "inhabitants of the West were alert to all manner of change," and that it would be "unreasonable to expect a pocket of irrationality or ignorance" (p. 73). Yet he knows that accounting methods in ironworks were no more than the crudest bookkeeping, with depreciation ignored altogether. Later he finds "blindness" in not widely copying British hard-driving of blast furnaces for 24 years after 1855 (p. 158); a "spirit of the age" in the 1890's that made reluctance to compete "peculiarly prevalent" (pp. 190-91); and an urge for a tariff that was "protectionist paranoia" (p. 210). Needed is a more subtle framework than rational versus irrational.

When a scholar has gone through the records and learned the technology of an industry, however, neither an occasional cumbersome addiction to static price theory nor an oversimplified approach to behavior can hold back his contributions. To cite just one example: Temin shows that charcoal did not hold out against coke in the early West because of the character of demand (blacksmiths and farmers), as Louis Hunter found in a well-known study, but because of both the higher cost and lower quality of coke before the development of the Connelsville fields.

W. PAUL STRASSMANN

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A History of Russian Economic Thought Ninth Through Eighteenth Centuries. Edited by JOHN M. LETICHE. Berkeley and Los Angeles: University of California Press, 1964. Pp. xvi, 690. $12.50.

This is a verbatim translation of *Istoriia russkoi ekonomicheskoi mysli* (*History of Russian Economic Thought*), Volume I, "The Epoch of Feudalism," Part I, IXth through XVIIIth Centuries, edited by A. I. Pashkov and published in Moscow under the auspices of the Soviet Academy of Sciences in 1955. The translation differs from the original by the addition of a two and

one-half page foreword by Professor Letiche, who indicates that the original aims to refute "past exaggerations" concerning Western influences on Russian thought, and that the translation aims both to provide "insights" into Soviet economic beliefs and the "flavor" of their particular brand of economic thought. Unfortunately Letiche leaves us then and there; he tells us only that he "shall have the occasion to appraise these and related issues elsewhere."(!)

Against a very laboriously drawn background of "class contradictions"—between peasants and nobles, princes and nobles of all stripes, merchants and everybody else—Pashkov and his collaborators present the scant Russian economic ideas prior to the existence of economics as a distinct discipline. The book shows that Russian economic ideas were then more often than not but a century-delayed echo of ideas predominant in the rest of Europe. In the early medieval period up to, say, 1300, Russian ideas did not differ much from the European ones concerning the merits of labor, the rights of ownership, the obligation of debtors, the usefulness of agriculture, or the wickedness of merchants and usurers. The thirteenth to the sixteenth centuries grant-charters of the Russian princes endowing landowners expressed and formulated as Pashkov himself says "the institution of immunities then in existence in western Europe" (p. 58). Truly enough, in contrast to Western Europe, the Russian centralized state arose on the foundations of feudalism rather than from the ruins of feudal relations. But the few Russian economic ideas of the fifteenth to seventeenth centuries discussed by Pashkov hardly varied from the earlier European heritage concerning the role of the state in respect to coinage, weights and measures, roads, protection of property rights, etc.

Finally, the Russians got infected with the mercantilist theory as a guiding principle of state policy toward the end of the seventeenth century, while the disease had been in force in Europe already by the beginning of the sixteenth century. The biggest mercantilist of them all, Peter the Great, is seriously dubbed by Pashkov as "an outstanding economist" because (like Stalin) he "addressed himself to the elimination of the backwardness of Russia, the creation of a native heavy industry, the development of trade, agriculture, and water transport, and the strengthening of the financial system" (p. 280).

All in all this is an unrewarding book which need not have been presented in this form by such a distinguished scholar as Professor Letiche. Pashkov's history continues over three additional volumes. A more judicial selection from these and related materials might perhaps have better fulfilled the goals set by Letiche in his foreword.

NICOLAS SPULBER

Indiana University

The Origin of Entrepreneurship in Meiji Japan. By JOHANES HIRSCHMEIER. Cambridge: Harvard University Press, 1964. Pp. x, 354. $7.50.

Johanes Hirschmeier proposes to deal with the origin of entrepreneurship in Japan. The book begins with the following quotation from the Bible: "Nor is new wine put into old wine-skins; if that is done, the skins burst, and there

is the wine spilt and the skins spoiled. If the wine is new, it is put into fresh wine-skins, and so both are kept safe." He manages to keep the spirit of this quotation.

Hirschmeier uses the term entrepreneur to denote "all businessmen of the modern type who excelled some way in the fields of industry, trade, or banking." "The test . . . is their actual contribution to the building of Japan's industrial economy." In order to trace the origin of such entrepreneurs he divides the book into seven chapters: "The Merchant Class"; "The *Samurai* Class"; "Rural Entrepreneurship"; "The Initiative from the Center"; "The Spirit of Enterprise in the Private Sector"; "The *Zaibatsu* Builders"; "Fifty Leading Entrepreneurs."

Hirschmeier is extremely skeptical of the so-called historical role of *bourgeoisie* in the case of Japanese economic development. He asserts that feudal society in Japan, unlike its prototype in the West, failed to produce any progressive merchant class to supplant the existing feudal productive relationship. Neither the great Osaka merchants nor the provincial putting-out masters, according to Hirschmeier, evolved into the modern type of entrepreneurs because their acceptance of the feudal mentality made it impossible for them to undertake new ventures. The merchant class, as a consequence, "succumbed to the stagnating influences of tradition and to the rigidities of Tokugawa social policy."

> Entrepreneurship must be viewed as a result both of individual personality and of the favorable economic and social conditions provided by the group and the cultural milieu. If there is too wide a distance between the new goal and the established standards of the group, even strong and talented persons will achieve the goal only with great difficulty. . . . What is to be remembered about the city merchant class of Tokugawa Japan is that its mentality and its culture both resisted change; and this made the emergence of entrepreneurs all the more difficult.

What then is Hirschmeier's thesis? His working model is provided by Alexander Gerschenkron who stresses two necessary conditions for rapid take-off: (1) the growth of internal tension and dissatisfaction, resulting from the fact that some groups in the society come under economic pressure or rise economically but are not able to achieve desired social status; (2) the speed with which industrialization will take place depends upon the sudden realization of the gap that separates the underdeveloped country from the industrialized nations, which can produce a "shock effect." This "shock," in Hirschmeier's view, "can awaken a sluggish people into feverish activity and a firm determination to catch up as quickly as possible."

Hirschmeier observes that a strong internal tension had been generated in Tokugawa Japan because of the economic plight of the *Samurai* and the accumulation of wealth in the hands of city merchants and rural merchant-manufacturers. However, he stresses that underlying discontents could not be mobilized as creative forces so long as the *Bakufu* retained its iron grip on the country.

But, with the visit of Commodore Perry in 1853, something miraculous

happened to this tradition-bound society. Japan suddenly entered, to use Hirschmeier's expression, into the age of "post-Newtonian spirit." The resulting "shock-effect" is described as follows:

> After the opening of the ports, the Japanese people were shocked into a realization of their own backwardness, which had been concealed from them by the official contempt for the Western barbarians. At this point the national capital that consisted in loyalty to the leaders, discipline, intelligence, and capacity for hard work was harnessed for the one great effort: to catch up, and fast. The "New Deal in emotions" had indeed taken hold of the nation and it is the major explanation for the irrational, noncapitalist, dynamic, and romantic approach of the pioneering entrepreneurs.

In Hirschmeier's view, this "new spirit" was strongest in the *Samurai* class. Indeed, he suggests that the *Samurai* were more adaptable to this new situation than any other social class because they were "generally more open-minded, since their educational traditon was formal and philosophical." This is very difficult for us to swallow, but let us note his further observation on the *Samurai*'s spiritual transformation:

> Unquestionably the *samurai* . . . carried out the Meiji Restoration, they also supplied the administrators for the new government. It was the *samurai* who provided the essential dynamic force that turned Japan away from the Tokugawa *status quo* policy and toward rapid modernization. Further, the *"spirit of the samurai"* not only determined official policies but during the Meiji era also became something of a standard public attitude; a happy mixture of militant patriotism and economically rationalized Confucian ethics.

Thus the determination on the part of Meiji leaders to close the gap between Japan and the advanced countries provided stimulus for a rapid progress. It was in response to this stimulus, according to Hirschmeier, that the emergence of the entrepreneurs took its place. Going through the "Who's Who" in the annals of successful Meiji businessmen, he notes the following characteristics: "he loved his work more than money and more than his life; he was a patriot; he hated the prevailing stagnant and conservative atmosphere; he regarded complacency and nostalgic attachment to the past as the worst enemy of progress."

The major shortcoming of Hirschmeier's thesis is his static treatment of the socioeconomic conditions in feudal Japan. Though he recognizes some changes which took place in the feudal period, such, for example, as the growth of money economy and the rise of the merchant class, he seems convinced that the feudal mode of production remained basically intact, so that the society inherited by Meiji Japan was capable of sustaining the rigid feudal traditions. As a result, the nature of social change in Japan immediately following Perry's visit appears to Hirschmeier to be of sweeping magnitude. "Like the castle community in the Sleeping Beauty tale," writes Hirschmeier, "Japan not only awoke, but it came suddenly to realize how much the world had changed during its long sleep."

Failing to interpret socioeconomic structure of mid-nineteenth-century

Japan in the proper historical perspective, Hirschmeier places too much emphasis on the personality factors of Meiji leaders as being mainly responsible for initiating Japan's rapid take-off. These patriotic leaders, according to Hirschmeier, were imbued with the "will to succeed" because they were economically dissatisfied on the one hand and emotionally disturbed by a sudden realization of Japan's backwardness on the other. However, he seems to be overlooking the historical facts that these supposedly "dedicated" Meiji leaders who, in Hirschmeier's view, were also guided by forces somewhat similar to Luther's "calling," represented by the low-ranking *Samurai* whose historical role had been to safeguard the feudal order. They were trained in the Confucian tradition whose basic objective was to sustain the *status quo* of feudal traditions. How could such a class, however frustrated, embody a new mode of thinking with respect to economic matters?

Finally, though it must be admitted that the problem of entrepreneurship in its formative stage no doubt requires treatment in the broadest possible context, neither Hirschmeier's concern with the success stories of Meiji businessmen nor his emphasis upon the importance of personality factors led this reviewer to feel that the book provides much useful insight into the origin of entrepreneurship in Meiji Japan.

KIYOTOSHI IWAMOTO

University of Utah

Economic Growth in a Free Market. By GEORGE H. BORTS AND JEROME L. STEIN. New York: Columbia University Press, 1964. Pp. ix, 235. $7.50.

The authors state that their purpose "is to explain the process of growth that occurs in a free-market area characterized by free trade and free movements of productive services under conditions of full employment" (p. 4). Emphasis is on "explain," and this, in their view, requires an articulated theoretical structure. Consequently, most of the book is devoted to developing a theoretical model of regional development and to delineating the model's consequences. One chapter is devoted to regularities found by previous regional analysts, and empirical data are used, sometimes ingeniously, to test specific hypotheses implied by the model. A final chapter is devoted to a critical discussion of "government policies toward growing and declining regions," largely in terms of their effect on national product.

There has been a rapid increase in income per capita in the United States since World War I, and this national growth has been accompanied by differential increases in income per capita among the states and a narrowing of interstate differentials. On several occasions the authors claim that, except for national currencies and other political barriers, their analysis would be applicable to the differential growth of nations. Almost the entire attention of the work is devoted to the manner in which the national growth has spread among the states

The empirical "regularities" of differential development which the model must explain, for the most part, are familiar. There has been a shift from agricultural employment to employment in the manufacturing and service in-

dustries which is far from uniform among states. The growth of manufacturing has taken the form both of expanding the activities at existing plant sites and of building new plants, often at new locations. The latter is found to be more effective in accounting for the observed growth of specific industries, primarily since the plants tend to be located where underemployed, or even unemployed, resources are available. Among other sectors, the service industries and trade are accorded little direct attention, construction is recognized primarily as the basis of the multiplier effect attending new investment, and transportation is handled largely by assumption. Wage-rate or earning differentials have narrowed among states, largely as a consequence of the differential development of manufacturing and the differential shift of workers from agriculture to nonagricultural employment. On a national basis, the high-wage industries have tended to be the more rapidly expanding industries; at the state level, however, this potential source of increasing dispersion has been offset, on a relative basis, by migration (largely to higher-earning states), an increase in the capitalization of the higher-earning industries with some substitution of capital for labor, and a tendency for individual industries in the lower-earning states to grow more rapidly than would be indicated by their national average rate of growth.

The model treats as exogenous the prices of export goods (i.e., goods sold in other than local markets), the price of capital goods, the rate of return on investment, wage differentials among regions, and the supply of labor (p. 132). Labor is exogenous (since "labor is not the output of an economic activity," [p. 168]) and since full employment is assumed, it often appears to control the rate at which a state grows. They find, however, that a disturbance in the price of export goods, by inducing the investment of capital funds which were saved by residents of other regions, or in changes in the wage differential among regions, can alter the allocation of resources in a region, while a change in the rate at which the labor supply is growing cannot (p. 141).

Regional analysts should welcome the competent performance of the authors in bringing the many facets of differential regional growth under a single theoretical tent. The theoretical tent is that of neoclassical economics and many will find the assumptions on which it is based too unrealistic for acceptance. The authors are aware of the unrealistic nature of these assumptions and they are repeatedly expressed in their baldest form, but they appear willing to accept the implications of "positive economics" (p. 4). The mere fact that so much of differential regional growth can be subsumed under a general economic theory should indicate to regional research workers that examining the operations of the nation's economy is likely to be more rewarding than searching for qualitative peculiarities with which to distinquish one region from another.

Being a general model, and one couched in terms that cannot be observed directly, it leaves many interesting problems unanswered. How, for example, would a firm looking for an additional labor supply choose between the several locations which can supply this need abundantly? Just what is the role of government capital formation in regional growth? To what extent do the level

and growth of governmental services influence the direction of regional economic growth? Do cities become too large to support further growth? If so, is this because people rebel against the living-working arrangements imposed by increasing congestion and travel time or because firms are faced with an increasing cost of services? In dealing with similar questions in their chapter on public policy, the authors drop many of the restrictive assumptions on which their model is based.

Frank A. Hanna

Duke University

Economic Development. 2nd ed. By Charles P. Kindleberger. New York: McGraw-Hill Book Co., 1965. Pp. xvii, 425. $8.95.

This latest addition to the rapidly growing number of textbooks dealing with economic development is an excellent text, but one that is deceptive in some ways. Only 425 pages in length, it covers much ground and generally does so in excellent technical and pedagogical fashion, particularly in the earlier chapters on theory. However, if one compares the topics listed in the index with the actual coverage in terms of data presented and length and depth of analyses offered—especially for the second half—it becomes clear that much of the text's excellence springs from skillful writing rather than from depth of content. Consequently, in the classroom, even with undergraduate students, it would have to be supplemented with other material. Nevertheless, the unusually clear writing coupled with Kindleberger's wide scholarship warrant describing the text as very good.

There are three parts containing 20 chapters and dealing with the theory of economic development (190 pages), domestic policy problems (103 pages), and international issues (94 pages). Richard S. Eckaus has contributed a 22-page "Appendix on Developmental Planning." The analyses, footnotes, and bibliographies (at the end of each chapter) reflect broad research and a great deal of thought on the author's part and contribute materially to the high quality of the text. Even so, as might be expected, especially in so short a book, certain deficiencies exist.

The most serious of these is the text's failure to discuss in any context any aspect of the many controversial developments in Communist China since 1949. China has a population of approximately 700,000,000—more than one-fifth of mankind—which is reportedly increasing by approximately 14,000,000 persons each year. Despite numerous economically signficant developments in and questions about China since 1949, there are only three brief references to China in the text, two of which deal with pre-Communist China and one with Taiwan. Over and above the author's otherwise wide reading, in common with most other specialists in economic development, he seems to be unfamiliar with the recent scholarship of specialists in the field. It is sufficient to note that the topical chapters—"Noneconomic Aspects of Economic Development," "Industrialization vs. Agriculture," "The Population Issue," etc.—are weaker for not reflecting their work. A second major deficiency of the text is the short shrift it gives land reform—only three

pages. Particularly slighted is its critical economic and social significance in Asia, which is to say Japan, Korea, Taiwan, and India as well as China. Part of the problem lies in Kindleberger's narrow view of land reform as consisting largely, if not solely, of land redistribution.

These errors of omission should not overshadow the very large number of positive features of the text. Kindleberger's treatment of the classical, Harrod-Domar, neoclassical, Kaldor, and Solow theories of growth is extraordinarily lucid, as is his discussion of the stage theories of Rostow and Gerschenkron. So too is his discussion of the work of Heller, Abramovitz, Rosenstein-Rodan, Chenery, Galenson and Leibenstein in his chapter on the role of capital. In the chapter, "The Need for Organization," the author makes effective use of the well-known scholarship of Harbison, Gerschenkron, W. N. Parker, and Hirschmann. Still another topical chapter, "The Size of the Market," reflects the basic work of Bishop, de Schweinitz, Spengler, Kuznets, Wallich, Scitovsky, Nicholls, and other familiar contributors to the field. Particularly noteworthy in the author's synthesis of the highly diversified writings of a large number of individuals is his willingness to admit that he raises many more questions than he answers. It is extremely refreshing to come across such phrases as ". . . we don't know . . . ," ". . . it is not clear how . . . ," ". . . beyond the limited powers of the writer . . . ," etc.

It is virtually impossible for one individual or one book, especially a short text, to be up to date or sufficiently informative on all aspects of so broad a field as economic development. The joint task force required by the mission is a team of authors and numerous volumes. Given this situation, it needs to be emphasized that Kindleberger has done a very good job of compressing much information into a small amount of space, and the text warrants high praise.

SIDNEY KLEIN

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Aspects of Economic Development and Policy. By B. K. MADAN. Bombay: Allied Publishers Private Ltd., 1964. Pp. xxiv, 363. Rs. 22.50.

Other than a 16-page introduction that was apparently written especially for this volume, the book consists of a collection of earlier speeches and articles reproduced without evident change. Most were delivered or first appeared in Indian journals during the last 10 years, although a few date from as far back as the 1930's. Despite the title, most of the articles deal largely with Indian problems. B. K. Madan approaches these problems from a varied experience in Indian government and academic life and the presidency of two professional associations of Indian economists.

The introduction is a broad but disconnected summary of the current Indian economic situation, with comments on the broad lines of the future development effort and government policies in a number of sectors. The remainder of the book is divided into three parts. The first, "Planning and Economic Policy," consists of a wide variety of pieces dealing almost exclusively with Indian development, particularly the five-year plans and agricultural policy. The second section deals with fiscal and monetary policy and banking and is almost equally centered on India. The last group, on international economic

policy, centers less on India, including some pieces on international financial institutions, but concentrates on development finance.

With the exception of a few articles previously published in professional journals, most are devoted to a semipopular discussion of (then) current economic issues. Few seem to be intended to present original research, ideas, or approaches of the author; rather, they consist largely of competent summaries of generally informed and enlightened Indian opinion on various subjects. Few of the author's recommendations are either very controversial in India or very explicit. With a few exceptions, few issues are dealt with in any detail. He frequently defends Government of India policies; where he differs, he is usually slightly on the conservative side, especially on questions of fiscal policy.

Not all Madan's views, particularly those that support GOI policies, will or should receive such ready acceptance in the United States. Three examples should illustrate the point.

Although Madan questions the advisability of farm cooperatives in the area of actual cultivation, he sees a "great need for a very considerable extension" of them in the areas of "credit, marketing, processing, supplies, irrigation, farm equipment, etc." (p. 116). That such an extension is not always desirable is suggested by the case of nitrogenous fertilizer, where the cooperatives are often given preference as distributors by the government agencies that handle distribution. If some of the profits accruing to the GOI from this operation were used to provide truly attractive sales commissions to private distributors, the problem of increasing fertilizer sales might be greatly alleviated.

Another GOI policy that Madan defends in several places is the expansion of public sector industry. In doing so, he states that "where outstanding considerations of public interest are decisive (and these are prominent where monopoly prevails), controversy has subsided, e.g., generation of power and even distribution of power" (p. 82). Controversy may have subsided, at least in India, but it is by no means clear that public interest has been best served by the largely public ownership of power and other infrastructure, or that the presence of monopoly should be a "decisive" consideration. The performance of Indian public electric power authorities in terms of the provision of adequate capacity and standards of service has been anything but in the public interest. Frequent and sometimes extended equipment failures have left major cities partly without power for weeks at a time; failure to anticipate demand has led to power rationing, and added one more vexatious problem and delay to the already unnecessarily difficult task of establishing new industrial facilities in many parts of India.

In fields where public interest is not "decisive," he finds that two of the "prime motives" for the extension of public enterprise are "the proper regulation of investment, e.g., steel" and "improvement of standard of service or quality of product, e.g., State road transport or dairy farms" (p. 82). It is not at all clear that either of these purposes have been prime motives (particularly in the case of steel), let alone proper ones for government entry into an industry. Since the GOI declared its intention to enter the steel industry, to use Madan's example, capacity has been either seriously deficient (while

expansion of private capacity has sometimes been denied or made unattractive) or more recently surplus in the case of certain major forms of steel. At the same time (to jump to the next "motive") government price controls resulted until recently in a serious deterioration in the quality of steel. Bus service may or may not have improved where it has come under state control, but in many cases it is more expensive than comparable private service.

To take a third example, although the author's position is a little vague, he appears to support the GOI in its policy of attempting to keep down the price of food grains (with the help of P.L. 480). His principal argument is that output is not responsive to over-all price changes. At least one empirical study would seem to raise doubts that this is the case.[1]

Despite these problems, the book may be of some interest to those not familiar with Indian economics who wish to become acquainted with some of the issues during the last three decades from a largely middle-of-the-road Indian viewpoint. Any regular reader of the *Economic Weekly* will be familiar with most of the viewpoints presented, however.

ALAN CARLIN

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[1] Raj Krishna, "Farm Supply Response in India-Pakistan: A Case Study of the Punjab Region," *Econ. Jour.*, Sept. 1963, *73*, pp. 477-87.

Capital Formation in West Germany. By KARL W. ROSKAMP. Detroit: Wayne State University Press, 1965. Pp. 287. $12.00.

This book examines capital formation in West Germany since the currency reform and comments on its structure and its implications for the growth of the economy. It designates capital formation as the key problem of the West German economy after 1948. Reconstitution of a functioning economic system and fresh growth depended on the creation of new capital assets. They were created and the economy recovered and grew. The book brings together a good part of the story of how the Germans did it.

Karl Roskamp points out that individual and business savings together contributed only about one-half of the net capital formation from 1948 to 1960. Government contributed the other half. Government spending and lending for housing construction, public construction, public enterprises and the fiscal support of private investment made this large contribution to capital formation in the presence of a dominant government preference for the market system as the structural basis of the economy. In the process, the government appears to have administered a correction to the income distribution through some redistribution of benefits from higher and lower income groups, thus mitigating a strong inequality of incomes by size.

This investigation leads the author into a discussion of many of the peculiar features of the German postwar economy, the aftermath of war and military occupation, institutional and behavioristic traits of households, business enterprises and the governmental system. The discussion is academic and bland, touching rather lightly on structures and viewpoints (including those of the members of the "Freiburg school"). Roskamp considers specific rea-

sons why individuals and businesses did not save even more than they did, and why government could afford to save as much as it did. But he does not bring out significant alternatives of capital financing before the country in those years, a difficult task to be sure, but probably an indispensable one if the choices made are to be understood and certain general conclusions are to be drawn. In the matter of income-distribution effects, Roskamp talks about relations of income inequality to growth and social tensions, but does not really come to grips with the problem, either theoretically or through an analysis of public debate on the issues in Germany.

Roskamp is perfectly right in concluding that the German experience shows that a high proportion of public capital formation via taxation can be compatible with the working of a vigorous market economy, contributing to its growth rather than stifling it. He may also be right in pointing out, on the basis of rather scanty data for 1950, that German public financing contributed to "equity" and thereby to the social acceptability of market economy. (The present reviewer suggested as much in a little book published in 1955.) But in calling these observations "lessons" Roskamp fails to make clear for whom, and under what conditions, the German lessons would be relevant, say, in the United States, Britain, Scandinavia, South Korea, or elsewhere. To do so would have required a far more incisive evaluation of the German postwar situation in the light of conditions elsewhere, a more incisive treatment of the interaction of public investment and market economy, and of the problem of "equity" which cannot be simply identified with the degree of inequality of the distribution of income by size. For lack of that, the lessons sound like rather vague contributions to an ideological discussion of "public versus private" capital formation. Historical German data, say, for the 1920's, might also have helped to put the postwar experience into perspective. Marschak's and Lederer's 1936 book on *Kapitalbildung* is not mentioned.

All in all, the topic to which this book addresses itself might have been investigated more effectively if the present empirical content had been treated more concisely (some of the statistical tables are not very interesting), if more empirical data below the macrostatistical level had been considered, and if the questions to be dealt with had been sharpened. As it is, the book plods bravely and modestly to the point where some really interesting questions begin to open up, and there it stops.

HORST MENDERSHAUSEN

The RAND Corporation

Statistical Methods; Econometrics; Social Accounting

Spectral Analysis of Economic Time Series. By C. W. J. GRANGER WITH M. HATANAKA. Princeton: Princeton University Press, 1964. Pp. xvii, 299. $8.50.

In view of the fact that most economic data appear in the form of time series, any book which deals lucidly with recent advances in mathematical

statistics relevant to the analysis of economic time series is a must for the quantitatively oriented economist. Such a book is Granger and Hatanaka's *Spectral Analysis of Economic Time Series*.

This excellent volume is extremely valuable for both the professional and for the initiate in economic spectral analysis, and indeed is the best existing exposition of the subject from the economist's point of view. While familiarity with Fourier analysis is a prerequisite, the degree of mathematical sophistocation presumed by the authors is not great. Throughout, proofs of the more difficult theorems are omitted and, whenever possible, heuristic interpretations of results derived on more rigorous bases are provided.

The book begins with a brief introductory section which includes a discussion of classical approaches to time-series analysis. The volume is divided into two parts. The first portion deals with spectral theory evolved under the assumption of stationarity, and the second relaxes this assumption to some extent.

In the first portion of the book the formal theory of spectral analysis of stationary time series is formulated, followed by examples of its application to relatively simple economic problems. Then, the concept of cross-spectral analysis is introduced, developed, and applied in a similar manner. Finally, an excellent discussion of feedback between pairs of economic variables and of applications of spectral techniques to the detection of this type of interaction brings the first part of the book to an end.

The second half is devoted to an extension of the theory of spectral analysis to stochastic processes which are nonstationary in the sense that they possess a time trend in either mean or variance (or both). Since most economic time series are much more realistically described by these kinds of postulates than by the more usual stationary representation, this portion of the book contains the real meat of the subject for the interested economist.

After development of the formal theory, the techniques of nonstationary spectral analysis are applied to the familiar problem of business-cycle indicators (Ch. 12). Basically, the results obtained by cross-spectral techniques are in good agreement with the NBER work, except that the duration of the average lead or lag appears to be significantly longer than that derived by NBER methods. Chapter 13, which deals with tests of the validity of alternative theories of inventory formation (unfortunately, with inconclusive results), is also of great interest.

As is usual in a work of this size and scope, one can find a small number of technical inaccuracies. Thus, the asymptotic variance of the estimates of the spectrum is stated incorrectly (p. 45) to be 4/5 instead of 3/4. On page 60 the asymptotic correlation of spectral estimates in adjacent frequency bands is given at 1/4 (the correct value is 2/3). And, on page 61, it is said that the Parzen window never produces negative estimates, a contention which this reviewer can refute with a large volume of experimental counterevidence! On the other hand, these minor errata are far more than counterbalanced by the way in which the book interprets the ideas of spectral analysis in economic terms and illustrates their applicability to real economic problems.

The substantive results obtained in economics to date by the application of

spectral techniques have been somewhat disappointing. In large measure this is because, in most economic studies, the derivation of the spectrum has been viewed as the end product of the analysis rather than as an intermediate step in, say, regression analysis. As a consequence, the questions asked with the aid of spectral techniques have dealt primarily with (1) the relative importance of cycles of various frequencies, (2) the appropriateness of methods employed to remove unwanted frequencies, and (3) the nature of the lead-lag relationships between various time series. The answers to these questions have, not surprisingly, excited little interest.

One attempt to break out of this circle of pedestrian question, pedestrian answer, and disappointing result is in the last chapter of this book. There spectral techniques are used to test the validity of one form of the acceleration principle of inventory formation against two alternative theories. Unfortunately, analyses of this character have been few and far between. Furthermore there have been no published attempts to combine a priori models with spectral methods. But it is in these more imaginative uses of spectral analysis, in which theoretical models are specified and used in combination with the largely empiricist spectral approach that the major usefulness of the techniques presented in this book will ultimately be found. Indeed, one can go one step further and assert that the techniques of spectral analysis are sufficiently flexible that one can profitably modify the constraints on the spectrum to conform with specific theories. In any event, the economist who intends to do anything in this field would be well advised to read this book first.

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[1] I am indebted to E. J. Hannan for a stimulating discussion concerning this book prior to writing this review.

Studies in the National Balance Sheet of the United States. Vol. I. By RAYMOND W. GOLDSMITH AND ROBERT E. LIPSEY. Princeton: Princeton University Press, 1963. Pp. xxv, 433. $8.50.

Studies in the National Balance Sheet of the United States. Vol. II. By RAYMOND W. GOLDSMITH, ROBERT E. LIPSEY, AND MORRIS MENDELSON. Princeton: Princeton University Press, 1963. Pp. xx, 531. $7.50. Set, $15.00.

These are useful volumes to have around. The first contains descriptions and analyses of the national balance sheet of the United States since 1900, although the emphasis is on the shorter period, 1945-58. There is also an interesting study of the influence of price changes on net worth, and a detailed look at the role of housing in the national balance sheet. The second volume has a long introduction on sources and methods of national balance sheet data, but primarily it presents the basic data, in table after table, which underlie the studies in Volume I. Or, rather, it presents all the data, only a tiny bit of which is actually used. So, the data are there, largely unused and waiting to be regressed.

There are so many findings in just the first part of Volume I that it is really impossible, in a short review, to do them justice. However, the table below presents a few of the principal results, and these in turn will suggest the various paths which the authors pursue.

National Balance Sheet
Selected Years Since 1900
(in $ billions, current prices)

	Assets				
	Total	Tangible	Intangible	Liabilities	Net Worth
1900	157	88	68	45	112
1912	306	165	142	91	215
1922	645	322	323	217	428
1929	973	423	551	316	658
1933	722	319	403	272	450
1939	863	376	487	346	517
1945	1533	555	978	778	755
1951	2439	1123	1315	1008	1431
1958	3735	1653	2082	1488	2247

National assets, and especially intangibles, have increased 25-fold since 1900, which is somewhat more than the growth of GNP over the same period. (By now, total assets are substantially in excess of $4 trillion.) In the recent postwar period, national assets grew at an annual average rate of 7 per cent, which was only a shade higher than the average from 1900 to 1929; intangibles did not rise as fast as tangibles, reflecting the elimination of excess liquidity which existed in 1945; liabilities increased more slowly than total assets; and net worth grew at a faster pace than it did in earlier years. With regard to this last point, Part 2 of this volume is devoted to an analysis of how price level changes have affected net worth in national balance sheets.

The authors also study the components of these data. A few of the important findings for the postwar period are that, within tangible assets, producer and consumer durables recorded relative gains while land and residential structures (the latter discussed at some length in Part 3) showed relative declines; within intangible assets, primary securities (especially the market value of corporate stock and mortgages) gained relative to indirect claims, which reversed the long-term trend; and, finally, private liabilities rose much faster than government debt.

Volume II, in addition to the useful introduction by Robert Lipsey, contains national balance sheets for selected years 1900-1945, and then annually through 1958; postwar balance sheets of the several sectors; data on individual assets and liabilities; and flow-of-funds data—through sectors and by transactions—1946-58 (prepared by Rachel Floersheim).

It took a lot of hard work, ingenuity, and flights of fancy to come up with these data. This is clear. And if the authors were too exhausted after such an effort always to tell us the significance of the figures, well, that is understand-

able. Others may now take up the task. These mountains of data should be scaled, if for no other reason than that they are there.

JOHN G. GURLEY

Stanford University

Experiments with Input-Output Models—An Application to the Economy of the United Kingdom, 1948-55. By A. GHOSH. New York: Cambridge University Press, 1964. Pp. xv, 148. $8.00.

The monograph describes experiments by the author based on the 1948 British input-output table for 1948 prepared at the Department of Applied Economics at Cambridge. It is part of the much-needed effort to evaluate input-output in comparison to other forecasting methods. The author is to be commended on undertaking a task of considerable magnitude. Chapter 5 is perhaps of the most general interest as it focuses on the comparison of output forecasts by input-output and three alternative models, namely regression, final-demand blow-up, and GNP blow-up, concluding (p. 48) that "input-output does better on the whole than any of the above methods, both in the two-year and in the seven-year analysis." It might be mentioned, however, that input-output provided the *best* prediction in only 17 of the 47 industrial categories.

Chapter 6 is concerned with the "aggregation problem." This chapter concerns itself exclusively with the *size* problem and not the qualitative features of aggregation. The author's conclusion that "the aggregated matrix multiplier projects a smaller output because of the elimination of the inter-industrial elements" (p. 60) could only result from the neglect of intra-industry transactions, since, if these transactions are included, the base-year bill of goods would automatically give identical output in aggregated or disaggregated form. The results of this chapter are (perhaps necessarily) not conclusive. Chapter 7 indicates that "a considerable amount of simplification" may be introduced by estimating input-output coefficients for industries where the sales are an unimportant part of the seller's output as a proportion of the seller's output. In the concluding note to this chapter, the author examines the suggestion (Morgenstern and Whitin) that the results of very highly aggregated matrices (say down to three or four industries) will be valid only if there is a "relatively self-contained character of the groups of industries." The experiments cited were not based on this self-contained property—and indeed could hardly be—when consolidation of *all but two* industries of the largest available aggregation was carried out.

Chapter 8 is based on the author's "augmented matrix multiplier." In this model the purchases of nondurable goods by final users are subtracted from final demand and are proportionately related to total factor costs. The demand thus subtracted amounted to over 60 percent of exogenous demand. The justification for this model is that "there is every reason to think that the everyday needs of the household for non-durable consumers' goods and services move in a simple relationship with income" (p. 79). Comparisons were made based on homogeneous and nonhomogeneous consumption rela-

tionships. The results were that the errors with the nonhomogeneous form were higher than the conventional form and those of the homogeneous form still higher. However, Ghosh argues that "while we may increase the error by augmenting the matrix, we shall reduce it considerably by having less final demand to estimate" (pp. 85-86).

Some experiments were carried out with a nonhomogeneous model where the input-output coefficient matrix was based on the ratios of input *changes* to output *changes* between 1948 and 1950 rather than the conventional dollar input per dollar of output form. This approach resulted in five *positive* input-output coefficients (interpreted as a decrease in input associated with an increase in output). The author ascribes this result either to technological change or to errors of measurement. In some cases, the difference in particular a_{ij} coefficients was extremely large, sometimes varying by a factor of ten from the corresponding conventional form coefficient. Nevertheless, in comparing estimated production indices with observed indices, "the nonhomogeneous model seems to give better results . . . in all the manufacturing and trading industries, while agriculture and building are better represented by the homogeneous model" (p. 106). The author concludes that it would be worth while to develop the nonhomogeneous model if it had a decided superiority in the case of manufactures. Present data are not adequate to determine whether this is the case.

It was interesting to find one chapter devoted to price-projection models, since output projections have received much more attention in the literature. The experiments led to the conclusion that: "Generally speaking, input-output models are far less efficient for price projections than for quantity projections" (p. 109).

Chapter 13 suggests that an "allocation model" could be used to estimate input-output coefficients in time of shortage, i.e., that "the input from the k^{th} industry to the j^{th} industry is a constant proportion of the output of the k^{th} industry . . . changes in final demand must be met by adjusting the production coefficients" (pp. 113-14). Ghosh suggests that the conventional model tends to underestimate the possibilities of the production system while the allocation model would overestimate them. It seems to the reviewer that the conventional model could either underestimate or overestimate the production possibilities, depending on the level of aggregation, a highly aggregated system leading to overestimation through its implicit assumption of perfect substitutability among components of the bill of goods in each industry.

The book provides useful experiments in an area where they are badly needed. He has also suggested several new and interesting hypotheses and has pointed out various areas for additional experiments to test these hypotheses. The author is to be commended for carrying out an arduous task with care. Nevertheless it is regrettable that he did not take into account various previously published studies that are extremely relevant, in particular Michio Hatanaka's *The Workability of Input-Output Analysis*, published in 1960 (Ludwigshafen am Rhein). A comparison of the empirical results would have been of interest, aside from the considerable contribution of Hatanaka to the conceptual aspect of the problem, which should not have been ignored. Also,

K. J. Arrow and Marvin Hoffenberg's *A Time Series Analysis of Interindustry Demand* (Amsterdam: North Holland Publishing Co., 1959) was conspicuous only by its absence from the bibliography.

T. M. WHITIN

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Economic Systems; Planning and Reform; Cooperation

Central Planning. By JAN TINBERGEN. New Haven and London: Yale University Press, 1964. P. 150. $5.00; 37/6 s.

This is a very important book, but not a good one. The field may be described as "comparative planology," a new and rapidly growing branch of economics which threatens to absorb all other branches. For nearly all governments now plan—no modern state feels well dressed without it; and it is hard to imagine a part of economic science that need not be, indeed is not, taken into account. The subject is also extremely interdisciplinary: half economics, a quarter public administration, a quarter all the other social sciences.

The faults of this book are three: excessive brevity, the neglect of everything that is not economic (though there are some "public administration" passages, they are very jejune), and the generally incomplete, inaccurate, and naïve presentation, which a little more trouble could easily have corrected. Its enormous importance lies in its pioneering quality. The mere fact that one wishes it longer speaks volumes. Tinbergen has rushed in where angels fear to tread; and to say this is simply to criticize the angels. However quickly his book is overtaken by others longer and more scholarly, it is a great landmark.[1]

The *pièce de résistance* is the response to a questionnaire sent to central planning agencies throughout the world. It is easy to make fun of this effort. The answers were sent by post, without personal interview. The questions were absurdly ill-adapted to Communist planning: Table 8, on "The Main Means Used In Planning," incredibly omits the straight legal command to produce this or that. The questions were also misinterpreted by various respondents: e.g., the Polish agency denied that one of its functions was to make forecasts (exports? agriculture?); the Czechoslovak agency answered an obscurely phrased question about training on the job for its employees as if it had been about prior degree courses.

Worse still, the author has incorrectly summarized even the answers he has; saying on page 35 that only Communist governments engage in "directive" planning, when in Table 5 Ecuador, for whatever reason, also claims to do this, and Bulgaria does not. No doubt this Ecuadorian claim rests on a pardonable confusion as to the meaning of "directive": evidently the author had the Communist command economy in mind, but did not say so, or put it

[1] For other literature in "comparative planology," cf. "Action under Development Plans," an occasional paper of the Combined Administrative Group, International Development Research Center, Indiana University. This paper was edited by Bertram M. Gross, August 1964.

in Table 8 where it belonged. Or on a lower level, it is stated (p. 37) that no Communist country answered the question on subdivision by sectors; yet in fact Hungary did (Table 9).

Let us hope for a much more careful and sophisticated revision, such as the author himself is entirely capable of giving us. This revision would, first, have to re-do the questionnaire on the spot in long interviews with many people; the postal method could scarcely ever have been less appropriate. The answers would reflect the interviewer's own mature opinions. Second, the whole study would be based on far deeper reading in Communist economics, with which the author is extremely unfamiliar. For instance the absolutely crucial section on "The Optimal Régime" (pp. 82-87) comes down flatly—but oh how discreetly!—against Soviet-type institutions; but does not explain why.

Third, it would not direct itself simply to the official central planning agency, which is so often unimportant or even (e.g., Mexico, the United States) nonexistent; but to what Professor Gross has so aptly called the "central guidance cluster"—which includes the heads of other government departments, the cabinet, the party leaders in single-party régimes, and the heads of public and major private enterprises. For instance in non-Communist countries it is ludicrous to speak of planning without reference to both the central bank and the minister of finance, who are by no means under command of the central planning agency—or of each other! Again in France the major trade associations are just as much a part of the planning agency as the civil servants concerned.

A fourth point to be observed is that very many governments "command" their own departments and even the nationalized industries, but "persuade" private enterprise and local government. Nay more, "planning" often means simply co-ordinating all the government departments; and this is no negligible task. No book on "comparative planology" should fail to bring this out.

Finally the ideal second edition would avoid the heavy pro-planning bias shown by the first. It would stress that erroneous planning is very likely to happen, and is often worse than *laissez-faire*. It would avoid implicitly sanctifying planners' preferences by such a sentence as: "The benefits taken into account should correspond with the aims of development policy and the costs with the use of scarce production factors" (p. 28). What about the aims of ordinary people? Suppose they said to hell with development policy?

It would not, again, flatly state, as if the matter were not controversial, that the market cannot rationally allocate things subject to increasing return; or that it cannot cope with "roads, education and information" (p. 85)—when it obviously can. The pure economics then, for all its high level and occasional brilliant simplicity, is marred by what, in a less gentle and persuasive author, we should be forced to call dogmatism.

Planning is something we *have*, in virtually every country, already. It will doubtless spread to all the others, whatever anyone says. But it still needs critical study, not mere acceptance. *Academia* is and ought to be the home of lost causes.

PETER WILES

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Crisis in World Communism: Marxism in Search of Efficiency. By FRANK O'BRIEN. New York: Committee for Economic Development, 1965. Pp. 191. Paper, $2.75.

This new CED book is not so much a study as a lengthy editorial with illustrative facts and statistics. While some of its arguments are defensible, there have been a number of better discussions of the subject, even at a popular level. Certainly it has none of the depth and care of the well-known *Comparisons of the United States and Soviet Economies* done by the Joint Economic Committee of Congress in 1960. It is worth exploring here only because it exemplifies an approach to the study of Communism which has created much mischief, both in the scholarly area and in the realm of practical politics.

Crisis in World Communism is divided into three parts. Part I, "Marxism in Search of Efficiency," deals with the goals and problems of the Soviet economy, including the recent tendencies toward greater reliance on the market. Part II, "Communist Man and His Marxist World," explores Communist doctrines and programs, and examines the working out of Maoist Communism in China. Part III, "The Strategy of Competitive Coexistence," deals with the question of Soviet economic relations with the West and with underdeveloped countries.

Perhaps the most grating aspect of this work is its incessant emphasis on "Marxism." This word, or a variant, occurs several times per paragraph through most of the book. Although the author states that he uses "Marxism" only as a convenient label for the contemporary political structures of Communist countries, he frequently lapses into using it to mean the actual doctrines of Karl Marx. Here his ignorance is painfully apparent. Veblen's technocracy, Mill's notions on distribution, and Lenin's theory of imperialism are all ascribed to Marx. The old chestnut that Marx believed in predestined stages of economic development which all countries—with emphasis on Russia—must go through, is trotted out for another encore. Marx's directly opposite assessment of the Russian situation, in a letter written in November 1877, has been available in English for at least a quarter of a century, and has been reprinted in many collections, including a recent paperback edited by Lewis S. Feuer. But myth goes marching on.

When the author reverts to his original definition of Marxism in terms of what present-day Communists say and do, he argues that their behavior is determined by Marxism—with no apparent embarrassment at the obvious tautology. From this he proceeds to depict Soviet maneuvers in international power politics as the unfolding of a philosophy rather than the pursuit of national advantage. Again, none of this would be worth noting if it were not so widespread. It is ironic that those with the most abysmal ignorance of Marxism seem prone to see in it the explanation of all Soviet actions.

Comparisons of the U.S. and Soviet economies are on the same level of superficiality. The term "efficiency" is used uncritically to mean output per man-hour. The author thus shows that the U.S. economy is more efficient—just as Soviet economists have used their country's greater output per unit of capital to show superior efficiency by *their* definition. The author makes no

attempt to develop measures of efficiency reflecting something more than the relative scarcities of capital and labor in the respective countries.

In the final section, there is a strong argument against expanded trade with the Soviet Union. This argument is permeated with a mercantilistic conception of trade from the point of view of individual producers rather than the consuming public. For example, we should oppose "large scale expansion of Soviet petroleum sales" because such expansion would be "at the expense of markets developed by United States (and other Western) oil companies." It never seems to occur to him that consumers might benefit, or that depleting the USSR's natural resources might be preferable to depleting our own.

Although a considerable amount of space is given to debunking Soviet economic claims, the author omits the important point that the gap between U.S. and Soviet output is widening. This omission is particularly unfortunate in the light of the "growthmanship" literature which manages to suggest the opposite with percentage extrapolations on logarithmic scales.

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Money, Credit, and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

Private Financial Institutions. By PAUL M. HORVITZ; DEANE CARSON AND PAUL H. COOTNER; THOMAS G. GIES, THOMAS MAYER, AND EDWARD C. ETTIN; LAWRENCE L. WERBOFF AND MARVIN E. ROZEN; FRED H. KLOPSTOCK; AND E. GORDON KEITH. A Series of Research Studies Prepared for the Commission on Money and Credit. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963. Pp. 460. $7.50.

The studies reported in this volume are the second of two series prepared for the Commission on Money and Credit's task force on problems of the private financial system. The first set was published under the title, *Private Capital Markets.* In the present volume, each study focuses upon a major policy-related operating feature of our intricate system of private financial intermediaries.

Here is a list of the studies: Paul Horvitz, "Economies of Scale in Banking" (pp. 1-54); Deane Carson and P. H. Cootner, "The Structure of Competition in Commercial Banking in the United States" (pp. 55-156); T. G. Gies, Thomas Mayer, and E. C. Ettin, "Portfolio Regulations and Policies" (pp. 157-264); L. L. Werboff and M. E. Rozen, "Market Shares and Competition among Financial Institutions" (pp. 265-332); F. H. Klopstock, "A New Look at Foreign and International Banking in the United States" (pp. 333-82); and E. G. Keith, "The Impact of Federal Taxation on the Flow of Personal Savings through Investment Intermediaries" (pp. 383-460).

Each of the studies is empirically based. In some cases data are cited for 1961 and 1962, but in a number of instances time series end at 1960, as in the detailed analysis of costs and efficiency in Horvitz' study and the exami-

nation of market shares and competitive position of the various financial industries by Werboff and Rozen.

Taken as a whole, the studies meet the standard of care and solid competence that we have come to expect of the staff work and research done for the Commission on Money and Credit. Each study has analysis and policy judgments that will be of interest to anyone seriously concerned with the financial structure of the American economy.

Horvitz examines, first, the general relation of operating cost, and its components, to bank size. Then, analyzing Federal Reseve data for 1959 on the composition of selected unit and branch banks in various size categories, he concludes: "Branch banks have a higher loan ratio and higher relative amounts of instalment and mortgage loans. Size was not a very important determinant of the loan ratio. Because of the high loan ratio, branch bank income was higher, but expenses were also higher than unit banks with the result that net earnings of branch and unit banks were nearly identical" (p. 39).

Horvitz directs his economies-of-scale evidence (which shows a slight margin of advantage to size) to the policy issue of branch banking versus unit banking and says: "The argument that branch banking must inevitably lead to monopoly is untenable" (p. 52). He also notes that "the validity of the argument that branch banking means more banking facilities for small towns is not dependent on cost advantages for branch banks. Branches can exist in communities where unit banks could not operate profitably even without any cost advantages" (p. 52).

The failure to discover significant economies of scale in banking tallies with the findings of Grebler and Brigham for California savings and loan associations. It appears, however, that the full story is not yet available. The technology of financial institution management is changing rapidly, both in the spread of computer methods and in the adoption of improved management techniques. We will be able to move beyond the present level of understanding of these efficiency problems as the dynamics of the financial intermediary firm are investigated.

There are, of course, difficulties associated with the problem of rationalizing the banking structure through branch expansion, merger, and other means of causing the disappearance of uneconomic units in the industry. One of these difficulties relates to the clouding of merger approval powers of the regulatory authorities by reason of the recent antitrust decisions.

Carson and Cootner in their study of banking structure comment that the commercial banking population "now shows signs of stabilizing at its present level" (p. 75). From 1940's population of 14,534, the number of banks fell to 13,460 in 1959, as mergers and other disappearances outnumbered new charters. One might enter the cautionary comment that significant changes in state or federal law and regulation, with respect to branching and mergers, might have major consequences toward further rationalization of the structure.

Carson and Cootner demonstrate that asset concentration of metropolitan-area banking has been, and is, substantial, with four or five banks dominant

in urban centers. They also conclude, comparing 1939 and 1959 data: "the concentration of commercial bank assets in these metropolitan areas has tended to decline . . ." (p. 82). The characteristic pattern is that of oligopoly in urban banking.

Carson and Cootner then undertake an illuminating analysis of the pattern of bank earnings and before- and after-tax returns on capital in the postwar period to 1960 and a penetrating discussion of concentration and profitability. Small banks did relatively better than large ones as to pretax yield on equity in the early postwar years and the position reversed in later years. *After*-tax yields on equity showed quite sharp convergence in the later 1950's for all size classes (cf. Chart II-8, p. 104).

To deal with the relation of concentration to bank profitability, Carson and Cootner use state-by-state comparisons of 1959 data. "The nine states," they say, "which fell into the most concentrated group show an average rate of return on capital of 22.5 percent in 1959, while the least concentrated group earned only 16.0 percent. The middle group earned an intermediate 18.1 percent." They go on to point out, however, that seven of the nine most concentrated states are also in the Far West, marked by rapid population expansion and economic growth.

As to chartering policy, Carson and Cootner recommend down-grading the survival criterion (both for the proposed new bank and for its existing competition in the market in question) and increasing the attention given to comparisons of management competence. Curiously, however, there is no mention of initial capitalization as an important screening factor and public policy control.

Branch-licensing policies are inhibited by restrictive legislation in many states, and national bank licensing, by law, follows such state restrictions. Carson and Cootner point out two defects of branch-licensing policies in many of the states where branching is permitted: (1) the tendency of the authorities to require the same economic tests for new branches as for new unit banks, despite the more relaxed conditions of viability which would be satisfactory for branches; and (2) the frequent failure (especially in states proscribing branches or permitting them only within a limited radius from the bank's home office) to encourage competitive interplay between good-sized banks by interlacing their presently separate market areas with branches of both organizations rather than by attempting to infuse each market with competition through chartering of new unit banks.

Carson and Cootner raise, as Horvitz did, the question of interstate branch banking. There is a potentially valid administrative obstacle to interstate branching so long as the system of dual federal-state licensing and supervisory jurisdictions persists, in that the examination and supervision of a state-chartered interstate branch banking organization would involve two or more state banking authorities as well as the FDIC and, presumably, the Federal Reserve examiners.

Carson and Cootner have a stimulating discussion of many of the existing problems of banking regulation under the multiple jurisdictions. Differentials in legal reserve requirements they find to be obnoxious and potentially threat-

ening to the Federal Reserve System: thus they recommend legislation to make Federal Reserve membership compulsory for all banks and reserve requirements subject to uniform regulation by the Board of Governors (p 142). They argue the case that differential access to branching is also discriminatory as between banks; but they do not quite come out flatly for conversion of the whole chartering and branch-licensing process to exclusive federal jurisdiction.

The workability of a scheme of multiple jurisdictions depends on coordination between regulatory authorities to minimize these competitive pressures between themselves. In the past few years of banking regulation, there has been obvious stress on many matters of both licensing and regulation between the Comptroller of the Currency and the other regulatory authorities, state and federal.

In their study Gies, Mayer, and Ettin tackle the problem of portfolio regulations and policies for the various types of financial intermediary. They then discuss a most interesting problem: the comparison of a putative schema of free asset selection by financial firms with the "tied" systems which are actually in being, given that households, in their provision of capital to these intermediaries, can respond to differences in the net yields they offer. The authors go on to say: "It may well be that financial intermediaries are at present too safe" (p. 228). And further, they say, "In particular, one may be dubious about our apparent tendency to make the degree of safety of various intermediaries homogeneous by such devices as government insurance of savings and loan shares, for example" (p. 229).

Their analysis of risk-taking by intermediaries and possible claims-shifting by households does not, however, take account of the public confidence problem. The FDIC and FSLIC were created in order to cut the social risks and social costs which would otherwise follow from such large-scale shifts of expectations. Policing of safety became a definitive responsibility of the public authorities, not of the general public. In the absence of these supportive institutions and of vigorous exercise of their supervisory duties by the public authorities, it would be necessary for many individual institutions to maintain far higher reserve and liquidity positions than the present systems require.

In general, the authors criticize restrictive statutes and regulations which prevent the allocation of savings and deposits from being responsive to best opportunity and which limit the flexibility of intermediaries in their choice of claims to hold. Many who have labored in the brier patch of detailed regulation would agree on the desirability of greater flexibility. There are, however, two stubborn questions—one of safety and the other of specialization—which are far from fully resolved. The public objective of maintaining safety does, as the authors say, compete on occasion with the objective of promoting economic growth. But this merely means that our systems of private financial intermediaries can only be counted on to play a limited role in offsetting the perturbations around the path of growth. The authors recommend, generally, that on the asset side, present absolute prohibitions and ceilings in asset choice be replaced by "trade-off" ratios. These would permit financial intermediaries to select riskier assets provided they were offset by increased hold-

ings of non-risky assets. It is not easy to design these kinds of trade-offs, but it is to be hoped that regulation will move in this direction.

The issue of specialization by financial intermediaries can be stated as follows: if one looks at the operations of any individual institution or any one type of institution, it appears potentially desirable on grounds of efficient resource allocation to broaden the opportunities to attract capital and the opportunities of asset choice. If this course were pursued for all these types of intermediary, however, the boundaries of specialization between these industries would be partly or wholly removed. We now have a series of sets of rules which enforce specialization; as these rules and the industries have evolved through time, and legislative bargaining has been a major element of the situation, the outcome is not necessarily sound. On the other hand, it is not obvious that the conversion of all financial intermediaries into general-purpose institutions makes sense either. What seems to be needed is the establishment of positive public policy criteria and tests for the design of the financial structure, so that each choice of overlap and of separate jurisdiction can be sensibly made.

Werboff and Rozen's study of market shares and competition inevitably deals with some of the same issues that arise in the first three studies. Intermediaries are seen as engaging in both intra-industry and interindustry competition—by seeking to locate new types of sources of funds and by creating asset-instruments and services attractive to particular sectors of the public. In this spirit, the authors review competitive pressures for acquisition of commercial bank deposits, security-insurance funds, thrift deposits, yield-risk funds, and yield-liquidity funds (pp. 270-96).

Competition for uses of funds, while less completely open to view than is the jousting for the public's attention on the inflow side, is seen as a mechanism of stress between specialist and diversified institutions. The former seek funds by price inducements and aggressive promotion when the use-markets they face are strong. The latter, with steady sources of funds tied to them by convenience of service, redirect their funds among uses in accordance with comparative yields, subject to restraints of regulation and their own operating conventions.

Turning to the regulatory process, Werboff and Rozen make the point that "The notion of a financial system comprised of a network of related institutions tied together in multi-market interdependence is not part of the theoretical underpinnings guiding our regulatory efforts" (p. 321).

Werboff and Rozen call attention, finally, to the unique factor in banking regulation: that the banking system is a specific object of monetary controls for economic stabilization, whereas its nonbanking financial competitors are not. At the same time, the banking system suffers inequities. Werboff and Rozen suggest study of such large new policy steps as variable asset reserve requirements for nonbanking intermediaries and a selective transaction tax on private noninstitutional credit extension, to bring the components of the system into better balance (pp. 324-25).

Klopstock, in his study of foreign and international banking in the United States, discusses the intricate flows of financial activity in the contemporary

international markets, in which New York is so prominent. U.S. banks experienced a five-fold increase in loans outstanding to foreigners between 1950 and 1961. During the latter 1950's gaps developed or became more apparent in the U.S. pattern of activity in international credit markets. First, U.S. banking, more than banking in Western Europe, has had a strongly domestic orientation, an orientation reinforced by the lushness of the U.S. market and by the detail and rigidity of banking regulation. Second, there was a gap in availability of intermediate-term credits and guaranties, partly filled as of the time of Klopstock's analysis by liberalized support through the Export-Import Bank. Credit-insurance coverage for short-term risks was also slower to develop in the United States than in Europe. There has of course been continued stress in the U.S. balance-of-payments position since the time (early 1962) of Klopstock's writing. The Regulation Q ceiling on time deposits, the effects of which Klopstock discusses (pp. 376-78), was raised late in 1964 in response to the sterling crisis after the British election.

While U.S. export expansion has continued since the time of Klopstock's writing and has undoubtedly been much facilitated by improvements in credit mechanisms, it would be interesting to see discussion of the character of recent developments: the impact of U.S. corporations' large-scale overseas direct investments and their multinational operations on international finance; the pressure on markets and institutions from balance-of-payments and international liquidity developments; and the nature and impact of President Johnson's "voluntary" credit and investment restraint on banking and other financial institutions.

Keith, in the final study of the series, analyzes the impact of federal taxation. There are two distinct areas of concern: different tax treatments of individual income which is received through holding of various investment instruments, and taxable status of financial intermediaries as corporate entities. (A third issue—underreporting of income—has since the time of Keith's writing been partly disposed of by expanded reporting of dividend and interest payments.) The Revenue Act of 1962 altered taxability of savings and loan associations very materially. Thus, Keith's discussion of this problem (pp. 401-17) is in part superceded by later events. There does remain the basic issue of *de facto* tax subsidy for the accumulation of general reserves which Keith raises as to banks (p. 399) and savings and loan associations (pp. 410-11). What we need to determine, in discussing this problem of taxes and reserves, is a criterion of public policy choice. The question is one of distributing the burdens of these restrictions and safety requirements as between the equity holders, the saving and borrowing public, and the government.

Redesign of the reserve structure of banking and of the savings and loan industry could perhaps be effected by expecting each individual institution to hold a thinner reserve margin and by transferring to the respective account-insuring institutions, FDIC and FSLIC, more responsibility for marshaling reserves to protect the public in the event of difficulty to the individual institution. But this would exacerbate another problem of the regulatory authorities, which is to determine when and in what ways to intervene. Given a state of affairs in which every institution in the financial structure possessed a very

thin reserve margin to cushion its risks, the regulatory authorities would have to have knife-edge decision rules for triggering intervention, because they would be the holders of the bulk of the whole system's reserves.

The taxation of life insurance companies, as Keith says, "has been one of our most baffling problems" (p. 423). Keith points out that policyholders' premiums contain three components: amounts paid for insurance protection; savings used to build up a reserve which, together with future premiums and interest earnings, will insure payment of all policy obligations as they fall due; and loading charges including those for overhead expenses (p. 424).

Since the precise time distribution of claims is not known, life insurance companies have both capital and surplus and accumulated reserves to meet variations in claims. Life insurance companies are fundamentally similar to banks and savings institutions in claiming the need for accumulation of these reserves. Their mode of accounting has led to an intricate method of determining how to tax them less than their apparent total net income might require, but the effect is to permit partly tax-free reserve accumulation as well as to provide tax benefits to the policyholders. Also, investment companies do not accumulate reserves against contingencies of loss, which simplifies treatment of them.

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Deutsche Geld- und Kreditpolitik, 1914-1963, 3rd edition. By RUDOLF STUCKEN. Tübingen: J. C. B. Mohr (Paul Siebeck), 1964. Pp. 341. DM 25.

The structure and policies of the German money and banking system have not changed basically over the last 50 years. This is the somewhat unexpected impression gained from reading this competent and comprehensive treatment of the subject. There were hectic intervals in the development, but the image at the end of the period closely resembles that of the beginning.

In pre-1914 days, Germany's monetary system was tied to gold, with all of the characteristics of the traditional standard present, including careful observation of the "rules of the game" that made it work. In World War I the monetary paradise was lost—for good. The years of war and ruinous postwar inflation brought German money into utter decay. Here the author touches on the rather confused theoretical background of that phase: two lines of thought were in dispute at the time. One was a less accepted "inflation theory" which cited money and credit expansion as the cause of soaring prices; the other was the officially supported "balance of payments theory" that saw in a negative balance of payments and deteriorating exchange rates the origin of the evil. A third concept, with its ominous policy implications then and later, Knapp's "state theory of money," is not mentioned. This oddly phrased but subtle analysis of money being, exclusively, "a creature of law," and therefore whatever the state desired to make of it,[1] laid much of the groundwork for monetary autocracy.

[1] Georg Friedrich Knapp, *Die Staatliche Theorie des Geldes,* first published in Germany 1905; translated in part into English (H. M. Lucas & J. Bonar), Macmillan & Co., London 1924.

By 1931—a few years after the 1924 currency stabilization with its return to some sort of a gold standard—Germany, in the wake of the world crisis, entered into a quarter century of external and, increasingly, also internal authoritative controls. This is taking 1958, when the German currency became fully convertible, as the turning point. Neither the Third Reich (1933) nor the outbreak of World War II (1939) induced any basic changes in the existing system.[2] Taking another brief step into the theoretical ideas behind policies, we now see none of the former confusion. All lessons had been learned. In terms of the quantity theory of money—and Irving Fisher's transaction formula is cited—price levels could best be held in check by putting the money side *and* the goods and service side of the equation under tight lids. If, for example, goods were not available, money became unusable, and there was no need to curb its supply. A decrease in velocity would provide the desired monetary brake. Compulsion supplanted free market forces and would give the government leeway for any economic policy it wanted to undertake.

The currency reform of 1948 that followed the monetary breakdown of the post-World War II years brought sound money once more to Germany, i.e., Western Germany.[3] A 10-to-1 exchange rate from the old Reichsmark into the new Deutsche Mark was stipulated, with numerous exceptions providing a more favorable ratio. By entering into the prevailing international monetary arrangements and institutions, Germany tied its foreign exchange rates and transactions again to a modified gold mechanism. Internally, however, at least in its statutes, the country kept free from gold attachments. There was to be no compulsory use of gold for redemption of currency or as reserve against liabilities of the central bank, nor any other "gold" obligation.

A double standard of this type—now used with various nuances all over—requires careful management by the central bank. But this should not be confused with authoritarian controls. There is not just a borderline between them; they are entirely different concepts. This does not come out clearly in the author's sudden sharp criticism of the Bundesbank and the commercial banks for failing to go through with an internally necessary contractive policy. The concentrated structure of the German banking system may lend itself to a stricter management "from above," but the central bank has, apparently, a philosophy that differs from the past. This spells closer cooperation in external relationships and a more liberal attitude with regard to domestic "free market forces." The commercial banks, in turn, may have interpreted this approach as giving free rein to their profitable self-interest. In the first years of the "economic miracle," 1948-52, and what the author calls "the golden years," 1952-58, harmony seems to have prevailed. In these periods the price level was kept stable while lagging capital formation, unemploy-

[2] The term "exchange control" did not really cover the full extent of this monetary management; much more than foreign exchange was involved and "control" went over into compulsion and force. The term "Zwangswirtschaft" employed in the German description is more accurate.

[3] Eastern Germany embarked on its currency reform at the same time and largely in the same way as Western Germany. After that, however, their economic and monetary developments moved further and further apart. But in the city of Berlin "D-Mark West" and "D-Mark East" had their special exchange problem as long as people moved relatively freely between the sectors.

ment, and other drawbacks were slowly eliminated; restrictive policies of the fiscal authorities and, we may add, of business management (lids on wages and salaries) aided the process. From 1958 on, these pressures eased and the monetary authorities did not step energetically into the breach. When the Bundesbank used its powers of control, 1959-60, the controls misfired, and the highly liquid commercial banks continued their credit expansion. At that point the Bundesbank gave up, using the excuse of external considerations, and the commercial banks were left, in the words of the author, to their "selfish" devices, but what may just as well be called their inherently businesslike, procyclical propensities. The author strongly condemns the "great domestic failure of the Bundesbank," and on this note the book ends.

What happened after that? Through 1963 and into 1964 the Bundesbank did continue to refrain from corrective measures.[4] But then a temporary slowdown of business seemed to take care of the situation, and fiscal transactions by various governmental bodies also helped toward a desirable contraction. In the second half of 1964 and into 1965, however, restrictive monetary procedures were used. Whether the ensuing greater stability is attributable to them or to the "free play of market forces" that dried up some of the banks' liquidity and consequently their lending expansion, does this state of affairs really justify the slightly muffled call for more comprehensive controls?

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[4] Report of the Deutsche Bundesbank for the year 1963 (pp. 11-22) and monthly reports of the Deutsche Bundesbank, particularly the October 1964 issue with its survey, "The Economic Situation in the Federal Republic of Germany in the Autumn of 1964."

Public Finance; Fiscal Policy

The Individual Income Tax. By RICHARD GOODE. Washington: The Brookings Institution, 1964. Pp. xvi, 367. $6.75.

With the publication of this book, another in their series of Studies of Government Finance, the Brookings Institution has performed a rare feat of simultaneously expanding and condensing the field of study covered. *The Individual Income Tax* rounds out beautifully a small team of indispensable classics that deal with the concept of income and with the complex problems of making it into a workable base for taxation. While Goode's book is less positive than its earlier predecessors by Simons and Vickrey, it achieves a skillful blending of principle and practice that may have more influence in the world of affairs. In addition, by making extensive use of empirical studies, both by others and by Goode himself, it shows how much the pragmatic science of public finance has progressed in recent years. There is, of course, still much to be done, and graduate students will find here a wealth of suggestions for future dissertation topics.

The book begins with an evaluation of the major alternative bases for taxation. In the Goode Society there would be a progressive personal income tax, gift and succession duties, an annual tax on net worth, and a few excises. Par-

ticular attention is paid to the relative merits of equal-yield expenditure, income and wealth taxes, but exclusion from the latter of human capital robs the comparisons of some of their significance. Since property income is typically less than 20 per cent of all personal income, it is surely unrealistic to propose a net worth tax that is confined to property as a realistic alternative to general income or expenditure taxes. One might compare such a partial wealth tax with an equally productive tax on property income only, or one might generalize the wealth tax to include the capitalized value of human earnings. The latter levy might, as Goode argues, be too costly to administer equitably—though I am not convinced that it would be any worse in this respect than either existing income or property taxes—but it would not need to endanger personal liberties in the way Goode implies (pp. 21-22). Would a person with a great earning capacity who refused to work, he asks, nevertheless be assessed with a high wealth tax? This is simply to ask whether leisure, which at a later point he classifies as a form of income (p. 150), should be taxed or not. It can be included under either an income or a wealth tax, with the intense incentive effects that he objects to, or it can be excluded from both. A tax on human wealth, in other words, can readily be made a function of actual current earnings rather than of earning power.

Be that as it may, his comparative analysis of expenditure and income taxes is comprehensive, and though lack of empirical evidence precludes a definitive choice between the two for a developed economy, he amply justifies his conclusion that the case in favor of expenditure taxation remains unproven. The most interesting data presented at this point are Goode's own estimates, derived in detail in Appendix C, that in 1950 a proportional income tax would have been no more unfavorable to private saving than an equal-yield proportional consumption tax. Each would, on the other hand, have reduced saving by about 17 per cent less than the federal income tax apparently did in fact. This illustrates one of the familiar threats of tax rate progression to a high rate of economic growth. Similar dangers to work and investment incentives are discussed, but here again the evidence needed to determine whether these effects are important or trivial is lacking.

The heart of the book (Chapters 5 through 9) is concerned with the structural aspects of individual income taxation—with exclusions from adjusted gross income, business and personal deductions, capital gains and losses, personal exemptions, and income-splitting within the family. On these important questions Goode takes a moderate position, somewhere between those who would not object to further erosion of the tax base and those who want to convert the tax into a broad-based low-rate levy. Four examples will indicate the flavor of the discussion. The first concerns student expenditures for vocational and higher education which, it is argued, are at least as much costs of earning income as they are personal consumption. Giving precedence to the former aspect, Goode recommends that students be allowed to amortize their expenses against current and future taxable earned income and bolsters his case with an extended analysis of administrative difficulties, probable revenue losses, and likely incentive effects. The admitted weakness of the latter may dampen popular enthusiasm for the proposal but, as Goode notes, its enactment should help bring about a more realistic understanding of the invest-

ment quality of educational expenditures. Even more important is the likelihood that adoption of Goode's plan would forestall the appearance of tax changes, such as personal deductions for the parents of college students or a special earned-income allowance, that have much less to recommend them.

It is interesting to compare Goode's concern for principle in the case of educational expenditures with his definite leanings towards pragmatism when it comes to retirement income. He rejects, for example, the proposal to tax any excess of social security benefits over the recipient's own prior contributions because this might conflict with the goals Congress has had in mind as it developed the social security system (p. 108). No doubt one of these goals is to give relatively greater aid to low-income families with many dependents, but tax exemption for social security benefits is surely a less efficient way of doing this than a systematic reform of personal exemptions such as Goode himself proposes at a later point. As for private pension plans, current taxation of employer contributions to these is rejected because the widespread practice of nonvesting makes it difficult to estimate the value of these contributions to the employee. This difficulty, however, exists only in an *ex ante* sense. If all employer contributions were fully taxed, the employee who disqualifies himself by shifting employment should be viewed as owning a capital asset whose value has suddenly fallen from the cumulated sum of all previously taxed contributions to zero (or to whatever capital sum he receives from the pension fund). Deductibility for capital losses of this sort would not only avoid interpersonal inequities but also offset some of the undesirable impact of nonvested pension plans on labor mobility. If rationalized taxation of employee retirement income is feasible, as it seems to me to be, the complex and makeshift Self-Employed Individuals Tax Retirement Act of 1962, whose scope may well be expanded in the future, could be given an honorable retirement of its own.

The imputed rent of owner-occupied dwellings is an esoteric type of income that probably stands little chance of being made taxable in the future. It is, however, an important element in household budgets—together with deductible mortgage interest and residential property taxes it came to nearly $20 billion in 1960—and the effects of its exclusion should be known and taken into account by government policy-makers. Goode's analysis, which is amply supported by empirical data, makes a significant contribution to this goal.

The final example concerns the much-discussed problem of capital gains and losses. After a cogent critical analysis of existing law, which acquires much of its excessive complexity from its special treatment of capital gains and losses, Goode proposes full taxation at ordinary income tax rates with net gains or losses prorated over a 3- to 5-year period so as to mitigate the impact of progressive tax rates on bunched income. The plan seems capable of achieving a reasonable degree of equity without excessive administrative complexity. It does not, of course, eliminate the disincentive to portfolio-switching that is inherent in any capital gains tax levied on a realization, rather than an accrual, basis, and Goode pays particular attention to the "locking-in" problem. All facets of it receive their due, but the issue remains unresolved for lack of the relevant empirical data. A further difficulty involves the extent to which full taxation of capital gains at the individual level would re-

quire reduction in the tax on retained earnings at the corporate level in order to avoid serious overtaxation of this highly interrelated income flow. Goode seems unduly ambivalent on this point. Whereas on page 343 he notes that "If . . . capital gains are taken into account, the addition of retained earnings as well would involve double counting . . . ," on page 192 he rejects preferential treatment of capital gains "as a means of abating the so-called double taxation of corporate profits." Neither of the reasons given for this conclusion is relevant to the point at issue. In this important respect, therefore, his treatment of capital gains and losses remains incomplete. Unlike many academic reformers, however, Goode does provide a second-best solution appropriately entitled "Improvements Without Fundamental Revision," which is well worth the attention of any policy-maker who is unwilling to risk fundamental reform.

When some of the major structural qualities of the individual income tax are put together and effective tax rates at different levels of income are shown, as Goode does in a chart on page 236, the results are striking. Whereas nominal rates in 1960 rose steadily from 20 per cent to 87 per cent at the top, effective rates, derived after allowing for the effects of personal exemptions and deductions and for long-term capital gains, rose to a peak of 40 per cent in the $100-$150 thousand income group and then declined to only 30 per cent at the top of the income distribution. If income-splitting, which not all would classify with Goode as undesirable, is also allowed for, the peak 1960 tax rate was only 33⅓ per cent. The chart that shows these relations should become one of the hallmarks of modern U.S. public finance, ranking with Daniel M. Holland's similar graph (p. 178 of his *Dividends Under the Income Tax*) which showed differential income tax rates (corporate and personal) on stockholders' income in 1950 rising to a peak of +10 per cent at a $12,000 income level, falling to zero at $100,000, and becoming −14 per cent on an average income of $500,000. What is widely regarded as a highly progressive system of income taxation, then, has some very definite regressive features.

Once these relations have been established, it is not surprising to find, as Goode demonstrates in Chapter 10, that the individual income tax has been "only a minor factor in the reduction in inequality of measured disposable income" (p. 279). Drawing on the results of a recent empirical studies in this area, he deals both with the so-called "before-tax" distribution of income, which the individual income tax can affect by retarding the accumulation of wealth, and with the directly created divergence between the "before-tax" and "after-tax" distributions. Valuable as these computations are, they provide only part of the final answer. If, as seems likely, the major fiscal forces against income inequality flow from the expenditure side of the budget, the individual income tax, because of its wide acceptability and high productivity, may have made an important indirect contribution by supporting human development programs at levels that would not have been possible under different forms of taxation. In addition, there is the distinct possibility that an equal-yield, proportional income or consumption tax would have precluded some of the self-development which the lower income groups have in fact been able to provide for themselves and their children.

The final topic discussed is the contribution of the individual income tax to

economic stability (Chapter 11). New structural estimates of built-in flexibility are given, and the results confirm Joseph A. Pechman's earlier inferences that rate graduation, contrary to what one would expect, has not been an important factor. Over the period studied (1949-60) a flat-rate tax of 23 per cent would have had virtually the same degree of built-in flexibility (8.7 per cent of GNP). Even so, the individual income tax has enough automatic short-run sensitivity and is sufficiently adaptable to countercyclical formula and discretionary changes in tax rates for Goode to rank it first among all major taxes for stabilization purposes.

The Individual Income Tax is a fitting tribute to the first fifty years of the tax it discusses. Written with a full recognition of existing weaknesses and inconsistencies, but at the same time with affection and understanding, the book makes a persuasive case for maintaining, or even increasing, the relative importance of the individual income tax in the U.S. fiscal system. Further reforms are of course needed, and the difficulties involved in enacting them are great. Because of this book, however, they should be a good deal easier to achieve.

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Fiscal Survey of Colombia—A Report Prepared under the Direction of the Joint Tax Program of the Organization of American States and the Inter-American Development Bank. By Milton C. Taylor and Raymond L. Richman. Baltimore: The Johns Hopkins Press, 1965. Pp. xviii, 277. $8.95.

This is the second volume of the Joint Tax Program's series of fiscal studies of Latin-American countries. Its organization is similar to that of the first volume treating the fiscal situation in Panama, also prepared by Professor Taylor with the collaboration of Professor Richman and others. The monograph begins with a summary of Colombia's revenues, expenditures, debt, fiscal policies, and the authors' proposals for tax reform. There follow detailed analyses of the operation of the income tax and the system of seven direct taxes linked with it, the death and gift taxes, taxes on agriculture, taxes on urban property, taxes on foreign trade, and internal indirect taxes, as well as chapters on tax incidence and the role of autonomous agencies. Specific recommendations for their improvement follow the discussion of each major group of taxes. The text is amplified by 134 helpful tables—both descriptive and analytical. It is especially useful to have this volume so soon after *Taxation in Colombia* in the World Tax Series of the Harvard Law School's International Program in Taxation. The two works complement each other admirably with the Harvard volume giving a full bibliography and emphasizing the background and the content of the present tax laws, and this study by the Joint Tax Program supplying a more detailed analysis of the economic effects of each tax and practical recommendations for reform.

This relatively short book is highly efficient, not only in giving a clear picture of Colombia's very complex tax system, but at the same time an understanding of its economic and social structure. Colombia's economic problems include the need for more revenue, control of inflation, improved distribution

of income and wealth, greater and more diversified productive investments, more effective use of the land, and an increased variety of exports.

While Colombia's economic problems may resemble those of most underdeveloped countries, its tax system is not typical. Colombia levied the first income tax in the Western Hemisphere and, today, gets more of its national revenue from income taxes than any other Latin-American country. Colombia collects both an excess-profits tax and a net worth tax, each working to penalize the taxpayer trying to avoid the other levy—a device recalling in some respects the unpopular "declared-value" excess-profits tax in the United States between 1933 and 1945. Its income tax allows the splitting between husband and wife of earned income up to 60,000 pesos (in 1963 when the study was being made, the official rate of exchange was 9 pesos per U.S. dollar). The following provisions are cited to illustrate the variety of Colombian tax measures designed to reduce specific economic problems. An income tax exemption on the export of nontraditional products, determining net income from exports as 40 per cent of their gross sales value, is granted to encourage a greater variety of exports. Other tax incentives include exemption from some direct taxes or from import duties as an encouragement to certain specified industries, to new investment in basic industries, and to those using domestic materials. As a virtually compulsory form of saving and investment three earmarked taxes on income offer the purchase of the salable securities of certain quasi-public organizations as an alternative to the payment of a portion of the tax. Agricultural land not used according to certain minimum standards of efficiency is liable to a special tax as a penalty, although the lack of land classification has prevented its assessment. As an encouragement to education, the taxpayer is allowed to deduct from taxable income as much as 500 pesos paid for the education in Colombia of each dependent. The income tax puts a slight penalty on expenditures for luxury housing by taxing the owner-occupier on the imputed rent on the valuation of his house in excess of 100,000 pesos. Capital gains on real estate are taxed—although not very effectively. Withholding at the source of 12 per cent of the dividends paid on bearer shares reduces evasion of the income tax through their use. But these and other well-intended provisions have added to the complexity of the tax system without removing its deficiencies.

No attempt is made in this short review to present a balanced description of the many specific recommendations of this study. The selected proposals mentioned below must not be taken as representative of the authors' over-all plan. In general, their suggestions include measures to broaden the base of the income tax, simplify the tax system and make it more progressive, remove earmarked taxes except in the case of the gasoline tax, improve the national-departmental-municipal distribution of revenue, and concentrate fiscal incentives for investment on selective short-run measures and allowances for faster depreciation. They base their recommendations on Colombia's need for more revenue, equity, and neutrality among kinds of businesses and sectors of the economy.

Since all their goals cannot be satisfied at once, the authors sometimes have had to focus their recommendations on the most urgent needs at the expense of the others. A few examples follow. In spite of a lack of neutrality among

types of business organization which penalizes the corporation, they conclude that the need for revenue and better income distribution is even more critical than for neutrality and, as a result, that "a policy of integrating corporate and personal income taxes does not appear to be warranted at this time" (p. 58). For the taxation of agricultural income, the authors offer an expedient instead of a more refined measure of taxable income. Their proposed "presumptive net income tax" would include 10 per cent of the assessed value of agricultural land in taxable income. They rest their proposal on the importance of agriculture to the Colombian economy, the small share of income tax revenue now received from the agricultural sector, convenience of administration, and the need to use taxation to further the goals of agrarian reform. In order to penalize expenditure on luxury housing, the authors recommend a progressive urban property tax, while calling attention to the fact that it is related to the value of property and not a personal tax according to the ability to pay of the individual taxpayer. They discourage the use of many tax incentives as costing more in revenue than the resulting investment is worth to the economy, and they criticize tariff exemptions for many durable-goods industries as an "attempt to develop from the top down" (p. 184). Instead they recommend encouragement to the production of raw and intermediate products as a means of increasing exports and of reaching a slower but sounder import substitution needing fewer imported materials.

Taylor and Richman did not include possible fiscal problems arising from Latin-American economic integration and from Colombia's membership in LAFTA in their appraisal of the Colombian system of internal taxes and recommendations for its reform. Nevertheless, this volume provides important information to economists considering fiscal harmonization as a part of Latin-American economic integration. While some economists may differ with the authors' ranking of fiscal goals and with their solutions to Colombia's tax problems, every economist interested in economic development will meet similar problems and will find pertinent advice in this book.

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International Economics

Balance of Payments Equilibrium: A Theoretical and Empirical Study. By Hourmouzis G. Georgiadis. Pittsburgh: University of Pittsburgh Press, 1964. Pp. ix, 220. $7.00.

The formulation of the conditions for balance-of-payments equilibrium given by Nurske and Meade fifteen or twenty years ago may well have to be reconsidered in the light of research on the balance of payments and on the theory of economic policy contributed during the intervening period. Professor Georgiadis sets himself the tasks to (a) review the Nurske-Meade concept in its own frame of reference and (b) derive a new set of conditions for external equilibrium from a large econometric model of the United States.

For his critical task, Georgiadis constructs and later modifies, a detailed "Keynes-like" model of an open economy (country A) with the rest of the world as a second country. (The microeconomic relationships underlying this model are developed in Appendix I.) The author concludes, correctly, that, if the cycles of economic activity are not of equal duration in the two countries, external equilibrium—defined as equality of external receipts and payments over the cycle—cannot simultaneously exist in both countries and thus in the world economy as a whole. Moreover, the exchange rate which would produce external equilibrium for country A would be different for each cycle in accordance with the different cyclical states of the rest of the world. Georgiadis becomes a victim of the limitations of his model when he concludes that usually simultaneous achievement of full employment and balance-of-payments equilibrium is impossible, since maintenance of full employment of a growing labor force requires growing demand, which (in the model) implies rising prices and thus a loss of international competitiveness. This conclusion is incorrect even within the framework of the model, if the labor force also rises, and full employment is maintained, in the rest of the world.

Georgiadis feels that there is not a sufficient basis, either on welfare grounds or otherwise, for specifying as equilibrium conditions—as Nurske does—that tariffs, trade restrictions, and exchange rates must not change, whereas monetary and fiscal policies can change freely. He rejects the possible defense that Nurske may have taken certain normative ideas, not from the Keynesian model which he analyzed, but from a larger (implicit) model that included the relevant welfare concepts. He even argues, under a simplifying assumption, that the selection of policy instruments has no effect on the operation of the economy. Here the author goes completely astray by "proving" that changes in alternative policy variables that produce a given effect on a target variable, such as employment, will have identical effects on all other variables in the system. A given increase in employment can be accomplished by reducing taxes, increasing public expenditures, increasing the money supply, increasing tariffs, depreciating the exchange rate, etc.; it is incorrect to suggest that each of these alternative measures would have the same effect on all other variables and that economic welfare does not depend on the choice of policy instruments.

Georgiadis' point is, however, well taken and one wishes that he had brought it out more clearly by discussing it in general terms rather than in the framework of his particular model. Nurkse defines balance-of-payments equilibrium under conditions which fix the levels of two sets of variables; (1) all those policy instruments which could be used to adjust external payments directly to external receipts, namely the exchange rate, tariffs, and trade or payments restrictions; and (2) any target variables other than balance-of-payments equilibrium, in particular the level of employment. This definition would indicate whether external payments are in equilibrium, provided that internal equilibrium is, as far as possible, maintained through monetary and fiscal policies. But according to recent analyses by Mundell and others, monetary and fiscal policies can be so designed as to achieve both external and internal equilibrium. Within certain limits, a persistent balance-of-payments surplus or deficit may thus not indicate a fundamental disequllibrium and a

need for adjustment of the exchange rate, but merely an incorrect mixture of monetary and fiscal policies. More generally, if there are n targets of economic policy, including balance-of-payments equilibrium, and n or more policy instruments, the modern equivalent of Nurkse's definition of external equilibrium would have to be formulated as follows: Select $n - 1$ policy instruments and assume that they are operated in such a way as to achieve the $n - 1$ targets other than balance-of-payments equilibrium. If then external receipts and payments are equal, or are expected to be equal, during the selected time period, the balance of payments is in equilibrium and the other policy instruments need not be used; otherwise, the n-th instrument, or one of the remaining instruments, would have to be used in conjunction with the other $n - 1$ policy variables to ensure external equilibrium. The consequences of such a definition are rather far-reaching. It implies, for instance, that there is no unique equilibrium exchange rate, provided that the number of other policy instruments exceeds the number of targets; subject to constraints on variables other than targets and instruments, balance-of-payments equilibrium could be achieved at any exchange rate, including the existing one. The alternative approach to the design of economic policy consists of the maximization of a welfare function defined on all target and instrument variables subject to certain constraints (e.g., that of the existing gold and foreign exchange reserves). As pointed out by Georgiadis, with this approach the optimum external surplus—in the long run presumably slightly positive to allow for adequate growth of reserves—would be achieved through the use of all instruments, including changes in tariffs and the exchange rate. Regardless of which of the two approaches to economic policy is chosen, what is to be defined is not external equilibrium, but the optimum economic policy given all the targets, constraints, and potential policy instruments.

The second part of the book consists of a 300-equation model of the United States for the 8-year period 1949-56. It is difficult to follow the development of this model, since a 9-page list of non-mnemonic variables is followed by 80 pages of equations and description almost entirely in terms of symbols and equation numbers. Individual items in the flow-of-funds accounts are related to other flow-of-funds items, and in a few instances to interest rates, by simple least-squares correlation in logarithms, resulting in a set of elasticity estimates (annoyingly given to 7 significant digits). Similarly, in the foreign sector of the model some 60 import commodities or commodity groups are related to components of consumption expenditure or of national income by industrial origin; all the former are in turn related to total consumption expenditure and most of the latter to income originating in the manufacturing sector. (The foreign sector of the model is re-estimated for the period 1949-60 in Appendix II.) No allowance is made in any of these equations for common trends in the pairs of variables correlated, nor for the jointly dependent character of most of the paired variables. Georgiadis discovers some peculiar relations, such as that imports of services depend on income originating in the U.S. service sector (with a correlation coefficient of .9682 and an elasticity of 1.782420), or that investment income paid by the rest of the world to the United States depends on investment income received from the United States.

Federal government expenditures on goods and services (other than payrolls) depend on income originating in manufacturing with an elasticity of about −3.6, a relation which reveals a powerful stabilizer or, being too much of a good thing, perhaps a source of explosive fluctuations. Exports and other U.S. receipts from abroad are related to dollar receipts by the rest of the world. Prices and employment (which do not appear elsewhere in the model) are made to depend, with constant elasticities, on a total-income variable.

By and large, all these sources and uses of funds, as well as imports, exports, prices, and employment go up and down together (being related to one another, in pairs, by constant elasticities). Since there are very few exogenous variables, Georgiadis in the end ignores the equations explaining various government expenditure categories and considers them as exogenous. Thus the economy moves essentially in response to changes in government expenditures. The author concludes that full employment with price stability is impossible: since the labor force grows, government expenditure must grow to maintain full employment, and prices rise in proportion to expenditure. A full quantitative solution of this peculiar model would probably reveal its weakness very strikingly. But no such solution is given and no conclusions are drawn about the net effect of various fiscal policy measures on the balance of payments. It is not clear how this part of the study is related to the over-all topic of the book, except for the trivial conclusion reached at the end that the balance of payments can be said to be in equilibrium only if all other sectors of the model are in equilibrium. It is a pity that such a monumental effort could not have been used more productively.

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* The views expressed are personal and not necessarily those of the International Monetary Fund.

General Equilibrium of International Discrimination—The Case of Customs Unions. By JAROSLAV VANEK. Cambridge: Harvard University Press, 1965. Pp. ix, 234. $5.50.

This is a rigorous analysis of the theory of partial and complete customs unions. Its approach can be compared best with that of Meade's *Theory of Customs Unions*. Meade's evaluation of trade is Pigovian: Trade increases or decreases in each case are multiplied by price differences between an exporting and an importing country. A given change in trade restriction or subsidy is then evaluated by the positive or negative sum of trade quantity changes, weighted by price differences.

By comparison, we have in Jaroslav Vanek's book an evaluation of particular cases according to whether they lead a collection of countries to a higher or a lower joint utility-possibility curve. The collection may include all the countries of the world (illustrated with three) or only the members of the union (represented by two). Individual countries are taken to possess fixed community indifference maps. Not surprisingly, some situations may be judged differently according as Vanek's or Meade's device is used for the evaluation. So far as I can see, this is because consumption and production effects

may pull in opposite directions for Meade, while they generally work in the same direction for Vanek. Roughly speaking, Vanek comes out with a favorable verdict whenever price ratios are moved closer together. In his two-good, three-country examples, however, a movement of one country's price ratio out of line with world prices may be enough to put the world on a lower utility-possibility curve.

The author believes that he has found a justification for community curves which enables him to draw conclusions about the individuals of a community from whatever happens on the community indifference map. Without comparing individual preferences or specifying anything about the distribution of income, he asserts that movement to a higher community curve makes it possible to improve at least one person's utility without lowering anyone else's. This point is developed in detail in an appendix to Chapter 4, which has appeared also in the 1964 *Review of Economic Studies*. If his findings are correct, then an economic change which puts a country on a higher indifference curve also places it on a higher consumption-possibility curve.

Space does not permit a detailed examination of this claim. But it is possible, by drawing sets of community-indifference and consumption-possibilities curves in the same diagram, to show cases where this is not true. Movement to a higher consumption-possibilities curve may not put the country on a higher indifference curve, and movement to a higher indifference curve may not put the country on a higher possibilities curve. The reader may also refer to Samuelson's article in the *Economic Journal*, December, 1962.

He makes a useful distinction between similar and dissimilar countries—always a difficult one to make precise. Two countries, *a* and *b*, are similar if in a world of two goods and equal and nondiscriminatory tariffs, they trade with the outside world (*c*) and not with each other. They are dissimilar if they trade with each other, and one of them (but not both) imports from the outside world at the same time. This definition is convenient for diagramming and gives precise answers. With more than two goods we have the same old troubles, but at least this is progress. The interesting case is of course union between dissimilar countries because in the other case nothing much happens.

When *a*-*b* union, partial or complete, takes place, the partner competing with the outside world for the other partner's trade is bound to gain. It trades more at given terms of trade, and the terms of trade tend to move in its favor. The partner who previously imported from the outside world may gain or lose because these two tendencies work in opposite directions. While this finding is not new, the argument leading to it is nicely made.

Vanek has reached some interesting conclusions on the world effect of customs unions which I shall try to state briefly and plausibly, but not rigorously. The test is whether it is possible to keep *c* and one of the two union members, say *a*, on the same indifference level and make the other member, *b*, better off. It is interesting that the consumption effect and the production effect generally work in the same direction and lead to the same conclusion. Thus one can examine one of the two effects alone and reach the right conclusion about the beneficial or detrimental effect of union from the world point of view.

Taking only the consumption effect, and looking first at a small union (one too small to affect rest-of-world prices) one can say the following:

1. When the rest of the world has free trade, the union can just as well be beneficial, a priori, as detrimental. This depends specifically on the shapes of the indifference curves of the two union members. Roughly speaking, if the import-displacing union member is rather small (and so has a sharply curved indifference map), it does not do much displacing of its consumption compared to anyone else (and similarly for production). In that case there is improvement through moving intra-union rates of substitution into line with each other, and not much net movement away from world prices.

2. When the rest of the world has a duty, more of the adjustment in substitution rates is away from the rest-of-world rate. In particular the import-competitor may on balance move its rate of substitution away from the rest of the world, and this fact is more important than its moving closer to that of the other union member. The other union member's rate of substitution does not change at all in the small union case, given a constant tariff.

Now when the union is large enough to affect the rest-of-world price ratio, any unfavorable effects can be mitigated and perhaps reversed. The limiting case, necessarily favorable, is where the union includes all countries. The smaller the rest of world, the more curved is its production-possibility surface and its indifference map. Therefore the absolute amount of displacement of production and consumption in the rest of the world is made smaller. When there is absolutely no shifting, one has the simple two-country model of the world, where reducing barriers always brings gain.

As a guess, Vanek estimates that a union embracing about two-thirds of the world's resources would just break even, from the viewpoint of world welfare. This is based on equal initial duties everywhere and equal curvature of indifference- and production-curves for countries of equal size. Modification for costs of transportation reduces the critical size; but nevertheless even the EEC would not get a favorable verdict, at best a neutral one (p. 153).

This unfavorable finding on unions is softened by a novel modification of the compensation principle. Instead of a purchasing-power transfer to the losing countries, let us take a compensating tariff reduction. This is a tariff large enough to leave *c*, the outside country, on the same level of indifference as before. Then a union, irrespective of size, can always bring an improvement from the world point of view, provided that the union makes a sufficient reduction of its common external duty. The needed reduction, however, may actually call for changing the common tariff to a subsidy for the outside world. This paradox results from the existence of tariffs in the outside countries.

A curious point has to do with unilateral tariff reductions. Suppose that *c*, the outside country, has the highest tariff and therefore an "extreme" price ratio (one not between those of *a* and *b*), while *a* and *b* have equal nondiscriminatory tariffs. If it competes with *a* for *b*'s trade, then for *c* to lower its duties only improves things for sure so long as *c*'s duty rate is higher than those of *a* and *b*. Complete removal of *c*'s duty may actually reduce the world's utility possibilities. In order to be sure of benefit from the removal of trade

barriers, the removal must be done simultaneously by *all* countries.

An appendix deals with the more difficult case of three goods, but for understandable reasons restricts itself to the effects of equal and unequal tariff changes on a single country. The conclusions are like those of Lipsey and Lancaster, that reducing a tariff on only one or a few goods doesn't always improve a country's welfare.

In general, what Vanek has done here is a welcome step forward in the theory of trade discrimination. The approach, despite the author's claims, does not in the reviewer's opinion deserve the utility-possibilities label. Rather, it is a halfway house in the same sense as Meade's earlier *Geometry*.

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International Trade Theory in a Developing World—Proceedings of a Conference Held by the International Economic Association. Edited by ROY HARROD WITH DOUGLAS HAGUE. New York: St. Martin's Press, 1964. Pp. xv, 571. $14.00.

This volume represents the proceedings of another conference of the International Economic Association which met at Brissago, Switzerland on September 1-9, 1961. The conference was attended by 47 economists drawn from 22 countries, and the book consists of 17 papers, complete with an introduction by Sir Roy Harrod and a lengthy record of the debate summarized by Professor Hague.

The subject of the conference was international trade and investment, but the title of the volume more accurately describes the contents, since some of the essays have nothing to do with investment. The topics in fact range generally over most subjects in international trade theory and are analyzed or surveyed at different levels of abstraction: technical essays like J. Bhagwati's excellent survey of the pure theory of international trade, H. Johnson's contribution to the theory of technical change, and K. Savosnick's essay on growth and balance of payments; empirical essays like A. Maizel's discussion of trends in world trade, T. Morgan's analysis of the terms of trade of primary producers, and A. Lamfalussy's essay on international aspects of the trade cycle; institutional essays including H. Leisner's on regional trade arrangements and R. Mikesell's analysis of common markets in developing countries; polemical essays by A. Bechin and V. Sergeyev of the Soviet Union on the evils of foreign investment and the principles of socialist trade; survey articles on economic structure and infant industries by M. Bye and H. Myint: policy essays dealing with monetary and exchange problems by I. Patel, J. Weiller, D. Delivanis and C. Kindleberger; and an essay by Harrod on capital movements and the problem of debt servicing.

Without attempting to discuss each contribution separately, it may be useful to point out some highlights. Bhagwati's survey article, which has been published in revised form in the *Economic Journal* (March 1964), will prove of great value to students; Johnson demonstrates, by an argument (from the Heckscher-Ohlin model) best characterized as verbal geometry, that an inno-

vation economizing on the use of a factor used intensively in the innovating industry can raise the price of the economized factor in the whole economy; Savosnick explores the position of countries growing at a slower pace than competitors selling in third markets; Morgan offers a useful survey of the terms of trade literature; Harrod points up the problem of debt-servicing and the limitations imposed by the growth of other factors on the feasible rate of capital imports; Bye makes some interesting points against the Heckscher-Ohlin model; Patel emphasizes the pros and cons of planning and the balance of payments limitation on growth; and Bechin expands on the theme of exploitation of the capital-importing countries by their richer neighbors.

The average quality of the essays is somewhat disappointing—some are repetitive, unoriginal, and verbose. This is not necessarily—or entirely—the fault of the authors, for the papers are command performances on assigned topics on which the authors may have little new to say: the Muse is not always willing! The method is not ideally designed to produce original results, and it would have been better, in my opinion, either to release the authors from the specifically assigned tasks to prepare original papers or else to narrow the scope of the subject to a specific problem, and thus obviate the need for repetitive surveys.

This is not to imply that the conference was without value or the book not worth publishing. The usefulness of a scientific conference comes from the stimulation to the participants and the published results offered to the outside world. From the record of the debate, which is generally more interesting than the papers themselves, it is clear that the conference was stimulating to the participants. In this connection it would have been more convenient to put the summary of the debate after each paper or else prepare an index for the summary. It is also true that the proceedings constitute a useful addition to the literature on international trade theory, and as a compendium of surveys and individual views on a variety of related subjects, as well as a few new insights, the volume will prove useful to both students and policy-makers.

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New Directions for World Trade. Proceedings of a Chatham House Conference, Bellagio, 16-24 September, 1963. New York: Oxford University Press, 1964. Pp. vi, 241. $4.80.

This is a useful, lucid, informative book. The "new directions" for world trade which it discusses are measures intended to improve the lot of the less developed countries. Like Raúl Prebisch's *Towards a New Trade Policy for Development*, which it antedates by some months, it was written with a view to influencing the mammoth United Nations Conference on Trade and Development that met in Geneva for 12 weeks in the spring of 1964. The Chatham House Report, however, is less dogmatic, didactic, and political than the report by the distinguished Secretary-General of UNCTAD. The very reasonableness of its approach—which should earn it a wide audience in the developed countries—may reduce its appeal in other areas.

The book consists of two parts, in one-horse-one-rabbit proportions. The overwhelming bulk of the book consists of the seven "preparatory papers" compiled for a small conference of economists from Western, Soviet, and underdeveloped countries which met at the Rockefeller Foundation Villa at Bellagio, Lake Como, in September 1963. (The conference was organized by the Royal Institute of International Affairs and sponsored by the Carnegie Endowment for International Peace.) The rest of the book consists of a brief but good introduction by Andrew Shonfield, Director of Studies at Chatham House, a summary of the seven days of discussion at Bellagio, and a "Statement of Policy" drawn up by the conference participants. The "Statement of Policy" became part of the documentation for the UNCTAD Conference, having been sent to governments by the Secretariat.

The seven essayists are Alfred Maizels, J. H. Richter, A. G. Hart, I. G. Patel, Caroline Miles, J. Royer, and Gerda Blau. They paint a somber, well-documented picture of the prospects of the low-income countries for earning through trade, under present ground rules and policies, the foreign exchange they will need in order to meet the target envisaged for the United Nations Decade of Development—a growth rate of 5 per cent per year in real income. They suggest a *smörgasbord* of policy changes intended to reduce fluctuations in export earnings from primary products to increase income from such exports, to improve the prospects for earnings from sales of semimanufactured and manufactured goods, and generally to facilitate longer-range planning with respect to trade and aid. None of their proposals will appear startlingly new to those aware of the dissatisfaction in the less developed world with the ground rules established during the 1940's for the conduct of international economic relations. The authors do, however, assign a large role to the technique of international confrontation, i.e., to procedures that will provide each country with better advance understanding and knowledge of the plans of other countries and with opportunities to propose modifications that will reconcile the interests of the world at large with those of the nation in question.

Patel calls it "a curious commentary on our international institutional framework that while the IMF 'consults' its poorer members every year with all the thoroughness of a judicial inquiry and with specific recommendations in regard to removal of exchange restrictions, no such machinery with the same authority exists for bringing about the necessary reform in the commercial policy of the advanced nations" (p. 104). ("The necessary reform" is radical modification of a tariff structure that discourages imports of semiprocessed and processed goods.) In view of the strong head of steam subsequently built up for tariff preferences in industrialized countries for manufactured products from less developed countries, it is worth noting that Patel, an inventor of the proposal, was not bewitched by it in mid-1963. There is so much still to be done in removing restrictions and eliminating "what amounts to discrimination against the developing countries" that "there is little to be gained by diverting attention to more radical and less acceptable solutions" (p. 107).

Gerda Blau in her essay on commodity export earnings and economic

rowth breaks new ground with her estimates of the volume of commodity rade for which the underdeveloped countries are in effective control of upplies in import markets, the shares of primary exports from the underde'eloped countries which are affected by protected agriculture in the high-inome importing countries, and the shares influenced by competition from synhetics. Caroline Miles contributes a good analysis of the market for manuactured exports from underdeveloped countries and of the adjustments in ad'anced countries that would be required to accommodate more of those exıorts. She also suggests a sensible strategy for the employment of adjustment ıssistance.

Hart makes a persuasive case for a Development Insurance Fund to com)ensate countries for shortfalls in earnings from commodity exports. The conerence participants, in their Statement of Policy, strongly support the pro)osal, and indeed it would seem to this reviewer to be among the most widely ıcceptable of the "new directions." Perhaps this is only another way of :onfessing that it is easier to obtain changes in aid policy than in trade polcy.

Despite a veritable flood of U.N. documents prepared before, during, and ;ince the 1964 Trade and Development Conference, and a heavy rain of ɔooks and articles from other sources, the Chatham House Report—though it acks an index—remains a handy and readable compendium.

ROBERT E. ASHER

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Industrial Organization; Government and Business; Industry Studies

Strategy for Mobility. By WILFRED OWEN. Washington, D.C.: The Brookings Institution, 1964. Pp. xii, 249. $5.00.

Wilfred Owen's book is the first of 15 studies on the role of transport in developing countries to emerge from the Brookings Institution's Transportation Research Program begun in 1962 under a grant from the Agency for International Development. It is a welcome addition to the dearth of scholarly works on this subject, whose importance is suggested by allocations in developing nations of 20 to 40 per cent of public investment to transport.

Started with funds from the Ford Foundation, Owen's book was designed to provide a general conceptual framework for studies of transport investment criteria, pricing and financial policies, government control and administration, transport cost and location, transport and geography, and the effects of specific transport investments on economic development in particular countries. These studies constitute an ambitious program of research to sharpen the theoretical framework for determining the role of transport in economic development; to disseminate useful information on transport in developing countries buried in the many official and consulting reports in the files of governmental and international agencies; to evaluate the far-flung transport aid programs of the International Bank for Reconstruction and De-

velopment, the Agency for International Development, and other such agencies; and to suggest means of improving the contribution of transport to economic development in the developing countries.

In Chapter 1, Owen notes the adverse effects of poor transport and some examples of beneficial effects of modern transport in selected countries. Contrasting the mobile and immobile countries, he finds a "tremendous gap between the transport capabilities of rich countries and poor" and cautions that closing the gap will be difficult in view of growing populations, rising living expectations, and natural and man-made obstacles to improving transport. Although the assumption that "appropriate transport facilities are the key to development" is evident throughout the work, Owen views transport as "a necessary but not a sufficient condition for economic development." The crux of the matter is finding "in what circumstances and on what basis the supply of this ingredient of economic progress is critical."

In Chapter 2, Owen shows that beaking the transport barrier historically required a large portion of available resources, a long time to innovate more efficient technology, and a big push in the transport sector. But he believes that developing countries can overcome the time element by leapfrogging some historical stages of transport development—they can choose from the wealth of modern technology and modes available today. And they can find the most suitable transport and minimize the drain on total resources by resort to more effective planning, including plant locations and other arrangements to reduce transport demand. Chapter 3, which describes weaknesses in planning the transport requirements of India and Pakistan, emphasizes that transport needs derive from studies of resources, agriculture, and industrialization; that insufficient attention was given to the probable increases in traffic in particular commodities and on specific routes; and that inadequate emphasis was placed on road transport in the five-year plans. Chapter 4 presents interesting data on division of traffic in less developed countries and, reflecting experience in the United States, summarizes the efficient traffic role of each mode. Owen recommends greater utilization of modern technology, especially road transport, stating that many "plans for future transport development seem to deny the transport revolution and favor instead a reconstruction of the past."

Owen's conceptual apparatus for deciding transport investment and related questions is given in Chapters 3 and 5. In judging need for transport investment, "the essential consideration is how it will be used to raise levels of living." Need, however, is reflected in traffic estimates, although Owen, particularly in establishing a road network for the first time, would not limit transport investments to self-liquidating projects. Need "is influenced by many social, cultural, and political factors, not merely by economic considerations," and "will be justified by comparing the cost of the facility and related investment with the net addition to the national product that building the facility is expected to create." Cost minimization comprehends cost of way as well as of vehicles, cost of nontransport investments essential to the anticipated traffic, effect of project costs on system costs, and price adjustments for scarce for-

eign exchange and plentiful labor. Assuring a net contribution to national product greater than what might be achieved if the resources were used another way involves relating estimated traffic to the cost of the needed facility and close assessment of "the economics of alternative transport methods."

Owen regards satisfactory institutional arrangements for constructing, maintaining and operating transport systems, and for financing necessary preconditions for an effective transport program. Noting that the typical practice of operating railways and highways at deficits comprises a relatively heavy drain on less developed economies, Owen lists many advantages of self-supporting user fees and of rail rates restricting excessive demand, encouraging use of excess capacity, and shifting short-haul traffic to trucks. But secondary and feeder roads, comprising up to 90 per cent of the highway system in developing countries, cannot be financed with user fees and tolls. Their external economies are viewed as so significant that Owen would develop them with public works programs, often, but not always, utilizing labor-intensive methods. Owen is critical of regulatory restrictions to protect railway traffic, favors lodging all public transport responsibilities in a ministry of transport, and feels that integrated transport, as in Canada, would maximize advantages of a total system approach, though it might lead to greater public ownership of transport. He endorses regional transport systems to minimize investment, encourage intraregional trade, and to obtain the economies of scale.

In Chapter 6, Owen reviews the transport assistance programs of the World Bank, IDA, Export-Import Bank, AID and its predecessors, and regional agencies. He finds that railways have obtained 38.7 per cent and roads 31.7 per cent of the $7.2 billion of cumulative aid for transport. He judges the effects of transport aid and technical assistance programs to be impressive but suggests, as measures to achieve greater effectiveness, that more of the aid for transport should be through international channels; that transport development should be a continuing effort with greater emphasis on grants; that pre-investment surveys of transport needs should emphasize determination and limitation of transport demand and assessment of alternative technology to avoid costly misallocation of resources; and that a global research and development effort, through a World Transport Center for the United Nations, should be established.

Owen's book will serve as an excellent orientation to the role of transport development in developing countries and in aid programs. It will be particularly useful in training transport specialists for those countries, and in stimulating greater awareness of the significance of transport to economic development in several ways, including serving as readings for college courses in economic development and transport. Owen achieves more in indicating the factors to be considered in making transport development decisions than in specifying precisely how the criteria suggested for guiding investment can be applied. Without questioning the need to emphasize choice of the best modern technology, one can question some judgments expressed concerning the need for main roads for heavy vehicles parallel to established railways with excess capacity and lower marginal costs. On the whole, however, Owen has pro-

duced a book comprehensively and objectively analyzing the elements of the entire problem, providing interesting and not hitherto available data on the transport problems of developing countries, and supplying general guidelines for avoiding mistakes in planning transport development and for improving the effectiveness of such programs.

JAMES C. NELSON

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Urban Transportation—The Federal Role. By GEORGE M. SMERK. Bloomington: Indiana University Press, 1965. Pp. xiii, 336. $7.50.

Marketing Urban Mass Transit—A Comparative Study of Management Studies. By LEWIS M. SCHNEIDER. Boston: Graduate School of Business Administration, Harvard University, 1965. Pp. xv, 217. $5.00.

The publication of these two books is of more than usual interest since they are concerned with one of the most frustrating problems faced by our nation. The United States today is experiencing significant changes through urbanization. During the last century, the nation has proceeded from a rural-agricultural society to an urban civilization with 66 per cent of the nation's population in 1960 residing within standard metropolitan statistical areas. By 1975, this percentage is expected to increase to 75 per cent and by 1985 to approximately 80 per cent.

Both authors point out that this concentration of people and resources in urban areas would have been impossible without the increased mobility afforded by the automobile. Each year urban America is spreading at the rate of a million acres. The authors point out that major problems have resulted from the mass migration of people into urban areas of our major cities. Traffic congestion has been rated as the number-one problem; there are too many vehicles and people in proportion to available space.

As economists, the authors are thus faced with a partial manifestation of the basic economic problem. Professor Smerk states: "Given the technicians and a sufficient supply of land, labor and capital, the congestion problem can be easily cured. But economic resources are scarce and therefore expensive. A critical factor in carrying out the transport policies necessary to meet the stated objectives is thus the process of choosing among alternative possibilities to find the methods of achieving the desired end at the least economic cost" (p. 234).

Unfortunately, Smerk's study is designed to concentrate on institional factors rather than extensive economic analysis. The author devotes only two out of eleven chapters to the application of economic analysis to the urban problem. This is a most important part of the book, but unfortunately not analytical enough.

The author concludes with several possible solutions to the urban transport problem, including decentralization of cities, greater use of mass transport, more judicious use of federal subsidies, and the setting of transit prices so as to control peak-hour-traffic flow. The author agrees with most other transport economists that in no other major sector of our economy are pricing practices

so irrational, so out of date, and so conducive to waste as in urban transportation. Two aspects emphasized are the absence of adequate off-peak differentials and the underpricing of some modes relevant to others.

Finally, Smerk makes specific recommendations for legislative and administrative programs on the federal, state, and local levels with emphasis on an Urban Transport Agency aimed at encouraging effective action. In general, the book is well written and is a contribution to the urban transportation literature.

Professor Schneider discussed the congestion problem from a different point of view. His analysis focuses on management in the urban mass transit industry. He recognizes, but does not include in this analysis, such important items as technology, social costs, or public benefit. The bulk of the study draws on data obtained from a questionnaire sent to transportation executives, and from case studies developed from interviews with transport officials and members of the press.

Much of the book is a shortened version of the author's doctorial dissertation and follows a two-stage plan of analysis. Schneider first stresses the importance of external marketing strategy by analyzing the mass transit industry's financial and passenger trends, the pressures confronting management, and the possible alternatives available to management. Marketing strategy is defined by the author as the level of rates, market research, promotion, and product planning policies designed to stimulate traffic and in the process generate or maintain an adequate profit. Secondly, internal strategies are analyzed. These strategies include labor relations, recruiting, research and development, finance, and marketing.

Some of the interesting findings of the Schneider study concern the evaluation of external and internal marketing strategy and the weaknesses in formal organization. For example, the study concluded that too much effort was being devoted to maintaining descriptive statistics of persons riding transit rather than analyzing which persons were not riding.

The evidence also showed the importance of integrating marketing and political strategy. Schneider feels that urban transit has always operated under political pressures and these pressures will increase as local, state, and federal participation increases. As the author notes in Chapter 2, transit fate rests largely on CBD and central city traffic. Furthermore, the state and federal government are now providing financial aid for the transit industry. In view of these recent changes, the external and internal strategies of mass transit management take on added importance.

The major recommendations of the study are directed to the transit industry. The author recommends that the transit industry plan and implement integrated marketing alternatives, reflecting price, product planning, market research, and promotion designed to attract different classes of riders. He outlines a series of services ranging from a premium-fare and premium-comfort express route to a low-cost service for school children. Schneider concludes by presenting a specific plan for reorganizing large transit companies to sell as well as provide transit service.

In summary, Schneider has focused attention on the problem of marketing urban mass transit. Schneider deserves praise for his efforts which represent a step in the right direction and should be used as a point of departure in future studies in this important area.

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Railroad Mergers and Abandonments. By M. CONANT. Berkeley: University of California Press, 1964. Pp. xiii, 212. $7.50.

This book by M. Conant contains a systematic and thorough review of the law respecting railroad merger and abandonment and its application. The effect of regulatory practice on efficient resource allocation in railway transportation is also well developed. However, the economic criteria adopted to support public policy recommendations are narrow in scope, and the analysis itself is deficient in many respects.

The model adopted by Conant to test efficiency of resource allocation is too simple. Basically, he argues that where five or six railway lines connect two urban areas, the markets would, of necessity, be more efficiently served by a reduction in the number of railways; that interrailroad competition is inadequate to secure the desired reduction; that existing regulatory methods are inadequate to enforce it; and that the necessary contraction in facilities can only be adequately attained through planning by government and enforced acceptance of merger by railway management through a system of tax incentives.

The analysis is based entirely on the economies in road maintenance and road property investment which occur as traffic volume increases. Therefore, the fewer railway lines over which traffic is diffused, the lower the cost of resources used in moving traffic. The author demonstrates the low traffic density on a significant portion of the railway plant. Unfortunately for the author's argument, urban markets are not the ones primarily served by railways. A demonstration that five or six railways operate between two urban centers does not mean that they serve the same market. Each railway may have a branch-line system that taps different intermediate territory and so develops density for the main line. Different intermediate points are usually served by each railway. It requires a most complex analysis to show that light density per se on a significant portion of the railway plant results in less-than-optimum resource allocation. Branch lines, for example, have light density almost by definition. Nonetheless, it requires an extreme degree of light density before the social and private costs of originating bulk traffic by highway are less than those of originating it by railway branch line.

The author is quite right in his conclusion that interrailroad competition itself cannot be relied upon directly to secure "operative social control"; that is, to adjust the size of firms to changing cost and demand conditions. By not examining the implications for railroad merger and consolidation of intermodal competition, however, the point of the exercise is in danger of being lost. Surely the current merger movement among U.S. railways is caused primarily

by the effects of competition from other forms of transportation or transportation markets. Much of the L.C.L. traffic, as well as carload traffic moving short distances, is now moved by highway, and this requires not only abandonment of certain railway services, but has led to different types of railway markets giving rise to the need for merger as a better means of accommodating these markets and to give effect to operational economies. Indeed, the failure to recognize, in regulation and in public policy, the profound implications of intermodal competition is undoubtedly the major reason for the slowness in railway adjustment to changing cost and demand conditions.

The book contains one of the best reviews of the application of the antitrust laws to railroad consolidation, economic appraisal of recent mergers, and regulation of abandonments and discontinuances with which I am familiar. The author shows that regulatory policy has failed to appreciate fully the inconsistency between achievement of maximum efficiency in resource allocation and adequate service to the public. Clearly, there are many situations where competition from other forms of transportation with lower costs, as in passenger service, has made it impossible for railway service to be provided except at a loss. In these cases, commission rulings appear generally to require continued service unless the financial integrity of the railway company as a whole is affected. This implies cross-subsidization through the freight rate structure, and further impedes adjustment of railway plant and services to changed cost and demand conditions. While the author recommends that the onus be placed on persons opposing the abandonment to show cause for continued operation, I would be more inclined to permit abandonment on a showing that a financial burden was imposed by continued operation so long as alternative service was available, and unless the local, state, or federal government provided a direct subsidy to maintain operation. If this policy were followed, it would be possible to compare directly benefits and costs, and for them to be subject to a periodic review.

To achieve greater efficiency in resource allocation in railroad operation, the author recommends that mergers be planned and enforced by government, using as a criterion adoption of the lowest-cost alternative method of serving the markets involved. While great advances have been made in computer applications and in econometric methods, I do not believe that the method suggested is feasible. There are many nonquantifiable factors—including the ability of people to work well together, competitive incentives, political considerations, and so on. Certainly, it is not a desirable means of attaining rapid adjustment to changing cost and demand conditions. Many weak railways should be abandoned—they are weak because, in a highly competitive intermodal environment, their economic role is limited, and in most cases they should be abandoned or operated directly through state subsidization. If, as mentioned above, the railway plant has not been adjusted more rapidly to changing cost and demand conditions because the forces of intermodal competition have not been permitted to work effectively as a result of undue government intervention, increasing government intervention and substituting extensive planning by government for greater reliance on competitive forces is

not a sound policy. If the results of the competitive model are desired, and if, as I believe, intermodal competition can be relied upon to achieve those results, why not try competition, unfettered by extensive regulation?

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Competition in the Midwestern Coal Industry. By REED MOYER. Harvard Economics Studies, Vol. 122. Cambridge: Harvard University Press, 1964. Pp. xiv, 226. $5.50.

A well-known theorist used to amuse his graduate students by defining an industrial organization economist as "a person who has never set foot in a factory." This work is a welcome exception to the rule. Before defecting to *academia,* the author was vice president of a coal mining firm. His past experience shows up favorably in this perceptive addition to the literature on the bituminous coal mining industry. Nor is the reader forced to pay the customary price of an "inside" analysis. Reed Moyer maintains a commendable level of objectivity throughout, and he has acquired the economist's standard box of tools as well as those of the manager. The book in fact follows closely the conventional Mason-Bain model of an industry study: there are chapters on market delineation, demand, industry structure, cost relationships, entry barriers, market conduct, and market performance.

As its title indicates, this volume focuses on the midwestern coal industry—a producing area comprising Illinois, Indiana, and Western Kentucky which serves a market including those states plus parts of Wisconsin, Iowa, Minnesota, and Tennessee. Through a study of delivered cost differentials and actual sales patterns, Moyer argues that this seven-state area is a distinct market virtually free from outside competition, except at points directly linked by water with eastern mines. He pinpoints important structural differences between the midwestern industry and other U.S. soft coal markets. On the demand side, midwestern producers deliver a much higher proportion (60 per cent) of their tonnage to electrical utilities, thereby enjoying greater cyclical stability and less secular stagnation, along with the mixed blessing of long-term contracts. On the supply side, Moyer documents the midwestern industry's rapid development (mainly since 1954) into an oligopoly with a competitive fringe. By 1962 the leading four production consolidations accounted for 55 per cent of midwestern soft coal output. The leading 20 produced 90 per cent of total tonnage.

Most of this increase in concentration was achieved via the merger route. Many mergers were motivated by the desire of owner-managers to retire from the arena, Moyer observes. Still a lessening of competition was a definite consequence, and one 1955 merger with more suspicious motivation eliminated the industry's leading price cutter, bringing a bitter price war to an end. One cannot help wondering, where was the Justice Department?

Although Moyer does not raise the issue pointedly, government policy toward the coal industry has seemingly continued to follow the lenient line of the Appalachian Coals decision. Perhaps this is not such a bad thing, even though the rule of reason has fallen from grace in both price-fixing and mer-

ger cases. Moyer concludes that industry performance has improved as a result of recent developments, admitting at the same time that scale economies would not have to be sacrificed at much lower levels of concentration. Price warfare has been largely avoided since 1955, prices have been stabler than in the more competitively structured eastern markets, capacity utilization has increased relative to the nationwide average, and yet profits have not risen above the average for *Fortune*'s 500 largest industrials. The public interest is protected, Moyer asserts, by the pressure of rival fuels on prices and by the threat that eastern mines will begin to make inroads at higher prices. Modest but not insurmountable barriers to local entry also inhibit midwestern producers' pricing freedom.

Moyer's performance judgment appears reasonable enough. His prediction of similar structural developments for other coal markets has the ring of prophecy. Still the book's value would have been enhanced had Moyer provided more guidance on how much concentration is enough, for someone in Washington must eventually make that decision.

Unfortunately, Moyer's analysis does not penetrate deeply enough to afford a firm basis for predicting future behavior. He states that recognition of mutual interdependence now inhibits cutthroat pricing, but from an observer with his background, one would like a much fuller account of how the temptation to seek short-run gain is resisted. Why members of the competitive fringe behave as they do is especially puzzling. According to Moyer, marginal costs are either constant or falling up to the point where overtime premiums must be paid. Yet if this is true, why do the small mines average only a three-day week, while the big mines average four days per week? Small mines ought to have a compelling incentive to cut prices and expand output under the circumstances postulated. Moyer notes this anomaly, but he fails to explain it satisfactorily.

Certain other shortcomings may be noted briefly. The analysis of scale economies is marred by the use of tons per man-day as an index of productivity, with no adjustment for capital intensity, which rises with firm size. A calculation of demand elasticity under price discrimination (p. 60) is conceptually and therefore factually incorrect. An analysis of the relationship between firm size and the propensity to innovate would have been useful, in view of the major impact innovations have had on the industry. Some of the tabular data could have been interpreted more conveniently with the aid of statistical tools. The prose is usually clear, but not exhilarating. In general, however, Moyer's study represents a useful contribution.

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American Government and the Economy. By Emmette Redford with C. B. Hagan. New York: Macmillan Co., 1965. Pp. xii, 674. $8.95.

The significant and growing relationship between American government and business makes this text welcome. Emmette Redford has to a great extent succeeded in writing a thorough and useful book.

Part I deals with "The Role and Process of Government" in its historical

setting. The concept of macropolitics which centers on the office of the President is elucidated with some finesse. One of the great contemporary needs is to articulate a macropolitical system which will make the macroeconomic concepts operational for public policy. This need was illustrated vividly by the 13 agonizing months of discussion over the 1964 tax cut. Effective macropolitics permits rationality in dealing with problems on the general macro level as in the case of the reciprocal tariff program; tariff policy would disintegrate to petty argument if tariffs were determined separately for individual commodities.

Unfortunately controversial statements are made categorically. It is at least an exaggeration, if not an error, to say that Keynes' writing had "great practical significance" for the New Deal. Furthermore, the statement that technological changes have "quickened at a continuously—lately an enormously—accelerated rate" may not be true if we utilize as an index of technological change, the ratio of output to weighted inputs, including in inputs investment in education.

Part II entitled "The Constitution and the Economy" provides an excellent summary of the economic significance of the Constitution and its positive contribution to the development of the economy. Economists, as well as other social scientists, are often prone to underestimate the importance of political innovations for economic growth.

Part III, "Policies Affecting the Economy Generally," covers many areas including monetary-fiscal policy, antitrust problems, and wartime controls. The discussion of wartime controls is extremely well done and is enlightening. It is, however, incorrect to say that changes in reserve requirements are slow and ineffectual. Furthermore, the New Deal engaged in a salvage operation rather than a positive program of recovery. It is questionable that the New Deal deliberately unbalanced the budget as the author declares. Rather, the deficits were unavoidable in the face of the drastic decline in economic activity and the consequent desperate need to provide relief.

Part IV, "Regulation and Promotion of Industries," is of limited objectivity. In the discussion of the 27.5 per cent depletion allowance for oil producers, there is no indication that support for it does not come from professional economists. The 27.5 per cent depletion allowance is excessive and serves no social purpose; in fact, it may be antisocial in that it depletes our domestic oil reserves too rapidly.

Part V deals with the "Regulatory System" and is involved in the various aspects of administrative policies and procedures. Part VI deals briefly with "Public Enterprise" and a summary of the author's views.

The concept of "the public interest" is seemingly the unifying element. Yet the student will be left frustrated as to its meaning, unless the instructor supplies one. There are many allusions to the public interest and several pages indicate how others define the term. But Redford's own meaning is abstruse. If we accept his conclusion that "politics is the final court of appeal" we are left with a somewhat nebulous concept. It is true, as he strongly states, that even the economists' usual criteria for efficiency would be cast aside by the economist *qua* politician if the political goal were more desirable.

What is not made clear, however, is where and when public and private welfare may differ. An excessive depletion allowance is politically possible, but has elements of private gain and public loss. It might not be a mistake at this juncture to refer to Lionel Robbins' view that economic science deals with questions of efficiency and takes values as given.

This is not to say that the book lacks objectivity. The economist *qua* economist takes the values of society as given. It is not always clear whether Redford is dealing with a question of fact (a scientific question) or a question of value. If the end is to conserve resources and to maximize consumer satisfaction, then the economist and political *scientist* probably would conclude that the 27.5 per cent depletion allowance has consequences which are socially undesirable.

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Land Economics; Agricultural Economics; Economic Geography; Housing

Agriculture and Economic Development. Edited by CARL EICHER and LAWRENCE WITT. New York: McGraw-Hill Book Co., 1964. Pp. 415. $10.00.

Deciding which articles to include when editing a book of readings must be like making a guest list for a cocktail party. You cannot ask all the people you like, you cannot avoid inviting one or two bores, and you never know anyway if at the last minute a freeloader will crash the party.

Agriculture and Economic Development presents in a single, ugly volume which looks waterproof, a potpourri of 25 articles which are more or less hard to find and which mix with different degrees of competence and clarity the fields of agriculture and development. The book is divided into four parts: (1) "Agriculture and Economic Development in Historical Perspective," (2) "Measurement Problems in the Agricultural Sector," (3) "Theoretical Aspects of Agriculture in Economic Development," and (4) "Some Aspects of the Process of Change in Agriculture." The editors preface each part with a short and chatty introduction.

The opening article by William H. Nicholls, "The Place of Agriculture in Economic Development," vintage 1960, is an unexpectedly militant piece in which Nicholls presents a hurried view of the role of agriculture in modern economic history. At one point he argues that the agriculture of Argentina was characterized by "optimum size farming units" (p. 17). He scolds Wilfred Malenbaum because in his well-known article "India and China: Contrasts in Development Performance" (*AER*, June 1959), he found that the agriculture of China was developing faster than the agriculture of India; and he refers to the Japanese experience in glowing terms and recommends it for the rest of Asia. After this article, my respect for the sure hand and wisdom of Alexander Gerschenkron went up a couple of notches.

Kazushi Ohkawa and Henry Rosovsky write a succinct and competent study on "The Role of Agriculture in Modern Japanese Economic Development," and Arcadius Kahan does the same thing for the Soviet Union in "The Collective Farm System in Russia: Some Aspects of Its Contribution to Soviet Economic Development." The experience of the United States is analyzed in several articles. A first-rate paper by Douglass C. North, "Agriculture in Regional Economic Growth," blends historical knowledge and economic analysis superbly, while establishing a contrast between the economic structure of the South and the West in the years prior to the Civil War. Earl O. Heady's paper, "Public Purpose in Agricultural Research and Education," is unexpectedly readable and it is not marred by a single formula! Dale E. Hathaway analyzes the role of migration in the United States in "Migration from Agriculture: The Historical Record and Its Meaning."

Only five articles are dedicated to the theoretical aspects of agriculture in economic development, and this is the really disappointing part of the book. "Disguised Unemployment in Agriculture: A Survey" by Kao, Anschel, and Eicher is an instant rehash of the controversy on disguised unemployment. In seven pages the authors squeeze 58 references! "Economic Theory and Agrarian Economics" by N. Georgescu-Roegen is a valuable article and it certainly deserved being reprinted. Here one finds keen insights into the differences between feudalism and capitalism. "A Theory of Economic Development" is the important article that introduced the model of Ranis and Fei. This is followed by a comment by Harry T. Oshima on the same model. A good, straightforward discussion of the role of agriculture in economic development is conspicuously missing and one wonders why the editors excluded from this mixed company the excellent article authored by Bruce F. Johnston and John Mellor.

Resource theory and location theory get little attention. A brief note by T. W. Schultz, "Connections between Natural Resources and Economic Growth," lists bluntly many of the doubts and incapabilities which modern economics suffers on the resource front. J. H. Dales in a comment to the previous article duly notes that Schultz neglected introducing locational approach. Robert E. Baldwin in "Patterns of Development in Newly Settled Regions" applies production functions to the analysis of regional growth. Simon Kuznets, Glenn L. Johnson, and Zvi Griliches discuss how to measure things in the agricultural sector. Griliches' article, "Research Costs and Social Returns: Hybrid Corn and Related Innovations," displays truly dramatic returns.

Land reform is discussed in two articles: "Economic Theory and Agrarian Reform" by V. M. Dandekar, and Doreen Warriner's famous article, "Land Reform and Economic Development." Both are essential items in any bibliography of land reform. Finally, there are two refreshing contributions by W. Arthur Lewis and Albert O. Hirschman.

The over-all impression left by this book is one of contradiction and confusion. The appalling lack of consensus that pervades the discussion of agriculture and development is faithfully registered all along. Each single assertion

seems to cause a counterassertion, and both appear to be equally valid or invalid. Here lies perhaps the most valuable contribution of these essays, since discord and dissent are presented at about the highest technical and intellectual levels that the combined professions of the agricultural economist and the economic developer can muster.

EDMUNDO FLORES

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Farmers in the Market Economy—Market Organization and Competitive Behavior in Relation to Farmers' Prices, Costs and Incomes. Ames: Iowa State University Press, 1964. Pp. vi, 308. $4.95.

This volume publishes the papers that were recently presented at one of a continuing series of conferences sponsored by the Iowa State University Center for Agricultural and Economic Development. The individual papers are highly variable in quality and relevance to the conference theme, as is usually the case with a product formed in this way.

The choice of the nondefinitive title *Farmers in the Market Economy* was unfortunate. A better choice would have been the book's subtitle, *Market Organization and Competitive Behavior in Relation to Farmers' Prices, Costs and Incomes,* which does have content. But then some of the chapters would have been incompatible with a more restrictive title.

The preface and subtitle of the book suggest that the objective is to examine the often-repeated proposition that farmers' prices and incomes are lower and their costs higher because they possess no bargaining power, i.e., the agricultural sector is perfectly competitive while the industries that it meets in the marketplace are imperfectly competitive. Such an undertaking is both ambitious and overdue. While some progress is achieved, this reviewer is left far from satisfied.

More progress would be made in this area if economists could break away from a common line of thinking which proceeds as follows: when there are "small" numbers of firms in an industry, product differentiation, and limited entry of new firms, then imperfect competition exists, and therefore (*sic*) "excess capacity," malallocation of resources, and higher prices for things sold and/or lower prices for things purchased occur than would exist under perfect competition. The researcher concentrates on measuring the degree of imperfection. This reviewer believes that the approach is faulty because the existing theories of imperfect competition have very low predictive power with regard to prices, and little or no attention is given to economies of scale, which is the force that drives industries to "small" numbers and imperfect competition.

Part I, "Market Power in the U.S. Economy," considers the general existence and effects of imperfect competition. In "Its Sources, Distribution and Consequences" R. B. Heflebower argues that inequality of factor earnings between the farm and nonfarm sectors of the economy is more a function of factor mobility than relative "bargaining power."

John B. Sheahan in "Its Relation to Growth and Employment" argues that

imperfect competition leads to a cost-push pressure in the economy with dire consequences for economic growth and employment. The free use of monetary and fiscal policy cancelled these effects during the decade following World War II, but with the current world economic integration new solutions must be found for the problem.

Julius Duscha of *The Washington Post* presents an interesting view of economics as observed in the nation's capital in a chapter titled, "The Politics of Market Power." What he sees can be summarized in his statement that "In the abstract we are all for competition, but when it gets down to specifics we all want to pull as many governmental strings as possible to get the jump on our competitors."

Part II, "Competitive Organization and Performance of Agricultural Industries," focuses on industries that agriculture meets in the marketplace. Lehman B. Fletcher in "Market Structures and Market Power" gives a thorough presentation of the available quantitative information on industry concentration. From 1947 to 1958 the evidence suggests more reduction in concentration than increase, although there is no general pattern.

Part III, "Organization, Productivity and Income Distribution in Agriculture," concentrates on structural changes within the agricultural economy. Harold F. Breimyer in "The Changing Institutional Organization" outlines the role of capital in causing structural changes and discusses vertical integration with its implications for price determination and the future organization and control of farm production.

In "Productivity, Farm Policy and Income" Earl O. Heady argues that past farm programs have encouraged technological advancement in agriculture also forcing major structural adjustments. The government programs have diverted competition from product price to factor prices, particularly rising land values.

The final chapter in this section, "Determining Factor Shares," by Gordon A. MacEachern and Vernon W. Ruttan contains more substance than most of the other chapters combined, but it is only remotely related to the conference theme. Government programs have reversed the declining factor share of farm real estate and allowed the farm labor share to continue its long downward trend.

Part IV, "Improving Farm Income Through Collective Action," is the section that is most compatible with the subtitle of the book. Dale Butz in "Cooperative Marketing and Purchasing" discusses the past and potential effects on the farmer's prices resulting from farmer co-operatives that purchase factors and market farm products. "Cooperative Bargaining" by Wendell McMillan reviews the experience and prospects for co-ops that are organized primarily to achieve "bargaining power." Sidney S. Hoos in "Marketing Agreements and Orders" merely updates his thorough coverage of this topic which he has published before. The potential of "Marketing Quotas" is discussed by Elmer W. Learn. It appears that none of the authors in this section is ready to propose the topic of his paper as *the* panacea for the "farm problem," although each device surely has some value, at least in the short run.

Part V, "Perspectives on Research Problems," finds S. Kent Christensen, of the National Association of Food Chains, and Wilbur Maki seriously questioning the value of the "Market Structure" research framework which is currently in vogue among agricultural economists.

This book is worth reading for anyone who is interested in agriculture's status in a society in which it is losing its identity. But those who are well read in this area of economics will find only a little material that is really new and relevant.

ROBERT S. FIRCH

University of Arizona

Agricultural Market Analysis—Development, Performance, Process. Edited by V. L. SORENSON. East Lansing: Michigan State University, Bureau of Business and Economic Research, 1964. Pp. xv, 344.

This book contains 16 chapters written by members of the Department of Agricultural Economics at Michigan State University. It developed from a series of faculty symposia. It is well written, well edited, and easy reading.

Chapter 1 is an introduction. Part I, "The Overall Role of Marketing," deals with the linkage between social organization and the physical and economic relationships included in the marketing process. Its theme—which recurs throughout the book—is that marketing is but one part of a total social system. Chapter 2, "Marketing in Social Perspective," emphasizes this by noting that "The question of what is a good marketing system cannot be separated from the more fundamental question of what is a good society. . . ." It utilizes concepts of rule, role, position, institution and norms and discusses efficiency, freedom, and human relations as performance results. Chapter 3 describes structural changes observed in economic growth and some growth theories. It touches on the links between production and the social system concepts discussed in Chapter 2 and it uses examples from United States economic history to illustrate interactions between social (and judicial) systems and economic development. The examples deal with court definition of market contract rules, bankruptcy procedures, corporation laws and exchange value as property.

Part II, "Firm Behavior and Adjustment Processes in Agricultural Markets," constitutes half the book. The main focus is on consideration of firm and group behavior in a dynamic setting. Chapter 4 treats "The Firm as a Focal Point in Market Analysis" as a dynamic behavioral unit influencing and influenced by market performance. Chapters 5 and 6 briefly review firm theory: the first covers pure competition and pure monopoly, the second covers imperfectly competitive firms. The next chapter takes up "Advertising in the Marketing Process." It discusses the measurement of effects of advertising on sales, summarizes the issues involved in evaluating the contribution of advertising to social welfare and describes the special problems facing farm groups in using advertising. Chapter 8, "Quality Competition and Product Development," is largely a description of business practices in this area. Chapter 9 treats "Firm Growth, Diversification and Integration." The next

chapter describes various kinds of organized groups involved in agricultural marketing and the kinds of actions taken by these groups. Chapter 11 is devoted to characteristics, development, objectives, advantages and limitations, and the future of agricultural cooperatives.

The two chapters of Part III are oriented to the macro level. The first deals with determinants of consumer demand, farm supply, marketing margins and price fluctuations. The second deals with "Market Organization and Performance." It covers changes in market organization, organizational structure of agricultural markets and interdependence of market organization and performance.

Part IV treats "Public Policy in Agricultural Markets." Chapter 14 points out that the basic determinants of public policy lie in society's value system. It uses the economic history of the United States to illustrate how public policy evolves as an economy moves through various stages of economic growth. Chapter 15, "Policies for the Regulation of Competitive Behavior," is mainly a description, using court cases and FTC cases as examples, of the application of antitrust laws to agriculture and farm-related businesses. In Chapter 16 attention shifts from policies to improve the market exchange system to policies that supplement that system. It describes programs to provide market information on prices, grades, quantities, etc.; programs aimed at reducing cost and increasing firm efficiency; programs to promote orderly marketing; and (income and commodity) redistribution programs.

In his preface, the editor writes that this book is intended "to stimulate discussion and inquiry into a series of questions related to agricultural marketing." I fear it will stimulate little "discussion and inquiry" among professionals. The topics covered are treated too descriptively or too briefly (even superficially) to generate much "discussion and inquiry" on my part. (This could be due to my own intellectual insensitivity; I like to think not.) Chapters 2, 3, and 4, however, did whet my appetite for reading some of the literature cited to learn more about the topics covered in these chapters.

The editor also writes that this book is "designed for use by students and others who wish to pursue the study of agricultural marketing beyond the first course or introductory level." The book would be useful as supplementary reading in intermediate or graduate-level courses in agricultural marketing. For this purpose its brevity is a virtue: enabling the reader to obtain a quick overview of a variety of topics. If I taught an introductory course in agricultural marketing to students with training in elementary microeconomic theory, I should try an experiment: devote part of the term to selected portions of a conventional agricultural marketing text[1] and part of the term to *Agricultural Market Analysis.*

GEORGE W. LADD

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[1] For example, Chapters 1-10 and 14-21 in Geoffrey S. Shepherd, *Marketing Farm Products* (4th ed.), Ames, Iowa, 1962.

Labor Economics

Economic Growth and Employment Opportunities for Minorities. By DALE L. HIESTAND. New York: Columbia University Press, 1964. Pp. xx, 127. $6.00.

This book seeks to identify the economic factors which are systematically related to changes in industrial, occupational, and income patterns for Negroes in relation to whites. Relying chiefly on the decennial censuses from 1910 through 1950, Dale Hiestand measures changes in the occupational profile and industrial structure of employment of Negro men, Negro women, and white women in relation to white men. The measurements primarily are of two related kinds: a comparison of the fraction which each of these labor supply groups was of total employment in particular sectors at the start of the decade with the fraction that each contributed to net sector change during the decade; secondly, percentage changes are reported for the decade's experience with respect to total employment against group employment within individual sets of occupations or industries.

In the end, after 181 pages of analysis, Hiestand re-emphasizes his key conclusion, the decisiveness of changes in economy-wide conditions to changes in the relative economic position of Negroes, and asks: "What then accounts for the variations in changes in the employment of minority groups within particular fields?" The answer is that "the economic variables on which this analysis centered did not give us an answer to this question."

Among the disappointing hypotheses that added up to this result was the finding that differences in the rate of growth in employment volume of particular occupational groups or industries have not "explained" differences in the internal changes in minority group employment (p. 77). Likewise, the rate of technological change as represented by the rate of change in productivity was not significantly correlated with changes in Negro male or female employment (p. 105). Also, for the period 1939 to 1949, there was, in general, no systematic relationship between changes in income levels and Negro male employment (p. 98).

When it comes to measuring the economic status of Negroes as compared with whites, one of the most important of Hiestand's contributions is his refinement and extension of Becker's (*The Economics of Discrimination*) index of relative occupational position. Becker had divided the occupational structure into three groups only. Hiestand, correctly pointing out that as the number of groups used is increased the index becomes more sensitive to occupational shifts, enlarges the technique to include seven occupational categories, including farmers—a group Becker omitted. But despite these reformulations, as Hiestand acknowledges, Becker's conclusions stand essentially untouched. The occupational position of Negro men in relation to white men changed hardly at all from 1910 to 1940, then it improved by 5 per cent in the 1940's, increased only a trifle more in the 1950's despite the rapid decline

of Negroes in agriculture, and in 1960 the index, using 1949 income weights, still had 22 per cent to climb to reach equality.

In his concluding remarks, among the most interesting in the volume, Hiestand speculates to the effect that for Negroes to catch up with whites, perhaps it is essential that they must somehow leapfrog into the growing edges of the economy—even if these edges have to be of their own making, as the Jews more or less did in movies, medicine, and so forth. That is, while equality is still remote, its attainment will not even be assured by fraternity, at least to the extent that the latter does no more than facilitate employment in conventional pursuits. Since the occupational and industrial mix is ever-changing, fundamental progress must come from Negroes leading the way into new and expanding sectors rather than merely catching up with whites in old and declining areas. Hiestand, of course is quick to remark that the question of how this is to be done may be beyond answering so long as the problem is couched in these group-breakthrough terms. Thus the burden shifts to individual Negroes and to the quality of the educational system upon which they must depend.

Given the essentially negative results that Hiestand uncovers, few readers will be likely to search out the additional detail in the Ph.D. thesis underlying this volume, but, as always in diagnosis, competently developed negative results are as important as positive ones—even if they are not equally interesting.

ROBERT L. RAIMON

Cornell University

Organized Labor in American History. By PHILIP TAFT. New York: Harper & Row, 1964. Pp. xxi, 818. $9.75.

Professor Taft has given us a warm, appreciative account of the development of the American labor movement into its present form. *Organized Labor* contains much up-to-date material (in fact, it tends to weight the twentieth century rather heavily relative to the nineteenth century and earlier) and is certain to find its way into many reading lists in courses of labor economics. As an economist, the reviewer finds this use of *Organized Labor* of the greatest interest.

Taft is at his best in showing how the great A. F. of L. unions grew in the face of bitter opposition. The student will come away with the thought that the successful union leader of this period was like a general, who must know when to risk all to exort a concession from an employer, but also when to avoid a fight at all costs, resisting employer provocation, squelching militant opposition, and making an orderly retreat to new positions of strength. It is probable that this analysis by Taft leads to an understatement of the positive value of rank-and-file opposition, or intraunion factionalism, but it also helps us to understand the mentality of the great union leaders and their supporters.

The reaction of the A. F. of L. leadership to the growth of industrial unionism in the nineteen-thirties and forties is examined in some detail. The

differing positions of the old-line AFL'ers as well as those of Lewis, Dubinsky, and Hillman, are presented with sympathy and understanding.

Another strong point of this book as a text is Taft's description of the forces moving both toward and away from centralization of the trade union movement, and his analysis of the importance these tendencies had in delimiting the roles that could be played by the various union leaders. For example, the difference in the power of the presidency of the C.I.O. during the tenures of Lewis and of Murray, or between the part of Murray in the C.I.O. and Murray in his steel union, are clearly presented.

Taft's book will not satisfy those who believe that the trade union movement is today in a state of crisis. He emphasizes the phenomenal growth of the trade union movement since 1929 rather than its relative stagnation in the past decade. *Organized Labor* says relatively little about the problems of organizing the unorganized in the South, the changing occupational structure in favor of the traditionally nonunion sector, and of the attempts of unionists to overcome these obstacles. This treatment probably results from the fact that the book's dominant concern is with the mainstream of organized labor in the United States (i.e., with successful, rather than with unsuccessful, trade unionism).

Taft does devote a chapter to the problem of corruption and racketeering and another to that of racism in the trade union movement today. These sections seem to be rather defensive in tone. In any event, Taft does not appear to be disturbed by the fact that the main impetus for the elimination of racism and corruption in our society comes from without, rather than from within, the labor movement. The conclusion that these evils will only be curbed when the larger, business-oriented society moves against them for its own reasons would probably be quite an acceptable one for Taft, for he is an advocate of business unionism as the appropriate form for the American labor movement. While business union leaders often use a rhetoric of alienation (refusing to be "taken in" by an ideology, but simply trying to further the immediate interests of their members), their academic interpreters, at least, are quite aware of the assumptions implicit in the business-unionist view.

Taft expresses this ideology in the following terms: "Business unionism stresses limited objectives, immediate improvements and eschews broader programs of social and political change. It depends upon the willingness of workers to organize for mutual help, upon the existence of an expanding economy as well as political freedom and civil rights for special groups to organize for the promotion of their own interests" (p. xv). Again: "As in the past, unions remain largely powerless, except for delaying actions, against the erosions of their positions by technical innovations (automation). Relieving unemployment generated by such changes has always been and remains beyond the power of labor unions, although they have made some contribution in this area by labor-management arrangements. Yet like the level of employment in general, the reabsorption of the technically displaced, their retraining, and offsets to wage loss must be the primary responsibility of government" (pp. 708-9). Again, "From the conspiracy trials to the Landrum-Griffin law, gov-

ernment has defined the rights and duties of labor and management; non-i tervention is largely a myth" (p. xvii). "No government can stand idly b while a union or employer paralyzes the economic life of the country over wage difference" (p. 573).

However, there is a feedback: the labor movement may affect labor legisl tion and may even help to control other groups through the political proces While Taft does sketch some of the history of labor in politics and of soci unionism in general, there is less of this history than the reviewer would lik to see in a text entitled *Organized Labor in American History.*

In general, as the student of labor economics becomes convinced (perhap by reading H. G. Lewis' *Unionism and Relative Wages in the U.S.*) th labor unions in the United States have had somewhat less effect on relativ wages than he had thought, his interest naturally shifts to other aspects o union activity, such as their political efforts or the changes they have brough about in industrial relations. Taft's treatment is more traditional; his empha sis is on the struggles of trade unions for higher wages and better workin conditions.

JOHN D. OWEN

The Johns Hopkins University

Public Policies and Manpower Resources—Proceedings of a National Man power Council Conference, 1959. New York: Columbia University Press 1964. Pp. xvii, 470. $8.50.

Government and Manpower—A Statement by the National Manpower Coun cil with Background Chapters by the Council Staff. New York: Colum bia University Press, 1964. Pp. xvii, 470. $8.50.

Manpower Policies for a Democratic Society—The Final Statement of th National Manpower Council. By HENRY DAVID. New York: Columbi University Press, 1965. Pp. xv, 121. $3.75.

The National Manpower Council was established at Columbia University i 1951 under a grant from the Ford Foundation. The Council, composed o leaders from industry, labor, education, and public service, had two goals One was to make studies of significant manpower problems. The second wa to make contributions to a more effective development and utilization o human resources. In implementing these goals, the Council has authore studies and convened conferences, the proceedings of which have then bee published.

Two of the volumes being reviewed are illustrative of these approaches They represent the Council's concern with the roles of governments both a demanders of labor and as major conditioners of the nation's supply of labor *Government and Manpower* includes a 75-page statement on the various fac ets of the government's role in manpower affairs by the Council, and te background chapters prepared by the staff. The staff chapters cover the sam subjects as does the statement and provided the basis upon which the state ment was drafted. The Council also called a conference at Arden House t consider many of the same problems. The results of the conference, six pre

pared papers, excerpts from five of the discussions, and a final statement of the participants are contained in *Public Policies and Manpower Problems.*

The third volume, *Manpower Policies for a Democratic Society,*[1] is the Council's eleventh and final book. Its characteristics are quite different from those of the earlier volumes. The previous volumes, other than the two being reviewed here, have been concerned with student deferment, scientific and professional workers, the skilled employee, women's role in the labor force, and education as it relates to manpower. Their formats have been similar to those of two of the books being reviewed; either conference proceedings or a Council statement based upon, and published in conjunction with, staff chapters. As a final statement, *Manpower Policies for a Democratic Society* naturally draws heavily, though not exclusively, upon the Council's published studies. It is designed to present the Manpower Council's position on basic aspects of the supply of manpower and its utilization, and to suggest some key guidelines for the determination of appropriate manpower policies in our society.

In considering governmental demand for labor, the first two volumes discuss the character of public employment, assess society's view of individuals who seek such employment, present the occupational, compensational and nonwage characteristics of public agency employment, and contrast these with similar ones as they exist in the private sector. In some detail, the staff chapters present aspects of governmental labor markets, wage relationships, provision for on-the-job training, merit programs, veterans' preference, and the shifting character of public-private occupational wage differentials.

The supply side is more fully treated, especially by the conference volume. The principal subject areas are public relationships and influences in the fields of health, education, and the development of basic knowledge in the sciences. Equal opportunities for employment and military manpower problems are discussed in only a single chapter, the former by the conference, the latter by the staff.

Manpower Policies for a Democratic Society contains five chapters which deal with the concept of manpower, the roles of the family, and the social context within which men live in molding and shaping a nation's labor resource, the principal problems and issues in this field, and lastly, with guidelines for appropriate policy. The Council notes that, while national concerns for labor resources are not new, only recently has the view that, except for a few crisis situations, labor resources will develop naturally been replaced by an acknowledgment that some more-formal planning is desirable.

The chapters on the manpower influences of the social context and the family constitute an excellent statement and illustration of the extent to which noneconomic and quasi-economic factors influence the development of our manpower resources both before and during their labor force years.

The last portion deals with policy. According to the Council the major

[1] The book was literally written by a Council member, Henry David, from a Council-prepared outline and with the extensive personal comments, especially at a conference devoted to it, of the other members.

issues and problems are (1) those associated with surplus labor; (2) the extent to which these surplus individuals result from and contribute to social problems like crime, poverty, and illness; and (3) the fact that the labor market is seldom in equilibrium.

The strengths and weaknesses of the books are largely determined by the reader's expectations. The principal beneficiaries, probably the audiences for which the first two books were written and the explicit potential readership of the third, are those who, for the first time, wish to give systematic consideration to the various roles which public authorities and others play in the field of manpower. Among these would be economic writers for newspapers, speakers at business gatherings, the League of Women Voters, authors of elementary economics texts, etc. The books, through their broad-guaged yet interrelated consideration of these issues, have real merit. Too, anyone contemplating research in the manpower field would probably benefit from a quick review of these books, especially the final statement volume. Lastly, the essay in the conference volume by Odin W. Anderson, "National Manpower Policy and the Health Field," is commended to everyone as excellent and is deserving of the widest possible audience.

Yet even for the groups listed above, and especially for the academic economist, there are serious shortcomings. The inadequacies stem from a lack of systematic structure in assessing the various problems and a failure to provide citations as a guide to alternative discussions of the issues. Illustrative is the discussion of wages and salaries. Data are presented to indicate that government-private wage ratios tend to be much closer to equality at lower occupational levels than at higher levels. It is then argued that the imbalance results in a severe problem in recruitment of personnel. Little attention, however, is given to the specific subquestions: how serious an employee shortage results, how much equality is needed, how might it be obtained, and which, if any, monographs listed in the bibliography deal with this problem. Another factor reducing the usefulness of two of the books is the length of time required to bring them into print. The conference was held in November, 1959, and the staff chapters appear to have been largely completed by early 1961. Consequently, proposals and insights which may have been original when offered appear quite traditional when they are read now. The final statement volume does not suffer from this, but rather is quite timely.

The books share some shortcomings with much of the other policy-oriented literature in economics. With the exception of a few brief comments by Wallace S. Sayre and Milton Katz, no attention is given to the questions of how policies are defined or the process by which they are initiated or implemented. The implications of specific policies are considered only as they relate to manpower. If the implications for manpower are favorable, the policy is considered desirable. Other ramifications are not discussed. For example, since better health leads to increased man-years of work available from a given population, health improvement measures are extolled. The fact that the extra years of life associated with better health also mean extra years when society must maintain retired individuals and perhaps incur more expensive terminal

medical expenses is not included in the analysis. It is not that the reviewer favors high death rates; it is just that many policies have both desirable and undesirable economic consequences and both should be considered if economics is used as the basis for judgment. Similarly with the recommendation that small school districts should be combined because this will lead to better-prepared students, no thought is given to the adverse social and economic consequences which such moves may inflict upon the nonstudent members of these communities. There is some recognition of this weakness in the final statement volume when it discusses manpower as "man." The view that it is man and not manpower that is precious in the sight of God is given some recognition in the final-statement volume, but like the boxer's handshake, it is not allowed to interfere with the main attraction.

In summary, the first two books are recommended to those interested in generalized, but fairly sophisticated, discussions of local, state, and federal governments' multitudinous interrelationships with the nation's manpower. They are not, however, for the individual interested in probing deeply into limited aspects of governments and manpower. The Council's final statement stands as a brief, excellently written introduction to some of the many facets of a complex world's interaction with man as an agent in economic output and growth.

ROBERT EVANS, JR.

Brandeis University

Men Without Work—The Economics of Employment. Edited by STANLEY LEBERGOTT. Modern Economic Issues Series. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1964. Pp. vi, 183. $4.95; paper, $1.95.

If you consider yourself a serious student of the controversial subject of recent high unemployment rates (or a beginner who wants to know what the argument is all about) and you haven't read most of the 12 essays in this collection, you should. In an essay which covers almost one-third of the volume, Stanley Lebergott provides his readers with a considerable amount of background material on the unemployment problem. For instance, there is a compact summary of the procedures used in compiling the Monthly Survey of the Labor Force, a defense of these procedures, a description of the age, educational, and geographical composition of the unemployed, an examination of the good and bad side of automation, a discussion of the structural unemployment versus deficient demand argument, and the selection of an unemployment target.

Other provocative essays include C. C. Killingsworth's analysis of the automation problem and his attack on the adequacy of the Administration's proposals for dealing with it, W. W. Heller's defense of the program, and Yale Brozen's emphasis on high wage rates and restrictive monetary policies in the late 'fifties as primarily responsible for high unemployment in the early 'sixties. Also of interest to this reader were E. M. Hoover's discussion of industrial structure and growth potential, Joseph Zeisel's profile of the unem-

ployed, and R. L. Stein's description of the work history and attitudes of the unemployed.

There is, in addition, a succinct description of programs here and abroad for dealing with specific aspects of the unemployment problem. For instance, there are summaries of the main objectives and provisions of the Manpower Training and Development Act and the Area Redevelopment Act. Finally, two short essays present information on European programs for stimulating employment. One deals specifically with the Swedish program. The other simply lists policies that have been adopted in other European countries.

The editor's shrewd selection of materials provides his readers with much of the flavor of the discussion in this controversial area. To be sure, one can easily find numerous points on which he may disagree with specific authors. That is to be expected. Nevertheless, the essays in this short volume also offer many stimulating insights into the problem of why there are men without work.

JOHN P. CULLITY

Rutgers—The State University

Population; Welfare Programs; Consumer Economics

Human Capital: A Theoretical and Empirical Analysis with Special Reference to Education. By GARY S. BECKER. New York: National Bureau of Economic Research, 1964. Pp. xvi, 187. $5.00.

Although the concept of human capital goes back to the earliest days of economics, its implications have only recently been developed. The leaders of this development have been T. W. Schultz, who has called attention to the central role of human capital formation in economic growth, and Gary Becker, who has provided the most sophisticated theoretical and empirical analysis of investment in people. In this book Becker brings together and expands his earlier work.

Part I, which contains the theoretical work, develops a mathematical model of on-the-job training and extends it to schooling and other forms of investment in people. The implications for rates of return on such investment and for wage differentials are then worked out. An important result is that the supply of skilled workers depends on the absolute real difference between the wages of the skilled and the unskilled, while the demand for such workers depends on the ratio of these wages. This leads to the conclusion that in an economy whose wage levels are rising, absolute skill differentials will widen through time, and relative differentials will narrow.

Another basic concept developed in Part I is the distinction between on-the-job training useful only to the employer who provides it (specific training) and that useful elsewhere (general training). Becker shows that the trainee will pay for general training through lower wages during the training period, while the employer will pay for specific training and will adopt policies to reduce the turnover of specifically trained workers in order to raise the return on his investment in them.

Part II contains the empirical work, which concentrates on the rate of return to college education. The private rate of return to college graduates is calculated as the internal rate, or rate of discount, that makes the series of differences in earnings between college and high school graduates sum to zero, where the costs of college (including foregone earnings) give rise to negative differences. This rate is estimated at 14.5 per cent for urban native white males who graduated from college in 1939 (p. 77) and 13 per cent for all white males who graduated in 1949 (p. 78). The estimated returns are lower for nonwhite males; about 12 per cent for 1939 graduates in the urban South and about 8 per cent for the corresponding group in the North (p. 94).

An interesting feature of this analysis is the correction for the differential ability of college graduates, which would have increased their earnings even if they had not gone to college. Using several independent pieces of evidence on ability by level of education, Becker concludes that the estimated rates of returns for white males cited above should be reduced by only two percentage points or less on this account.

The social rate of return on college education, which includes public expenditures in the costs and tax receipts from additional earnings in the returns, is estimated as only slightly below the private rate. Becker recognizes, of course, that this calculation does not fully reflect the social value of education. Other chapters in Part II study in less detail rates of return on high school education, trends in rates of return through time, and profiles of earnings and human wealth by age for different education levels. The estimated rate of return to high school education is above that to college before adjustment for ability, but Becker indicates that a larger adjustment would be needed on this account for high school graduates, which could equalize the adjusted rates.

All of the work is done with evident care and anticipates many of the questions that might be raised about it. But the growth of knowledge inevitably leads to the discovery of new problems. One of these concerns the higher apparent rates of return to the last years of college than to the first, as shown by a comparison of rates of return to graduates and to drop-outs. Becker attributes much of the bonus for graduation to differential ability, but feels that the rest may indicate some "increasing returns" to the third, fourth, and later years of college (p. 94).

This conclusion runs counter to Becker's convincing argument elsewhere (p. 113) that the variability of returns among individuals and the long payoff period work together to raise the value of education that is useful in a variety of environments and to lower the value of highly specialized education. The former kind of education (liberal or general education) tends to be concentrated in the first two years of college.

Perhaps a clue to the puzzle lies in employer discrimination in favor of high school and college graduates over high school and college drop-outs. A policy of hiring only graduates reduces screening costs when there are many applicants for openings, selects workers who have demonstrated persistence, and provides a rational and defensible decision rule in a society that increasingly condemns nepotism and racial discrimination as guides to hiring. In many cases, the preference for graduates is justified by the nature of the

work, but it is often extended beyond these cases, leading to the characterization of the degree as a "union card." Even where the preference is justified on the average, rules against hiring nongraduates may express it too strongly. Suppose that among pairs of applicants otherwise seemingly to be equal, the graduate would be a better worker than the nongraduate six times out of ten. An employer lacking other information would always hire the graduate, so that the nongraduate would experience longer periods of unemployment and receive lower wages while working. The differences in rates of return to the two groups would be larger than justified by their productivities, for although the inferior graduate might eventually fall behind and the superior nongraduate might pull ahead, the earlier gains and losses would often not be fully compensated.

This argument does not affect the accuracy of Becker's estimates of private rates of return, but it does affect his estimated social rates. Since his income data are for 1939 and 1949, years in which employment was respectively far below and somewhat below full employment levels, his social rates may be biased upward and lie too close to the private rates.

One of the advantages of full employment is that it forces employers to abandon hiring standards expressed by rules of thumb and to invest more in getting information that will enable them to select satisfactory workers from nongraduates and other groups excluded by such rules. One might even suggest in principle, though hardly in practice, a definition of full employment as a state of the labor market that eliminates increasing private returns to the last years of an educational program (including the Ph.D. in economics)! Note that this does not state that the graduate ought not to receive higher income, but only that his gains should not be disproportionate to his added costs.

The careful reader will discover many other equally interesting issues raised by Becker's analysis. Indeed, the book should have only careful readers, for it is tightly written, and important points can easily be overlooked. Although this work is very different from much of the traditional work of labor economics, it will have an important place in the literature of the field for many years to come.

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Priority Patterns and the Demand for Household Durable Goods. By F. Graham Pyatt. New York: Cambridge University Press, 1964. Pp. xv, 147. $8.00.

Consumption Patterns in Israel. By Nissan Liviatan. Jerusalem: Falk Project for Economic Research in Israel, 1964. Pp. x, 88.

The Pyatt and Liviatan studies represent the current expanding interest in consumer behavior and the attempt to refine its analysis by the introduction of new techniques and the consideration of a wide range of explanatory variables. In addition, they are indicative of the growing concern of individual countries with household consumption. Both books are based on doctoral dis-

sertations, use empirical cross-section studies in their respective countries, Great Britain and Israel, in combining empirical and theoretical analysis. Nissan Liviatan confines himself to the careful use of the well-established Prais-Houthakker techniques in his analysis of consumption patterns in Israel, the results of which are presented in detail. F. Graham Pyatt, however, spends the major part of his book (Chapters 2-10) on the mathematical development of a probabilistic system which is used to explain the accumulation of durable goods by households and forecast their future acquisition. He deals with both static and dynamic models. His study is theoretically more ambitious than that of Liviatan and more limited in the sense that it applies to only a part of consumer expenditures, consumer durable goods.[1]

The most interesting part of Liviatan's analysis of the Family Expenditures Survey, 1956-57, is his attempt to isolate the effect of the consumers' country of origin upon consumption patterns—a factor of peculiar importance to Israel in view of the large immigration of families from Asia, Africa, and Europe. The immigrants were classified by country of origin and subdivided into newcomers and veterans (those who immigrated before the Israeli State was established). Both food and nonfood items were analyzed in detail after eliminating the effects of income, family size and structure, and occupation, upon consumption elasticities and consumption levels. Significant differences in consumption levels were found, such as the relatively low expenditure of Asian immigrants on animal proteins and their relatively high expenditure on bread, cereals, and vegetables as compared to European immigrants. Similarly, on nonfood items, those of European origin spent more on education, literary, and health items and less on clothing, footwear, and tobacco. In examining the differences between veterans and newcomers, there appeared a tendency for Asian immigrants to approach European consumption standards the longer they stayed in Israel.

Pyatt's probabilistic model[2] is based on the premise that classical consumer preference theory is inadequate to explain the "saving up" and "stockholdings" which characterize household behavior in acquiring durable goods. He is not concerned with consumer choice itself, but with the fact that an individual household acquires durable goods in some order. Its priority pattern for future purchases is, then, determined by its current stock of durables. This is the household's preference statement, given its social and economic circumstances, and it must then decide what durable good to purchase next, and when. This determines its rate of accumulation over time. The mathematical framework deals with the solution of the system for individual and aggregates, given individual household priority patterns, and determining its velocity of accumulation of additional durables under various assumptions. Applying this mathematical system to cross-section surveys of household ownership

[1] In comparing his model with the Stone and Rowe one (pp. 134-36), Pyatt points out that both models may be adapted to deal with "perishable" goods by certain assumptions.

[2] I shall leave a mathematical critique of Pyatt's complicated model to those more accomplished in this area.

of durable goods and intentions to purchase additional durables makes it possible to forecast the purchases of durables most likely to be made by households with certain ownership patterns, and at what rate. The application of the model to time-series data is also discussed in Chapter 13.

Using the British Market Research Bureau Survey of 1958 Pyatt comes out with results such as the following: In group I (Cooker, Electric Vacuum Cleaner, Electric Washing Machine, and Refrigerator) cookers are almost always purchased first, vacuum cleaners second, and washing machines third. Refrigerators occupy a special position—22 per cent of households buy them third, but 76 per cent do not buy one at all until they own the other three durables. Groups of other durables are subjected to a similar analysis which enables one to predict the probable future pattern of purchases of durables for the group in question. Refrigerators apparently have a low preference rating in Great Britain!

It is not possible here to cover all the detailed empirical results of the two authors, nor, in Pyatt's case, the many theoretical ramifications of his probabilistic system. In both cases, the empirical results make sense and provide interesting contributions to the literature on household behavior. Liviatan's study of consumption patterns in Israel is, indeed, a model of its kind in the field. It should be very useful to the Israeli government. His results might be more easily absorbed by the reader if his conclusions had been presented separately from the mass of statistical data and analyses. As it is, one has to dig for them. Pyatt's exposition is admirably clear, but I wonder if such a highly complicated mathematical system is essential to the analysis of the stock-flow aspects of durable goods.

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In Aid of the Unemployed. Edited by Joseph M. Becker, S.J. Baltimore: The Johns Hopkins Press, 1965. Pp. xiii, 317. $8.95.

Father Joseph Becker, Associate Director of the Institute of Social Order at St. Louis University, has assembled a collection of essays by specialists which examine the different ways in which our society aids the unemployed worker. The subject is particularly timely in view of the present concentration of federal programs which are aimed at the alleviation of unemployment.

Much legislation concerning unemployment has been passed during the Kennedy-Johnson Administrations. The Area Redevelopment Act was passed in 1961 and was followed by the Accelerated Public Works and Manpower Development and Training Acts in 1962. The Economic Opportunity Act was passed in 1964 and the Appalachian Regional Development Act in 1965. The best features of the area redevelopment and accelerated public works programs are currently being incorporated into a new act called the Public Works and Economic Development Act. This act will establish an agency for public works and economic development, within the Department of Commerce, to provide financial assistance to areas with substantial unemployment. Among the new features of this act are: (1) the guarantee of working

capital loans made to private borrowers by private lenders in connection with loan projects; (2) the provision of 2 percentage point interest subsidies to companies which obtain their financing from private sources and locate within redevelopment areas; and (3) the designation of multicounty economic development districts.

The book is divided into four parts. Part I sets forth the over-all history of aid to the unemployed in the United States. Shifts in attitudes concerning the unemployed and the role of the federal government in providing for the unemployed are examined during several time periods.

Part II involves a description of the unemployed. The first essay, by the late Richard Wilcock, describes the socioeconomic characteristics of the unemployed, and a second, by Walter Francke, describes the characteristics of a problem group within the unemployed—the long-term unemployed. The information developed is of relevance to the development of specific aid programs for this group.

Part III consists of essays devoted to an analysis of private and public programs of aid to the unemployed. The coverage and financing of unemployment insurance is discussed in one essay. Two other essays involve an analysis of such private aid programs as supplementary unemployment benefits and severance pay and early retirement plans. An essay by William Miernyk on area redevelopment explores state and local development programs, examines the area redevelopment programs of Great Britain, and describes and evaluates the programs created under the Area Redevelopment Act. The potential limitations of public works and work relief are explored rather effectively in an essay which was contributed by Roger Freemen. Job training programs—public and private—are examined in an essay by Gerald Sommers, and welfare services, including surplus food distribution programs, child day care, and vocational preparation, counseling, and placement, are discussed by Elizabeth Wickenden.

In Part IV, two essays develop policy guides for aid to the unemployed. The first essay, by Eveline Burns, points out the necessity of relating the appropriate aid program to the recognized needs of the unemployed. The second essay, by Joseph Becker, William Haber, and Sar Levitan, offers policy recommendations to implement the current programs of aid to the unemployed. One recommendation involves an extension of unemployment insurance coverage to smaller firms, most nonprofit institutions, and government employees. Two aids which are used in other countries—accelerated tax amortization and relocation allowances to stimulate the mobility of workers from depressed areas—are also recommended. Other recommendations include (1) the modification of current public works programs to incorporate more jobs for unskilled workers; (2) an increase in the number of severance-pay plans; (3) an expansion of the scope and an increase in the efficiency of the present employment system; and (4) the expansion of governmental financial support for job retraining.

Becker's book makes a contribution to the literature on the subject of unemployment. The essays are competently handled by the specialists involved.

With a major emphasis of the Johnson domestic program placed on poverty, Appalachian development, and other aid programs to the unemployed, the book comes at a very propitious time.

However, with the passage of time, it will be necessary to examine the effectiveness of these aid programs. Has the accelerated public works program had any significant effect on unemployment? Will poverty be reduced by the anti-poverty programs? It will be interesting to find out the answers to these and similar questions.

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NOTES

At the March 12-13, 1965, meeting of the Executive Committee it was decided to add the *Journal of Economic Abstracts* to the publications that members and subscribers receive. In order to cover the cost of the *Journal of Economic Abstracts,* it was voted to adopt the following rate schedule, effective as of January 1, 1966: annual dues, $10.00; family membership, $1.00; junior (student) membership, $5.00; subscribing membership, $12.00; contributing membership, $25.00 or more; life membership, $200.00; subscription to the *American Economic Review* and the *Journal of Economic Abstracts,* $10.00.

1965 Candidates for Office
American Economic Association

President-Elect:

Milton Friedman

Vice Presidents

Dorothy Brady, University of Pennsylvania
Charles P. Kindleberger, Massachusetts Institute of Technology
Tjalling C. Koopmans, Yale University
Guy H. Orcutt, University of Wisconsin

Executive Committee

James M. Buchanan, University of Virginia
Earl O. Heady, Iowa State University
Henry Houthakker, Harvard University
Lawrence R. Klein, University of Pennsylvania

SEVENTY-EIGHTH ANNUAL MEETING OF THE AMERICAN ECONOMIC ASSOCIATION

New York Hilton Hotel, New York, New York—December 28-30, 1965

Preliminary Announcement of the Program

The program is designed around a central theme: "Knowledge Production and Innovation." This theme is developed in the following 15 sessions:

Allocation and Distribution Theory: Technological Innovation and Progress
Capital Theory: Technical Progress, Capital Structure, and Growth
Economic Development: Advanced Technology for Poor Countries
Knowledge, Information, and Innovation in the Soviet Economy
Money and Banking: Innovations in Finance
Public Finance: Promotion of Knowledge Production and Innovation
International Economics: Progress and Transfer of Technical Knowledge
Antitrust and Patent Laws: Effects on Innovation
Public Regulation: The Impact of Changing Technology
The Economics of Education
The Economics of Publishing
The Economics of Broadcasting and Advertising
The Economics of Science Policy
The Production and Use of Economic Knowledge
Labor Economics: Effects of More Knowledge

The Richard T. Ely Lecture will be given by Kenneth E. Boulding on "The Economics of Knowledge and the Knowledge of Economics."

Five allied associations have kindly agreed to have jointly sponsored sessions oriented toward the same general theme:

Agricultural Economics: Innovations in Agriculture (AFEA)
Business Finance: Innovations in Analysis (AFA)
Industrial Relations: Adjustments to Technological Change (IRRA)
Econometric Models for Educational Planning (ES)
Innovational Impacts on American Capitalism (A for Compar. E)

A round table discussion arranged by the NSF remains within the framework. Only two sessions, the Invited Doctoral Dissertations and the Graduate Students' Papers, depart from the theme.

Tuesday, December 28, 1965

8:30 A.M. AGRICULTURAL ECONOMICS: INNOVATIONS IN AGRICULTURE (Sponsored jointly by the American Economic Association and the American Farm Economic Association)

Chairman: R. J. HILDRETH, Farm Foundation
Papers: Innovations, Technological Change, and Production
EARL O. HEADY, Iowa State University
Income Effects of Innovation
T. DUDLEY WALLACE, North Carolina State University
DALE HOOVER, North Carolina State University
Discussion: GLEN T. BARTON, U.S. Department of Agriculture
BURL BACK, U.S. Department of Agriculture

8:30 A.M. BUSINESS FINANCE: INNOVATIONS IN ANALYSIS (Sponsored jointly by the American Economic Association and the American Finance Association)

Chairman: MYRON J. GORDON, University of Rochester
Papers: Alternative Lower-Level Criteria for Investment Decisions
WILLIAM W. ALBERTS, University of Chicago
Financing Decisions of the Firm
EUGENE M. LERNER, New York University
WILLARD T. CARLETON, New York University
Valuation of the Firm
ALEXANDER A. ROBICHEK, Stanford University
STEWART C. MYERS, Stanford University
Decision Theory and Financial Management
HENRY A. LATANÉ, University of North Carolina
Discussion: JOHN BOSSONS, Carnegie Institute of Technology
WILLIAM F. SHARPE, University of Washington
JACOB MICHAELSON, University of California, Berkeley
WILLIAM BERANEK, University of Wisconsin

10:30 A.M. ALLOCATION AND DISTRIBUTION THEORY: TECHNICAL INNOVATION AND PROGRESS

Chairman: SIDNEY WEINTRAUB, University of Pennsylvania
Papers: Change and Innovation in the Technology of Consumption
KELVIN J. LANCASTER, The Johns Hopkins University
Profit Maximization, Utility Maximization, and the Rate and Direction of Innovation
WILLIAM J. FELLNER, Yale University
The Role of Technological Progress in the Theories of Income Distribution
BERNARD A. CORRY, London School of Economics and University of California, Berkeley

Discussion: HANS J. BREMS, University of Illinois
JOHN S. CHIPMAN, University of Minnesota
TIBOR SCITOVSKY, University of California, Berkeley

10:30 A.M. PUBLIC REGULATION: THE IMPACT OF CHANGING TECHNOLOGY

Chairman: JAMES R. NELSON, Amherst College
Papers: Community Antenna Television Systems and the Regulation of Television Broadcasting
FRANKLIN M. FISHER, Massachusetts Institute of Technology
Regulation and Technological Destiny: The National Power Survey
WILLIAM R. HUGHES, Wesleyan University
New Technology and the Old Regulation in Radio Spectrum Management
HARVEY J. LEVIN, Hofstra University
Discussion: BEN W. LEWIS, Oberlin College
GEORGE WILSON, Indiana University
WILLIAM IULO, Washington State University

2:30 P.M. MONEY AND BANKING: INNOVATIONS IN FINANCE

Chairman: LESTER V. CHANDLER, Princeton University
Papers: Effects of Automation on the Structure and Functioning of Banking
GEORGE W. MITCHELL, Member, Board of Governors, Federal Reserve System
Recent Innovations in the Functions of Banks
A. JAMES MEIGS, First National City Bank of New York
Innovations in Interest-Rate Policy
FRANCO MODIGLIANI, Massachusetts Institute of Technology
Discussion: WARREN L. SMITH, University of Michigan
A. R. KOCH, Member, Board of Governors, Federal Reserve System
BURTON G. MALKIEL, Princeton University

2:30 P.M. THE ECONOMICS OF BROADCASTING AND ADVERTISING

Chairman: SIDNEY S. ALEXANDER, Massachusetts Institute of Technology
Papers: The Economics of Broadcasting and Government Policy
R. H. COASE, University of Chicago
The Quest for Quantity and Diversity in Television Programming
DAVID M. BLANK, Columbia Broadcasting System
Supply and Demand for Advertising Messages
LESTER G. TELSER, University of Chicago
Discussion: HAROLD J. BARNETT, Washington University, St. Louis
HYMAN H. GOLDIN, Federal Communications Commission
PETER O. STEINER, University of Wisconsin

2:30 P.M. THE ECONOMICS OF SCIENCE POLICY

Chairman: C. J. HITCH, University of California, Berkeley
Papers: National Science Policy: Issues and Problems
EDWIN MANSFIELD, University of Pennslyvania
Economic Analysis, Science Policy, and the Defense Establishment
CARL KAYSEN, Harvard University
The Allocation of Scientific Effort Between Teaching and Research

MICHAEL D. INTRILIGATOR, University of California, Los Angeles
BRUCE L. R. SMITH, Rand Corporation
Discussion: WILLIAM CAPRON, Bureau of the Budget
MERTON J. PECK, Yale University
JEROME B. WIESNER, Massachusetts Institute of Technology

4:30 P.M. ROUND TABLE ON STUDY PROBLEMS IN THE ECONOMICS OF RESEARCH AND DEVELOPMENT (Arranged by the Office of Economic and Manpower Studies of the National Science Foundation)

Chairman: ZOLA BRONSON, National Science Foundation
Panel: EDWIN MANSFIELD, University of Pennsylvania
FREDERIC M. SCHERER, Princeton University
IRVING H. SIEGEL, Research Analysis Corporation
PAUL J. STURM, U.S. Department of Defense
GEORGE W. WRIGHT, National Aeronautics and Space Administration

8:00 P.M. RICHARD T. ELY LECTURE

Chairman: BERNARD F. HALEY, University of California, Santa Cruz
Invited Lecture: The Economics of Knowledge and the Knowledge of Economics
KENNETH E. BOULDING, University of Michigan

Wednesday, December 29, 1965

8:30 A.M. INDUSTRIAL RELATIONS: ADJUSTMENTS TO TECHNOLOGICAL CHANGE (Sponsored jointly by the American Economic Association and the Industrial Relations Research Association)

Chairman: GARTH L. MANGUM, National Commission on Technology, Automation, and Economic Programs
Papers: Airline Flight Crews: Adjustment to Technological Change in a Regulated Growth Industry
MARK L. KAHN, Wayne State University
Technology and the Labor Force in the Maritime Industry
AARON W. WARNER, Columbia University
Discussion: To be announced

8:30 A.M. INNOVATIONAL IMPACTS ON AMERICAN CAPITALISM (Sponsored jointly by the American Economic Association and the Association for Comparative Economics)

Chairman: ARTHUR SCHWEITZER, Indiana University
Papers: Fiscal Policy Innovations: In Relation to Economic Growth
GERHARD COLM, National Planning Association
Fiscal Policy Innovations: In Relation to Cyclical Fluctuation
ALBERT G. HART, Columbia University
Discussion: WALTER P. EGLE, University of Cincinnati
PAUL W. MCCRACKEN, University of Michigan

8:30 A.M. THE ECONOMICS OF EDUCATION

Chairman: THEODORE W. SCHULTZ, University of Chicago
Papers: Education and the Personal Distribution of Income
GARY S. BECKER, Columbia University
BARRY CHISWICK, Columbia University
Investment in the Education of the Poor: A Pessimistic Report
EUGENE SMOLENSKY, University of Chicago
Measurement of the Quality of Schooling

FINIS WELCH, University of Chicago
Discussion: LEE R. MARTIN, University of Michigan
ALICE M. RIVLIN, The Brookings Institution
ANDRE DANIERE, Harvard University

10:30 A.M. CAPITAL THEORY: TECHNICAL PROGRESS AND CAPITAL STRUCTURE

Chairman: ROBERT M. SOLOW, Massachusetts Institute of Technology
Papers: Sources of Measured Productivity Change
ZVI GRILICHES, University of Chicago
DALE JORGENSON, University of California, Berkeley
Inventive Activity and Capital Accumulation
KARL SHELL, Massachusetts Institute of Technology
Investment in Humans, Technological Diffusion, and Economic Growth
EDMUND S. PHELPS, Yale University
RICHARD R. NELSON, Rand Corporation
Discussion: EDWARD F. DENISON, The Brookings Institution
KAZUO SATO, United Nations
JACK HIRSHLEIFER, University of California, Los Angeles

10:30 A.M. KNOWLEDGE, INFORMATION, AND INNOVATION IN THE SOVIET ECONOMY

Chairman: ALEXANDER GERSCHENKRON, Harvard University
Papers: Information and Innovation in the Soviet Economy
GREGORY GROSSMAN, University of California, Berkeley
The Role of Information in Soviet Economic Reforms: Types, Amount, Cost, and Quality
EGON NEUBERGER, Rand Corporation, University of Michigan
The Environment for Technological Change in Soviet Agriculture
NANCY NIMITZ, Rand Corporation
Discussion: ALEXANDER ECKSTEIN, University of Michigan
JOSEPH S. BERLINER, Brandeis University
D. GALE JOHNSON, University of Chicago

2:30 P.M. PUBLIC FINANCE: PROMOTION OF KNOWLEDGE PRODUCTION AND INNOVATION

Chairman: JAMES M. BUCHANAN, University of Virginia
Papers: The Tax Treatment of Individual Expenditures for Research and Education
RICHARD B. GOODE, International Monetary Fund
The Tax Treatment of Research and Innovative Investment
RICHARD E. SLITOR, U.S. Treasury Department
The Efficient Achievement of Technological Breakthroughs: A Major Problem in Public Finance
RICHARD R. NELSON, Rand Corporation
Discussion: D. H. ELDRIDGE, National Bureau of Economic Research
MARTIN J. BAILEY, Institute for Defense Analyses
WILLIAM CRAIG STUBBLEBINE, University of Delaware, Massachusetts Institute of Technology

2:30 P.M. LABOR ECONOMICS: EFFECTS OF MORE KNOWLEDGE

Chairman: MARGARET S. GORDON, University of California, Berkeley
Papers: Information Networks in Labor Markets
ALBERT E. REES, University of Chicago
The Effect of Education on Labor-Force Participation
WILLIAM G. BOWEN, Princeton University
T. ALDRICH FINEGAN, Vanderbilt University

Changing Distribution of the Labor Force
JACOB MINCER, Columbia University
Discussion: W. LEE HANSEN, University of Wisconsin
ROBERT EVANS, JR., Brandeis University
JOSEPH SHISTER, State University of New York, Buffalo

4:00 P.M. INVITED DOCTORAL DISSERTATIONS

Chairman: GEORGE LELAND BACH, Carnegie Institute of Technology
Papers: A Dynamic Programming Approach to Demand for Money and Portfolio Selection
VIKASCHANDRA S. CHITRE, University of Toronto, (Ph.D., University of Rochester)
The Analysis and Testing of the Asymptotic Behavior of Aggregate Growth Models
JOHN CONLISK, Rice University, (Ph.D., Stanford University)
An Analysis of the Fiscal Impact of the Federal Budget in the United States
EDWARD M. GRAMLICH, Member, Board of Governors, Federal Research System (Ph.D., Yale University)
Discussion: ALLAN H. MELTZER, Carnegie Institute of Technology
ROBERT SOLOW, Massachusetts Institute of Technology
RICHARD MUSGRAVE, Harvard University

8:00 P.M. PRESIDENTIAL ADDRESS

Chairman: CALVIN B. HOOVER, Duke University
Presidential Address: The Economist and the "Population Problem"
JOSEPH J. SPENGLER, Duke University

Thursday, December 30, 1965

8:30 A.M. ECONOMETRIC MODELS FOR EDUCATIONAL PLANNING (Sponsored jointly by the American Economic Association and the Econometric Society)

Chairman: To be announced
Papers: A Planning Model for the Efficient Allocation of Resources in Education
SAMUEL BOWLES, Harvard University
A Linear Programming Model for Educational Planning in an Underdeveloped Economy: A Case Study of Argentina
IRMA ADELMAN, The Johns Hopkins University
Discussion: JOHN FEI, Cornell University
JAMES HENDERSON, University of Minnesota

8:30 A.M. GRADUATE STUDENTS' PAPERS (Arranged by *The American Economist* for Omicron Delta Epsilon, Honor Society in Economics)

Chairman: SOLOMON FABRICANT, New York University
Papers: A Generalization of Hicksian Analysis
NORMAN C. MILLER, University of Pittsburgh
A Cross-section Analysis of the Demand for Basic Living Materials in Japan
FENG-YAO LEE, Michigan State University
Fabricant's Determinants After Twenty Years: A Critical Reappraisal
ROY W. BAHL, JR., University of Kentucky
ROBERT J. SAUNDERS, University of Kentucky
Discussion: CHARLES J. SIEGMAN, Swarthmore College
HARLAND W. WHITMORE, Michigan State University
JOHN D. GUILFOIL, New York University

8:30 A.M. ECONOMIC DEVELOPMENT: ADVANCED TECHNOLOGY FOR POOR COUNTRIES

Chairman: EDWARD S. MASON, Harvard University
Papers: Transport Technologies for Developing Countries
JOHN R. MEYER, Harvard University
The Capacity to Assimilate Advanced Technology
ROBERT SOLO, National Research Council, National Academy of Sciences
Invention and Innovation in Less Developed Countries
RICHARD S. ECKAUS, Massachusetts Institute of Technology
Discussion: BAREND A. DE VRIES, International Bank for Reconstruction and Development
LLOYD REYNOLDS, Yale University
WOLFGANG F. STOLPER, University of Michigan

10:30 A.M. INTERNATIONAL ECONOMICS: PROGRESS AND TRANSFER OF TECHNICAL KNOWLEDGE

Chairman: GOTTFRIED HABERLER, Harvard University
Papers: Labor Skills and Comparative Advantage
DONALD B. KEESING, Columbia University
The Transmission of Technical Change by International Corporations
JACK BARANSON, Indiana University
International Flows of Human Capital
HERBERT GRUBEL, University of Chicago
A. D. SCOTT, University of British Columbia
Discussion: STEPHEN HYMER, Yale University
BURTON A. WEISBROD, University of Wisconsin
HARRY G. JOHNSON, University of Chicago

10:30 A.M. THE ECONOMICS OF PUBLISHING

Chairman: MILTON FRIEDMAN, University of Chicago
Papers: The Market for Academic and Professional Writing
ALEXANDER J. MORIN, President, Aldine Publishing Company
The Pricing of Textbooks
PAUL M. HORVITZ, Office of the Comptroller of the Currency
The Economic Rationale of Copyrights
ROBERT M. HURT, Princeton University (Completed and edited by ROBERT M. SCHUCHMAN, University of Chicago)
Discussion: REUBEN E. SLESINGER, University of Pittsburgh
ROBERT W. FRASE, American Book Publishers Council
ARMEN A. ALCHIAN, University of California, Los Angeles

2:30 P.M. ANTITRUST AND PATENT LAWS: EFFECTS ON INNOVATION

Chairman: CLAIR WILCOX, Swarthmore College
Papers: Anniversaries of the Patent and Sherman Acts: Competitive Policies and Limited Monopolies
MARK S. MASSEL, The Brookings Institution
The Joint Effect of Antitrust and Patent Laws Upon Innovation
JESSE W. MARKHAM, Princeton and Harvard Universities
The Effects of Patents and Other Barriers to Entry on Innovation
ALMARIN PHILLIPS, University of Pennsylvania
Discussion: CORWIN D. EDWARDS, University of Oregon
ALFRED E. KAHN, Cornell University
RICHARD B. HEFLEBOWER, Northwestern University

2:30 P.M. THE PRODUCTION AND USE OF ECONOMIC KNOWLEDGE

Chairman: SIMON KUZNETS, Harvard University

Papers: Economic Research Sponsored by Private Foundations
RALPH L. NELSON, Queen's College, City University of New York
The Production and Use of Economic Knowledge
ROBERT D. CALKINS, The Brookings Institution
Trends, Cycles, and Fads in Economic Writing
MARTIN BRONFENBRENNER, Carnegie Institute of Technology

Discussion: HENRY H. VILLARD, The City College, New York
JACOB L. MOSAK, United Nations
SOLOMON FABRICANT, New York University

4:30 P.M. BUSINESS MEETING

Announcements

Arrangements have been made for group flights to the New York City meeting, December 28-30, 1965, from Chicago, San Francisco, Los Angeles, and St. Louis. These flights are scheduled to arrive in New York City the afternoon of December 27 and to depart late afternoon of December 30. Potential savings amounting to 20 per cent of round trip air fare are possible by traveling as a group of 25 or more (members and their families). Because of the difficulty in obtaining air transportation during the height of the tourist season, 15 round trip reservations have also been made at the normal tourist rate for flights between Miami and New York, leaving Miami the afternoon of December 27 and New York City late afternoon of December 30. Inquiries about all flights should be directed to the Drake Travel Service, 1316 Chicago Avenue, Evanston, Illinois, 60201.

The American Universities Research Program of the Agricultural Development Council again is inviting faculty members of American universities to submit applications for grants to conduct research on the economic and human problems of international agricultural development. This program, which is to run for five years, was launched in 1963 to stimulate American professors to undertake research abroad and at the same time to help accelerate agricultural development in the developing nations. The field work for most of the projects is conducted in Asia, Africa, and Latin America. Correspondence relating to the program and applications should be directed to Dr. Clifton R. Wharton, Jr., American Universities Research Program, The Agricultural Development Council, Inc., 630 Fifth Avenue, New York, N.Y. 10020.

The Asia Foundation has made it possible for the Association to make a limited number of travel grants to individuals from certain Asian countries who (1) are enrolled at an educational institution in the United States, (2) plan to return to their native country after completing their education, and (3) are recommended by a member of the faculty of the institution at which they are enrolled. Application should be made to Harold F. Williamson, Secretary, American Economic Association, 629 Noyes Street, Evanston, Ill. 60201.

Deaths

Matthew M. Fryde, lecturer, department of economics, Columbia University, March 15, 1965.

Solomon S. Huebner, July 17, 1964.

Earl E. Muntz, March 26, 1965.

Marcus Nadler, April 24, 1965.

Max H. North, May 7, 1965.

E. J. Spomer, Ft. Hays Kansas State College, April 26, 1965.

Rodman Sullivan, April 5, 1965.

Martin W. Wilmington, May 1964.

Edwin B. Wilson, December 28, 1964.

Charles M. Winston, October 12, 1964.

Retirements

Frederick A. Bradford, professor and head of the department of finance, Lehigh University.

Howard S. Ellis, University of California, Berkeley, June 1965.

Promotions

Gene S. Booker: professor of management, Western Michigan University.

John W. Budina, Jr.: assistant professor of business and economics, McKendree College.

Willard Carleton: associate professor of finance, Graduate School of Business Administration, New York University.

William J. Carroll: associate professor of business administration, Rutgers—The State University.

Jon Cunnyngham: associate professor of economics, Columbia University.

Harold Dilbeck: associate professor of finance, University of Southern California.

Holger Engberg: associate professor of finance, Graduate School of Business Administration, New York University.

Donald Grunewald: associate professor of business administration, Rutgers—The State University.

James Heilbrun: assistant professor of economics, Columbia University.

Edward Kuriger: assistant professor of economics, Geneva College.

Eugene Lerner: professor of finance, Graduate School of Business Administration, New York University.

Henry S. McDonald, Jr.: assistant professor of economics, University of Georgia.

Edward L. Rada: associate professor of economics, University of California, Los Angeles.

Richard Scheuch: professor of economics, Trinity College.

Jack W. Skeels: professor of economics, Northern Illinois University.

George Sternlieb: professor of business administration, Rutgers—The State University.

Edwin R. Westcott: associate research specialist, department of agricultural economics and marketing, Rutgers—The State University.

Jean Wilburn: assistant professor of economics, Barnard College, Columbia University.

Administrative Appointments

Jack F. Bennett: manager, general economics department, Standard Oil Company of New Jersey.

Robert L. Bishop: dean, School of Humanities and Social Science, Massachusetts Institute of Technology.

Walter P. Blass: public relations supervisor—economic analysis, American Telephone and Telegraph, May 1, 1965.

E. Cary Brown: head, department of economics, Massachusetts Institute of Technology.

Walter Buckingham, Georgia Institute of Technology: chairman, department of economics, Drexel Institute of Technology.

Rockwood Chin: chairman, department of economics, Wheaten College.

Sherrill Cleland: dean of academic affairs, Kalamazoo College.

Barend A. de Vries: deputy director, economics department, International Bank for Reconstruction and Development.

Byron F. Doenges: dean-elect, College of Liberal Arts, professor of economics, Willamette University, September 1, 1965.

Guillermo Franco-Camacho, Universidad de los Andes: director, Foreign Trade Division, Colombian Planning Office.

William L. Henderson: acting chairman, department of economics, associate professor, Denison University.

Richard J. Jacob: executive vice president, Dayce Corporation.

Milton G. Johnson: chief economist, Coast and Geodetic Survey.

Vladimir Katkoff: professor and acting chairman, economics department, West Virginia Wesleyan College.

Jacob J. Kaufman: director, Institute for Research on Human Resources, Pennsylvania State University.

Charles L. Leven: director, Institute for Urban and Regional Studies, Washington University.

Emily P. Mackall: chairman, department of economics, Youngstown University.

Frank Pievia: director of economic research, W. B. Saunders & Company.

Morton J. Plotnik: chairman, department of economics, Franklin College.

Roy A. Ruffner: chairman, department of economics and business administration, Westminster College.

Philip S. Thomas: chairman, department of economics and business administration, Kalamazoo College.

Appointments

John P. Barrados, University of Saskatchewan: assistant professor of economics, University of Georgia.

John A. Bergeron: associate professor of economics, Northeastern University.

John G. Boyd, State University College at Buffalo: international economist, United States Tariff Commission, Washington, D.C.

Benjamin Bridges: economist, Division of Research and Statistics, Security Administration, Washington, D.C.

Karl Brunner: visiting professor, department of economics, Northwestern University, spring quarter.

Edwin Burmeister: assistant professor of economics, Wharton School, University of Pennsylvania, July 1.

David Conrath: assistant professor of industry, Wharton School, University of Pennsylvania, July 1.

J. D. DeForest, Center for the Study of Higher Education, University of Michigan: professor of economics, Parsons College.

James J. Diamond: visiting associate professor, department of economics, Northwestern University.

Howard S. Ellis: chief of party, AID mission to Brazil, University of California.

Edwin J. Elton: assistant professor of finance, Graduate School of Business Administration, New York University.

James C. Emery: assistant professor of industry and of statistics and operations research, Wharton School, University of Pennsylvania.

Willy J. Feuerlein: professor of economics, College of Social Science, Florida Atlantic University.

Linda P. Fletcher: assistant professor of insurance, Wharton School, University of Pennsylvania, July 1.

Ronald E. Frank: associate professor of marketing, Wharton School, University of Pennsylvania, July 1.

Robert W. French, University of Southern California: professor of business administration, College of Business Administration, University of Illinois, Chicago; staff associate, Office of the President, University of Illinois.

Michael J. Gardone, Jr.: assistant professor, department of economics, Northern Illinois University.

J. K. S. Ghandi: associate professor of finance, Wharton School, University of Pennsylvania.

Martin J. Gruber: assistant professor of finance, Graduate School of Business Administration, New York University.

W. Eric Gustafson: assistant professor of economics, University of California, Davis.

Albert R. Gutowsky: assistant professor of economics, Arizona State University.

Bernard F. Haley, Stanford University, emeritus: visiting professor of economics, University of California, Santa Cruz.

Keith A. J. Hay: assistant professor of economics, Carleton University.

Shinsuke Horiuchi: assistant professor, department of economics, Northern Illinois University.

Harry G. Johnson: visiting professor, department of economics, Northwestern University, spring quarter.

Ethel B. Jones, University of Arkansas: associate professor of economics, University of Georgia.

Vernon Kam, University of California, Berkeley: assistant professor of accounting, College of Business Administration, University of Illinois, Chicago.

W. E. Kuhn: professor of economics, Roosevelt University, September 1965.

Leonard Kunin: acting assistant professor of economics, University of California, Santa Cruz.

Lester Lave: visiting assistant professor, department of economics, Northwestern University.

Mildred Levy, Northwestern University: instructor of economics, College of Business Administration, University of Illinois, Chicago.

Millard F. Long: associate professor of business economics, Graduate School of Business, University of Chicago.

James C. Loughlin: assistant professor of economics, Trinity College.

Jacob B. Michaelsen, University of California, Berkeley: assistant professor of economics, University of California, Santa Cruz.

Constantine Michalopoulos: instructor in economics, Trinity College.

Deborah D. Milenkovitch: instructor of economics, Barnard College.

Edward J. Mock: associate professor of finance, University of Southern California.

Dale T. Mortensen: assistant professor, department of economics, Northwestern University.

Ronald P. Moses, University of Chicago: assistant professor of economics, College of Business Administration, University of Illinois, Chicago.

Michael H. Moskow, Lafayette College: assistant professor of economics, Drexel Institute of Technology.

Alan P. Murray, staff economist, Joint Economic Committee: economist, Joint Committee on Internal Revenue Taxation.

James F. Niss: assistant professor of economics, College of Business Administration, University of Illinois.

N. D. O'Bannon, Federal Reserve Bank of Atlanta: associate professor of economics, Pacific University.

William L. Parks: associate professor, department of agricultural economics and marketing, Rutgers—The State University.

Ivor Pearce: visiting professor of economics, Wharton School, University of Pennsylvania.

Donald K. Pemberton, University of Kansas: assistant professor of economics, Hiram College.

Henry M. Peskin, Institute for Defense Analyses: scientific staff, MATHEMATICA.

James A. Pope, III: instructor, department of economics, Guilford College.

Ronald L. Racster: assistant professor of real estate, University of Southern California.

Miguel A. Reguere: associate professor of finance and statistics, College of Business Administration, Drexel Institute of Technology, June 1965.

Eldon Reiling: assistant professor, department of economics, Northern Illinois University.

Robert H. Renshaw: assistant professor, department of economics, Northern Illinois University.

Fred B. Renwick: assistant professor of finance, Graduate School of Business Administration, New York University.

Norton H. Robbins: associate professor, department of economics, Guilford College.

Roy J. Sampson: professor of transportation, School of Business Administration, University of Oregon.

Sherman Shapiro, senior economist, Comptroller of the Currency: professor of economics, College of Business Administration, University of Illinois, Chicago.

Mitchell M. Smiland: professor of economics, Blackburn College.

William B. Sorge: assistant professor of finance, University of Southern California.

Richard J. Swersey: assistant professor of industry, Wharton School, University of Pennsylvania.

George R. Taylor: Senior resident scholar, Eleutherian Mills-Hagley Foundation Incorporated.

Donald L. Thompson: associate professor of marketing, School of Business Administration, University of Oregon.

William W. Tongue, economist, Jewel Tea Co., Inc.: professor of economics and finance, College of Business Administration, University of Illinois, Chicago.

Elias H. Tuma, assistant professor of economics, University of California, Davis.

Clifford E. Wheeler: assistant professor of economics, Westminster College.

Oliver E. Williamson: associate professor of economics, Wharton School, University of Pennsylvania.

Richard D. Winkelman: assistant professor of economics, Arizona State University.

Francis W. Wolek: assistant professor of industry, Wharton School, University of Pennsylvania.

Colin Wright: assistant professor, department of economics, Northwestern University.

Leaves for Special Appointments

Nicholas G. Bohatiuk, Le Moyne College: visiting associate professor, James Wilson department of economics, University of Virginia, 1965-66.

Oswald Brownlee: visiting scholar, department of banking and economic research, Office of the Comptroller of the Currency.

Rondo Cameron, University of Wisconsin: visiting professor, Instituto de Economia, Universidad de Chile, 2 years.

Robert Clower, Northwestern University: University of Essex, England, 1965-66.

Sidney Klein, Rutgers—The State University: director of the economics training program, Council for International Economic Cooperation and Development, Executive Yuan, Government of China, Taiwan, 1965-66.

David T. Kleinman, Fordham University: visitor, department of applied economics, Cambridge University.

Lester S. Levy, Northern Illinois University: Fulbright lecturer, Turku School of Economics and Swedish School of Economics, Abo Academy, Finland.

Joseph A. Martellaro, Indiana University: Fulbright lecturer on economic development, University of Cordoba, Argentina.

Howard W. Nicholson, Clark University: program director for economics, National Science Foundation, academic year 1965-66.

Thomas J. Orsagh, Lehigh University: visiting professor of economics, University of North Carolina, 1965-66.

Edwin P. Reubens, City College of the City University of New York: project director, United Nations Research for Social Development, Geneva, Switzerland, July 1965-September 1966.

Ronald G. Ridker, Syracuse University: visiting research professor of international economics and development, Brookings Institution, academic year 1965-66.

Ira O. Scott: visiting scholar, department of banking and economic research, Office of the Comptroller of the Currency.

Lee Soltow, Ohio University: University of Ibadan, Nigeria, academic year 1965-66.

Milton H. Spencer, Wayne State University: visiting research professor of business economics, University of Hawaii, July 1965 to June 1966.

Resignations

Deane Carson, Brown University, June 30, 1965.

Albert O. Hirschman, Columbia University.

Robert Lekachman, Columbia University.

Paul A. Weinstein, Columbia University.

SIXTY-SECOND LIST OF DOCTORAL DISSERTATIONS IN POLITICAL ECONOMY IN AMERICAN UNIVERSITIES AND COLLEGES

The present list specifies doctoral degrees conferred during the academic year terminating June 1965, and theses undertaken in the same period.

General Economics; Methodology

Degrees Conferred

Carl R. Jones, Ph.D. Claremont 1965. The Gurley-Shaw theory of rudimentary economy: an analysis of its local structure and methodological completeness.

Theses in Preparation

Lawrence A. Boland, B.S. Bradley 1962; M.S. Illinois 1963. The methodological implications of some recent contributions to modern capital theory. *Illinois.*

John O. Cole, B.A. Duke 1958; M.A. Indiana 1964. Methodological problems in economics. *Indiana.*

Harriet M. Hinck, B.S. Long Island 1944; M.A. Alabama 1949. A critical appraisal of Joan Robinson's contribution to economic analysis. *Rutgers.*

Norman L. Lindsey, B.A. Antioch 1957. Methodological problems in the implementation of inter-industry models at the regional level. *Rutgers.*

Price and Allocation Theory; Income and Employment Theory; History of Economic Thought

Degrees Conferred

MUHAMMAD S. A. ABOU-ALI, Ph.D. Harvard 1965. Effects of income distribution on saving-income ratios and capital formation with special reference to low-income countries.

JOEL I. BREST, Ph.D. Brown 1965. The demand for plant and equipment: an econometric study of investment.

EDWIN D. BURMEISTER, Ph.D. Mass. Inst. Technology 1965. Stability and causality in two-sector models of economic growth.

DAVID CASS, Ph.D. Stanford 1965. Optimal economic growth.

YOUNG-IOB CHUNG, Ph.D. Columbia 1965. The role of government in the generation of saving: the Japanese experience, 1868-1893.

JOHN CONLISK, Ph.D. Stanford 1965. The analysis and testing of the asymptotic behavior of aggregate growth models.

ARTHUR J. CORDELL, Ph.D. Cornell 1965. Imperfect and monopolistic competition in historical perspective.

PIERRE R. CROSSON, Ph.D. Columbia 1964. Harrod-Domar growth models: concepts vs. empirical materials.

PETER L. DANNER, Ph.D. Syracuse 1964. An inquiry into the social aspects of Adam Smith's theory of value.

PAUL W. EATON, Ph.D. Minnesota 1965. On the intersection of Edgeworth and equilibrium sets.

KENNETH D. GOLDIN, Ph.D. Stanford 1964. The optimum rate of savings.

SURAJ GUPTA, Ph.D. Chicago 1964. Expected rate of change in prices and rates of interest.

WILLIAM J. HOCTER, D.B.A. Indiana 1965. A theory of consumer investment.

EUGENE P. HOWREY, Ph.D. North Carolina 1964. Some aspects of technical change, capital longevity and economic growth.

PETER J. KALMAN, Ph.D. Purdue 1965. Theory of choice when prices enter the utility function.

DONALD W. KATZNER, Ph.D. Minnesota 1965. Integrability and non-integrability in demand theory.

LUBOMYR M. KOWAL, Ph.D. Illinois 1965. Economic doctrines of M. I. Tugan-Baranovsky.

BONG S. LEE, Ph.D. Harvard 1965. English mercantilist thought as a theory of economic growth.

ROBERT E. LOVETT, Ph.D. Southern California 1965. Concepts of entrepreneurship in recent economic thought.

ANDREA MANESCHI, Ph.D. Johns Hopkins 1965. Efficient paths of capital accumulation and optimum rates of saving.

JOHN R. MORONEY, JR. Ph.D. Duke 1964. Relative income shares in the United States, 1922-1961.

CLARENCE C. MORRISON, Ph.D. North Carolina 1964. Marginal cost pricing and the general theory of second best.

MARNIE W. MUELLER, Ph.D. Yale 1965. Structural inflation and the Mexican experience.

SEONG Y. PARK, Ph.D. Yale 1965. Bounded substitution, fixed proportions, and economic growth.

ROBERT A. POLLAK, Ph.D. Mass. Inst. Technology 1964. Two essays in the theory of consumer behavior.

CEDRIC J. POUNDERS, Ph.D. Southern Methodist 1965. The choice among alternative weapons and weapons systems: an economic analytic treatment.

RICHARD W. RUPPERT, Ph.D. Purdue 1965. Dynamic stability and qualitative economics.

RALPH RUSSELL, Ph.D. Harvard 1965. The empirical evaluation of theoretically plausible demand functions.

GIAN SAHOTA, Ph.D. Chicago 1965. An analysis of the causes of the secular decline in the relative price of fertilizer.

KARL SHELL, Ph.D. Stanford 1965. Invention and technical change.

HUGO F. SONNENACHEIN, Ph.D. Purdue 1964. The relationship between transitive preference and the structure of the choice span.

CARL B. TURNER, Ph.D. Duke 1965. An analysis of Soviet views on John Maynard Keynes.

JEAN-PIERRE M. WAMPACH, Ph.D. Cornell 1965. Technological change and the production function.

IAN D. S. WARD, Ph.D. California (Berkeley) 1964. Early theories of economic development.

ROGER N. WAUD, Ph.D. California (Berkeley) 1965. Structural unemployment in the United States.

ARNOLD A. WEINSTEIN, Ph.D. Virginia 1965. Transitivity of preference: a comparison between age groups.

ALEXEJ WYNNYCZUK, Ph.D. Columbia 1964. Changes in the structure of national income.

Theses in Preparation

IMTIAZUDDIN AHMAD, B.A. Muslim (India), 1958; M.A. 1960. Time rate of diffusion of a product innovation in the manufacturing sector. *New York (Buffalo).*

HERTA AMIR, B.A. Queens 1955; M.A. California (Los Angeles) 1958. Disaster without recovery. *California (Los Angeles).*

ROBERT A. BLUM, B.A. California (Berkeley) 1961; M.A. 1964. External diseconomies and remedies through the law. *California (Berkeley).*

SAM H. BOOK, B.S. Wilkes 1962. John Maurice Clark: an analysis of his works and a discussion of his economic philosophy. *Columbia.*

KEITH C. BROWN, B.S. Texas 1953; M.A. Southern Methodist 1964. A theoretical and statistical study of decision-making under uncertainty: competitive bidding for leases on offshore petroleum lands. *Southern Methodist.*

ROBERT COEN, B.A. Harvard 1961. Tax incentives for investment. *Northwestern.*

FORREST E. COOKSON, JR., B.S. Clemson 1953; M.A. Princeton 1957. The pure theory of the Phillips curve. *Georgetown.*

DAVID J. DONALDSON, B.A. Toronto 1960; M.A. Stanford 1963. The theory of welfare economics when conditions of risk and uncertainty prevail. *Stanford.*

M. G. A. EL DAHAB, B.S. Cairo 1952; Dipl. in Statistics 1958; M.S. Kansas State 1962. An aggregation procedure for deriving representative firms in estimating supply functions. *Kansas State.*

FRANCES D. FERGUSON, B.A. St. Francis 1961; M.A. Fordham 1962. Survey and analysis of time series econometric studies of U.S. manufacturing investment in plant and equipment. *Boston College.*

K. L. GUPTA, B.A. Panjab 1957; M.A. Delhi 1959. Empirical aspects of the theory of aggregation. *Toronto.*

JAGDISH HANDA, B.Sc. London School of Economics 1962. Technical change and inputs in the production function. *Johns Hopkins.*

DONALD J. HARRIS, B.A. University College of West Indies 1960. Inflation, capital formation and accelerated growth. *California (Berkeley).*

STEPHEN N. K. HU, B.A. National Taiwan 1956; M.S. Tennessee 1958. On some aspects of the theory of the multi-product firm. *North Carolina.*

BDUL-FATTAH KANDEEL, L. L. B. Cairo (U.A.R.) 1953; M.A. Stanford 1960. Welfare considerations in economic appraisal of projects. *Southern California.*

IERAN A. KENNEDY, B.A. University College, Dublin, 1960. Some aspects of industrial productivity in Ireland. *Harvard.*

ASUO MURATA, B.A. Kobe 1953; M.A. 1955. Consumption and economic Growth. *Stanford.*

ONALD MURRY, B.A. Missouri 1959; M.A. 1961. Conditions of optimum spatial allocation of resources for scientific research. *Missouri.*

HILIP A. NEHER, B.A. Pomona 1956. Multi-sector models of economic growth with applications to the Latin American experience. *Brown.*

ONALD A. NICHOLS, B.A. Yale 1962; M.A. 1963. An underemployment growth model of the United States economy. *Yale.*

EORGE E. SCHNEIDER, B.A. St. Meinrad 1954; M.A. Chicago 1962. Adam Smith's theory of economic policy. *Notre Dame.*

HARLES H. SHAMI, B.S. Columbia 1961; M.A. 1964. Monopolistic elements in the theory of internal improvements by Charles Ellet, Jr.: 1810-1862. *Columbia.*

YTAN SHESHINSKI, B.A. Hebrew 1960; M.A. 1962. Short and long run employment and productivity. *Mass. Inst. Technology.*

SMAIL ABDEL-HAMID SIRAGEL-DIN, B.Sc. Cairo (U.A.R.) 1954; M.S. Toronto, 1962. Potential national income and production of time in the U.S. *Michigan.*

OSEPH E. STIGLITZ, B.A. Amherst 1964. Essays in the theory of growth and income distribution. *Mass. Inst. Technology.*

SAUH-AN SU, B.A. National Taiwan, 1959; M.A. Rutgers 1963. Dynamic input-output model with respect to labor productivity. *Rutgers.*

EOPOLD A. SVEIKAUSKAS, B.A. Harvard 1959; M.A. Yale 1960. The elasticity of substitution and efficiency parameters in United States two-digit manufacturing. *Yale.*

AKASHI TSUSHIMA, B.L. Waseda 1958; M.A. Rutgers 1964. A critical study in the mathematical programming theory of economic growth. *Rutgers.*

OMAN L. WEIL, JR. B.A. Yale 1962; M.S. Carnegie Inst. Technology 1965. A new approach to the generalized von Neumann model and turnpike theory. *Carnegie Inst. Technology.*

ICHARD D. WINKELMAN, B.A. Southern Illinois 1961; M.A. Illinois 1963. An investigation into the influence of late 19th century American economists on selected federal policy and legislation. *Illinois.*

Economic History; Economic Development; National Economies

Degrees Conferred

ABBAS A. K. AL NASRAWI, Ph.D. Harvard 1965. Financing economic development in Iraq: complications of the dependence on oil revenues.

GEORGE H. BOSSY, Ph.D. Columbia 1964. Effects of tariff protection on economic development in Australia: Victoria and New South Wales from 1871 to 1900.

ADOLPH S. BUTKYS, Ph.D. Pennsylvania 1964. The development of export in an infant agrarian economy—Lithuania between 1918 and 1940.

LOUIS M. BYKOSKI, Ph.D. Western Reserve 1965. Economic development of India and Pakistan, 1945-60.

LIH Y. CHANG, Ph.D. Cornell 1965. Economic development under foreign aid and preparation for war: a case study of Taiwan, 1942-1962.

LASZLO CZIRJAK, Ph.D. Columbia 1965. Hungarian industrial development as revealed by production indexes, 1938, 1946-1960.

THOMAS R. DE GREGORI, Ph.D. Texas 1965. Sub-Saharan Africa: a technological frontier.

NOEL J. J. FARLEY, Ph.D. Yale 1965. Government policy for economic growth in a small economy: a case of Ireland, 1948-1960.

LEONARD P. FLETCHER, Ph.D. Brown 1965. Some aspects of economic development in Trinidad, 1951 to 1959.

DAVID T. GEITHMAN, Ph.D. Florida 1964. Money and income in Colombia, 1950-1960.

RONALD S. GRAYBEAL, Ph.D. Stanford 1965. An econometric model of the external trace of Hawaii.

RAYNAL L. HAMMELTON, Ph.D. Southern California 1964. New port cities in Western America and their effect on economic development.

HAROLD H. G. HENRY, Ph.D. Cornell 1964. The Basutoland economy: a case study of backwardness in a traditional society.

NICOLAS HERNANDEZ, Ph.D. Rutgers 1965. The entrepreneurial role of the government in the economic development of Puerto Rico.

SAMUEL P. S. HO, Ph.D. Yale 1965. Development alternatives—the case of Taiwan.

BARTELL C. JENSEN, Ph.D. Purdue 1965. Impact of reparations on the post-war economy of Finland: an input-output study.

SAKARI JUTILA, Ph.D. Syracuse 1965. Multiplier-accelerator-diffusion model for the dynamics of regional economic development.

HYONG C. KIM, Ph.D. Oregon 1964. Factors contributing to the rapid growth of Japan's exports of manufactures, 1953-61.

HY S. LEE, Ph.D. Wisconsin 1965. The entrepreneurial activities of the government in the economic development of Puerto Rico.

WILLIAM P. MCGREEVEY, Ph.D. Mass. Inst. Technology 1965. Economic development of Colombia.

JAMES B. MONTGOMERY, Florida 1965. An economic analysis of a locally oriented community.

DEAN W. MORSE, Ph.D. Columbia 1965. John Stuart Mill on history and economic development.

ROBERT MUSCAT, Ph.D. Columbia 1964. Economic development strategy in Thailand.

LAWRENCE H. OFFICER, Ph.D. Harvard 1964. An econometric model of the Canadian economy under the fluctuating exchange rate.

IBAHIM I. POROY, Ph.D. California (Berkeley) 1965. Planning for economic development: an input-outcome model for Turkey.

EMIL SALIM, Ph.D. California (Berkeley) 1964. Institutional structure and economic development: the case of Egypt.

ERNEST STERN, Ph.D. Fletcher School 1964. The role of government in the economic development of Turkey, 1948-1960.

ARTHUR I. STONEHILL, Ph.D. California (Berkeley) 1965. Foreign-owned business enterprises in Norway.

SUBRAMANIAN SWAMY, Ph.D. Harvard 1965. Economic development and income distribution in a developing nation: the case of India.

EDWARD VAN ROY, Ph.D. Texas 1965. A study of economies in the hills of North Thailand.

JEROME C. WELLS, Ph.D. Michigan 1964. An appraisal of agricultural investments in the 1962-68 Nigerian development program.

MAURICE WILKINSON, Ph.D. Harvard 1965. Swedish economic growth: a model of the long swing.

Theses in Preparation

OMAR E. ALBERTELLI, M.A. Fordham 1963. Skill requirements and manufactured exports in developing economies. *Columbia.*

ROBERT ARMSTRONG, B.A. Yale 1958; M.A. Johns Hopkins 1960. The role of direct

private foreign investment in an underdeveloped economy: the Liberian case. *Northwestern.*

JOHN BRODE, B.A. Harvard 1959. The role of foreign trade in Japanese industrial growth. *Harvard.*

DAVID CANTOR, B.A. Boston 1957; M.A. 1958; M.A. Harvard 1961. Balanced vs. unbalanced growth in Ghana. *Harvard.*

PETER B. CLARK, B.A. Williams 1956. The relationship between intertemporal linear programming models and micro investment criteria with reference to development in Nigeria. *Mass. Inst. Technology.*

JON S. COHEN, B.A. Columbia College 1960; M.A. California (Berkeley) 1964. Study of the relationship between finance and industrialization in Italy from 1861 to 1914. *California (Berkeley).*

FRANCIS X. COLACO, B. Com. Bombay 1958; M.B.A. Detroit 1961. Factors affecting changes in the cost of living in Brazil: a sectoral analysis. *California (Berkeley).*

HARRELL C. DAVIS, M.A. California (Berkeley) 1961. Economic development of Spain. *California (Berkeley).*

DHARMASENA DESILVA, B.S. Evansville 1957; M.S. Southern Illinois 1959. The record of foreign aid in Ceylon's economic development, 1948-1963. *Indiana.*

ALBERTO P. DI PIERRO, B.A. Pisa 1961; M.A. Virginia, 1963. The growth of Soviet industry: a critical analysis of Western estimates. *Virginia.*

MILTON ESBITT, B.A. City (New York) 1961; M.A. Michigan State 1962. Discontinuity in the process of self-sustaining economic growth: the American economy during the 1830's. *Michigan State.*

JORGE F. FREYRE, Dr. in Law, Havana 1954; M.A. Yale 1956. Role of external investment in financing the economic development of Puerto Rico, with special reference to the servicing of external capital inflows. *Yale.*

MYRON J. FRANKMAN, B. Mgt. E., Rensselaer Polytechnic Inst. 1961. Postwar commercial policy in Chile. *Texas.*

NACHUM T. GROSS, M.A. Hebrew 1954. Industrialization in Austria in the nineteenth century. *California (Berkeley).*

UDO E. G. HEYN, B.A. San Diego State 1953; M.S. San Francisco State 1961. The role of Frankfurt am Main, as a center of international finance, in the industrialization of Germany, 1825-75. *Wisconsin.*

JOHN W. ITON, B.A. McGill 1962. Economic development and the external sector: a study of the South American experience in the 1950's and early 1960's. *Johns Hopkins.*

ANCILLA M. KILBOURN, B.A. Trinity 1953; M.A. Catholic 1958. A study of the impact of the colonial policies of Great Britain and France on the economic development of Ghana and the Ivory Coast. *Johns Hopkins.*

DAVID J. KLOCK, B.A. Middlebury 1960. The impact of money, trade and foreign exchange policy on economic growth in the Philippines, 1950-1962. *Columbia.*

JOSEPH S. LACASCIA, B.A. Mexico City 1958; M.A. Middlebury 1961. Capital formation in Mexico from 1958 to 1964. *Florida.*

PETER D. LATIMER, B.A. Ohio Wesleyan 1961; M.A. Fletcher School 1962; M.A.L.D. 1963. Economic and social change under the Alliance for Progress: the Colombian response. *Fletcher School.*

RICHARD H. MARSHALL, B.A. Emory and Henry 1962. The postwar economic development of Spain. *Virginia.*

DAVID MISHKIN, B.A. Queens 1958; M.A. Columbia 1960. The American colonial vineyard: an economic interpretation, 1565-1800. *Illinois.*

SYED M. NASEEM, B.A. Sind Muslim College 1952; M.A. Karachi 1956; M.S. London 1959. Import substitution and industrialization: the possibilities for Pakistan during 1960-70. *Yale.*

JOHN D. PATRICK, B.A. Harvard 1961; B.A. Oxford 1963. Economic growth, external balance, and the optimum policy mix. *Columbia.*

SARAH C. PETERS, B.A. Duke 1961; M.A. Yale 1963. A cost analysis of community development food-for-work programs: Mexican experience. *Yale.*

BARRY W. POULSON, B.A. Ohio Wesleyan 1959; M.A. Ohio State 1964. Value added in manufacturing, mining and agriculture in the American economy from 1809-1839. *Ohio State.*

JOSEPH R. RAMOS, B.A. Columbia 1959; B.S. 1960. Shifts in labor force composition in post World War II Latin American economic development. *Columbia.*

PETRONIO RIOS, B.S. Minas Gerais 1955. Brazil economic development: an analysis of structural changes. *Iowa (Ames).*

CARL A. RISKIN, B.A. Harvard 1960. Disguised unemployment utilization costs and a multi-technique strategy of development in China. *California (Berkeley).*

DANIEL K. ROYER, B.C.E. Florida 1943; M.S.B.C. 1963. Economic development of El Salvador, 1945-1955. *Florida.*

JOHN A. SHAW, B.A. San Diego State 1961; M.S. Purdue 1964. Economic changes in the U.S. in the 1930's. *Purdue.*

FRANKLIN B. SHERWOOD, B.A. Connecticut 1958; M.A. 1959. The role of the Guatemalan government in the economic development of Guatemala. *Illinois.*

JOSEPH E. SICHLER, B.A. Georgetown 1963. Innovation and finance in the British industrial revolution—1760-1820. *Georgetown.*

EDUARDO SOSA-RODRIGUEZ, B.S. Mexico 1943; M. City Planning, Harvard 1962; M.A. North Texas State 1964. Economic history of Venezuela. *Texas.*

NYLE SPOELSTRA, B.A. Colorado 1961. Regional patterns in Philippine economic growth. *Wisconsin.*

KIMON P. VALASKAKIS, B.A. American (Cairo) 1961. Power politics and the French international economic position: 1870-1914. *Cornell.*

JAMES N. VEDDER, B.S. Buffalo 1957; M.B.A. 1959. Determining the objectives of state economic development programs. *Michigan.*

FREDERICK S. WEAVER, JR., B.A. California (Berkeley) 1961. Fiscal decentralization as a strategy for economic development in Chile. *Cornell.*

PETER B. WEBB, B.A. Toledo 1960; M.A. Indiana 1962. The Alliance for Progress and agrarian reform in the light of the Mexican experience. *Indiana.*

SAMUEL H. WILLIAMSON, B.S. Purdue 1962; M.S. 1964. An investigation of the type and volume of commodities carried by coastal shipping in the U.S. from 1820-1860. *Purdue.*

MEZMUR YEHEYES, B.A. University College (Addis Ababa) 1956; M.S. Illinois 1958. British imperialism in East Africa: a study in the economic origins of Kenya. *California (Berkeley).*

RICHARD ZIND, Licencie en Droit, University of Paris 1947; M.B.A. Southern California 1963. An estimate of some basic parameters of the Moroccan economy. *Southern California.*

Statistical Methods; Econometrics; Social Accounting

Degrees Conferred

ANTONIO CAMACHO, Ph.D. Minnesota 1965. Alternative concepts of informational efficiency in resource allocation mechanisms.

JOHN G. CRAGG, Ph.D. Princeton 1965. Small-sample properties of various simultaneous-equation estimators: the results of some Monte Carlo experiments.

JON CUNNYNGHAM, Ph.D. Chicago 1964. The spectral analysis of economic time series.

AHMED H. I. EL-GOWAINY, Ph.D. Syracuse 1964. Theory and methodology of the public sector in national income accounts.

ROY D. HARRIS, Ph.D. California (Los Angeles) 1965. An empirical investigation of a jobshop as a network of queueing systems.

HIDEO KANEMITSU, Ph.D. Minnesota 1965. The K-process—a study of informational efficiency in decentralized resource allocation processes.

THOMAS J. MUENCH, Ph.D. Purdue 1965. Consistency of least square estimators of coefficients in explosive stochastic difference equations.

DONALD B. RICE, Ph.D. Purdue 1965. Discrete optimizing solutions to integer programming problems.

SALLY S. RONK, Ph.D. New York 1965. The sources and uses of funds approach to analysis of interest rate developments.

A. GARY SHILLING, Ph.D. Stanford 1964. Relation of certain economic variables to the Federal Reserve flow of funds accounts.

A. V. SRINIVASAN, Ph.D. Chicago 1965. An investigation of some computational aspects of integer programming.

Theses in Preparation

LOWELL R. BASSETT, B.S.M.E. Carnegie Inst. Technology 1959; M.S. Purdue 1964. Potential stability of economic matrices. *Purdue.*

BEN W. BOLCH, B.B.A. Emory 1960; M.A. 1962. A spectral analysis of the interrelationship of certain asset markets and the money supply. *North Carolina.*

PETER B. BURLESON, S.B. Mass. Inst. Technology 1961; M.B.A. Cornell 1963. An econometric investigation of the U.S. import-export balance in steel. *Cornell.*

ROBERT C. BUSHNELL, B.A. Oberlin 1957. The use of linear programming in non-linear problems. *Princeton.*

PERIYAPATNA S. DHRUVARAJAN, B.S.C. Mysore 1953; M.A. Delhi 1956; M.S. Chicago 1960. Problems of estimation in simultaneous equation systems with autoregressive disturbances. *Northwestern.*

FLOYD HARMSTON, B.A. Utah; M.A. Wyoming (no years given). The use of inter-industry multipliers in the analysis of regional change. *Missouri.*

SANTIAGO I. FRIEDMAN, Civ. Engr. Chile 1953; M.Sc. California (Berkeley) 1964. An algorithm for dynamic programming of economic growth. *California (Berkeley).*

T. K. KUMAR, B.Sc. (Hons.) Andhra (India) 1959; M. Stat. India Statistical Inst. 1961. Models of optimization and control in the theory of economic policy. *Iowa (Ames.)*

MICHAEL C. MCCRACKEN, B.A. Rice 1961. M.A. Southern Methodist 1964. A multi-country econometric model. *Southern Methodist.*

KAREN POLENSKE, B.A. Oregon State 1959; M.A. Syracuse 1961. Estimating inter-regional trading pattern. *Harvard.*

ROSE M. RUBIN, B.A. Wellesley 1961. Aggregation criteria in regional input-output analysis. *Kansas State.*

M. V. R. SASTRY, B.S. Andhra (India) 1955; M.S. Gujarat (India) 1957. A critical appraisal of methods of estimation of econometric models—with special reference to agricultural sector of Brookings S.S.R.C. model. *Iowa (Ames).*

MILAD TAWADROUS, B. Comp. Ein Shams (Cairo) 1950; M. Comp. U.A.R. 1959. Econometric model of the State of Iowa: consumption sector. *Iowa.*

TERRY M. WALKER, B.S. Florida State 1961. A study of the small sample properties of selected simultaneous equation estimators utilizing distribution sampling techniques. *Alabama.*

KENNETH F. WALLIS, B.Sc. (Hons.) Manchester 1959; M.Sc. Tech. 1961. Business and economic statistics: econometrics. *Stanford.*

JOHN C. WERTZ, B.A. Montana State 1961; M.B.A. Northwestern 1962. Dynamic programming approaches to complex business decisions. *Purdue.*

KUNIO YOSHIHARA, B.A. Long Beach State 1962. An econometric model of Japan, 1902-61. *California (Berkeley).*

TZONG-SHIAN YU, B.A. National Taiwan 1956; M.A. Indiana 1962. The measurement of the demand for imported automobiles. *Indiana.*

STANLEY ZIONTS, B.S.-E.E. Carnegie Inst. Technology 1958; M.S.-I.A. 1960. Size-reduction techniques of linear programming and their application. *Carnegie Inst. Technology.*

Economic Systems; Planning and Reform; Cooperation

Degrees Conferred

AZHARI (no first name supplied), Ph.D. Indiana 1965. Public control and economic planning in Indonesia 1950-1962.

JOSEPH T. BOMBELLES, Ph.D. Western Reserve 1965. Planning and economic growth in Yugoslavia, 1947-61.

SEI Y. CHO, Ph.D. Columbia 1965. Japanese economic planning, 1952-62.

LORAINE DONALDSON, D.B.A. Indiana 1965. The implementation of Irish development planning: a study of government influence and control of industrial investment.

NOKYOON KWAK, Ph.D. Southern California 1964. A comparative appraisal of developmental planning in India and Communist China under the second five-year plan.

HAROLD MOODY, Ph.D. California (Los Angeles) 1964. The theory and application of regional income and product planning.

STANLEY A. NICHOLSON, Ph.D. Duke 1965. The economy of Ghana with special reference to government strategy for economic development.

GILBERT L. RUTMAN, Ph.D. Duke 1965. An analysis of the economy of Tanganyika with special reference to development planning.

MANUEL A. VELEZ-MONTES, Ph.D. Rutgers 1965. A public policy model of economic growth in underdeveloped market economies.

WILLIAM T. WHITE, Ph.D. Georgetown 1965. Socialism, communication and money: a theoretical study with an application to the case of Yugoslavia.

Theses in Preparation

JERALD R. BARNARD, B.S. Utah State 1959; M.S. 1961. A model for economic analysis in state development planning. *Iowa (Ames).*

FOUAD T. E. TAHER, B. Com., Cairo 1953; M.B.A. Pennsylvania 1962. The effective integration of targets and budgets in planned economics the U.A.R. case. *Pennsylvania.*

PAUL W. GAEBELEIN, JR., B.A. Pomona 1941. Israel's new economic policy. *Claremont.*

RACHEL GOLDEN, B.A. Bryn Mawr 1957. Recent trends in Soviet personal income and consumption. *Columbia.*

SALMA A. S. F. KANDEEL, B. Com. Cairo 1959; M.A. California (Berkeley) 1964. Planning under socialism and risk. *Southern California.*

DOUGLAS H. KEARE, B.S. Dartmouth 1956; M.S. Thayer School of Engineering & Amos Tuck School of Bus. Admin. 1957. French economic planning and French economic policy: 1958-1965. *Princeton.*

SHUMPEI KUMON, B.S. Tokyo 1957; M.S. 1962. Recent developments in the Soviet theory of economic planning. *Indiana.*

SOM LIEM, B.A. Indonesia 1958; M. A. Harvard 1962. Flexibility of development planning and world price movement. *Harvard.*

C. A. KNOX LOVELL, B.A. William and Mary 1963. Cybernetics and Soviet economic planning. *Duke.*

ANA M. MARTIRENA DE MANTEL, C.P.A. Buenos Aires 1959. A dynamic model for planning a "rapid" and "smooth" development. *Yale.*

Rolf R. Mantel, C.P.A. Buenos Aires 1961. General equilibrium and development planning: existence and computation of optimal programs. *Yale.*

Jerry L. Petr, B.A. Cornell 1961. Possibilities and problems in rationalizing production and trade in Soviet bloc energy materials. *Indiana.*

J. Eugene Poirier, Lic. L'IM. Conception (Montreal) 1951; Lic. Bellarmine (P.I.) 1958; M.A. Georgetown 1962; M.A. Yale 1963. Planning economic growth: capital-labor substitution and foreign-aid. *Yale.*

Ralph G. Saylor, B.A. Kalamazoo 1962. The economic system of Sierra Leone. *Duke.*

Erfan A. Shafey, B.A. Alexandria (Egypt) 1962; M.A. Leeds (London) 1956. Ideology and practice of socialism in the United Arab Republic. *Southern California.*

Samir Y. Sidhom, B. Com. Cairo 1949; M.A. Stanford 1963. Developmental planning in the United Arab Republic (Egypt); an economic appraisal. *Southern California.*

Robert C. Stuart, B. Comm. British Columbia 1961. The methodology of managerial decision making in the Soviet Kolkhoz, 1952-1962. *Wisconsin.*

Charles K. Wilber, B.A. Portland 1957; M.S. 1960. The Soviet model of economic development: a case study of Soviet Central Asia. *Maryland.*

Wassyl Znayenko, B.S. Franklin & Marshall 1953. The development and use of regional inter-industry models in the Soviet Union. *Columbia.*

Business Fluctuations and Forecasting

Degrees Conferred

Sven Arndt, Ph.D. California (Berkeley) 1964. Disequilibrium in an open economy: Canada 1950-62.

James P. Bennett, Ph.D. Michigan 1964. An econometric study of investment demand in the United States steel industry.

Kyou E. Choi, Ph.D. Florida 1964. Florida business cycles, 1920-1960.

Peter C. Dooley, Ph.D. Cornell 1964. The upper turning points of 1953, 1957, and 1960.

Bruce F. Duncombe, Ph.D. Minnesota 1964. Interregional multipliers.

Victor Garlin, Ph.D. California (Berkeley) 1965. Technological change and business cycles in the post-war period.

Charles S. Rockwell, Ph.D. California (Berkeley) 1964. Profits, normal backwardation and forecasting in commodity futures.

Reynold M. Sachs, Ph.D. Columbia 1965. Manufacturer's capital appropriations as a measure of investment decisions: an econometric study of quarterly time series.

Ralph Sultan, Ph.D. Harvard 1964. The stock adjustment effect in the demand for nine consumer durables.

Rafael J. Villanueva, Ph.D. Columbia 1964. The inflationary process in Argentina, 1943-1960.

Theses in Preparation

Charles McV. Becker, B.A. Arizona 1960; M.A. 1962. A comparison of specific cycles in U.S. copper output with reference cycles. *Arizona.*

Gerard Brown, B.A. Wernersville 1950; M.A. St. Louis 1956. Range of seasonality over time. *Georgetown.*

William P. Carton, B.A. Princeton 1943. The national bureau of economic research's program in business cycle research. *Georgetown.*

George D. Craig, B.A. Wheaton 1960; M.S. Illinois 1962. Predictive accuracy of aggregate consumption functions. *Illinois.*

Keith A. J. Hay, B.Sc. Econ. Honors, Southampton 1959; M.Sc. Toronto 1962. A new approach to the American origin of the Canadian business cycle, 1867-1960. *Brown.*

FRANCISCO A. TREVINO-CISNEROS, C. P. & A. Instituto Technologico (Mexico) 1959; M.B.A. Columbia 1962. Business cycle patterns and indicators in the economic growth of Mexico. *Columbia.*

Money, Credit and Banking; Monetary Policy; Consumer Finance and Mortgage Credit

Degrees Conferred

ROBERT W. ADLER, Ph.D. Oregon 1965. The organized financial markets of Colombia.

LOUISE F. AHEARN, Ph.D. Columbia 1965. The financing of U.S. government securities dealers, 1960–1963.

BERNARD E. ANDERSON, Ph.D. Ohio State 1964. An investigation into the effects of banking structure on aspects of bank behavior.

ROY C. ANDERSON, Ph.D. Tulane 1965. Swedish monetary and fiscal policy 1945-1959.

WILLIAM BEAZER, Ph.D. Northwestern 1965. Optimization of bank portfolios and response to monetary policy.

JOSEPH C. BLUMEL, Ph.D. Oregon 1965. Nonmonetary financial intermediaries and the effectiveness of monetary policy.

PATRICIA F. BOWERS, Ph.D. New York 1965. An analysis of the influence of the Federal Reserve Bank of New York upon the policy decisions of the Federal Reserve System, 1946-1956.

ALBERT J. BRETON, Ph.D. Columbia 1965. The demand for money in Canada, 1900-1959.

ANDREZEJ BRZESKI, Ph.D. California (Berkeley) 1964. Inflation in Poland, 1945-1960.

EDWARD J. CAMPBELL, Ph.D. New York 1965. The role of nonbank financial institutions in the process of financial intermediation.

DAVID W. COLE, D.B.A. Indiana 1965. Senior securities in the capital structures of commercial banks.

DWIGHT B. CRANE, Ph.D. Carnegie Inst. Technology 1965. The commercial banking market and the demand deposits of business firms.

DELEEUW, FRANK, Ph.D. Harvard 1965. A model of financial behavior.

HUGH R. ELLIOTT, Ph.D. Chicago 1964. Savings deposits as money.

LIANG-SHING FAN, Ph.D. Minnesota 1965. A study in the maturity structure of interest rates.

CURTIS F. FORNER, Ph.D. Rutgers 1965. A study in postwar American central-banking: theory and policy.

HENRY J. GUITHUES, Ph.D. Saint Louis 1965. Standards of financial operating efficiency of Missouri chartered credit unions.

ALBERT R. GUTOWSKY, Ph.D. Oregon 1965. The Oregon commercial banking industry.

PHILLIP J. HAHN, Ph.D. Western Reserve 1965. Factors determining adequacy of capital in commercial banks.

GEORGE D. HANRAHAN, Ph.D. Minnesota 1964. Three econometric monetary models of the United States—1952-1960.

PATRIC H. HENDERSHOTT, Ph.D. Purdue 1965. Monetary policy, 1952-62.

JEROME J. HOLLENHORST, Ph.D. Iowa (Ames) 1965. An analysis of the Federal Land Bank System's debt management, 1947-1961.

NGUYEN T. HUNG, Ph.D. Virginia 1965. An analysis of money and credit in Vietnam (1884-1962).

WILLIAM H. JEAN, Ph.D. Purdue 1964. An econometric study of monetary policy.

ROBERT A. JOHNSTON, Ph.D. Yale 1965. The Canadian experience with the floating discount rate 1956-62.

GEORGE K. KARDOUCHE, Ph.D. Brown 1965. Monetary developments and policy in the U.A.R. (Egypt) 1952 to 1962.

DAVID T. KRESGE, Ph.D. Harvard 1965. An econometric model of the commercial banking system.

KERN O. KYMN, Ph.D. Chicago 1964. The demand for currency in Japan.

DAVID F. LOMAX, Ph.D. Stanford 1964. Monetary control in Peru from 1945 to 1960.

GORDON H. MELLISH, Ph.D. Virginia 1965. Official intervention and the Canadian dollar. 1950-1962.

RICHARD L. MOORE, Ph.D. Claremont 1965. The structure of competition: California Savings and Loan Association.

MOHAMMED I. NADIRI, Ph.D. California (Berkeley) 1965. U.S. balance of payment and monetary policy 1958-1962.

RONALD OLSON, D.B.A. Indiana 1965. Profit planning in commercial banks.

SAM PELTZMAN, Ph.D. Chicago 1965. Entry in commercial banking.

MARJORIE P. PERSON, D.B.A. Indiana 1965. Marketing and banking: an evaluation of marketing program development in selected commercial banks.

JAMES L. PIERCE, Ph.D. California (Berkeley) 1964. The monetary mechanism: some partial relationships.

WILLIAM RUSSELL, Ph.D. Washington 1964. Commercial bank portfolio adjustments.

ANNA J. SCHWARTZ, Ph.D. Columbia 1964. A monetary history of the United States: 1867-1960.

HAROLD T. SHAPIRO, Ph.D. Princeton 1964. The Canadian monetary sector: an econometric analysis.

GORDON R. SPARKS, Ph.D. Michigan 1965. The monetary sector in a U.S. econometric model.

WILLIAM F. THOMPSON, Ph.D. Pittsburgh 1965. An economic model of the United States money and capital market.

ROBERT J. TOMPA, Ph.D. Rutgers 1965. An analysis of the postwar monetary policy of America.

AUBREY D. TUSSING, Ph.D. Syracuse 1964. The availability of credit.

GERHARD W. VOSSHALL, Ph.D. New York 1964. The West German banking system.

NEIL WALLACE, Ph.D. Chicago 1964. The term structure of interest rates.

RICHARD S. WALLACE, Ph.D. Virginia 1965. A case study of three small market areas.

DONALD Q. WEBB, Ph.D. Southern Methodist 1965. Economic development and central banking in Costa Rica.

ROBERT G. WILSON, Ph.D. Harvard 1964. The monetary system of Egypt: an interpretation.

Theses in Preparation

BASIL AL-BUSTANY, B.A. Baghdad 1957; M.A. Pennsylvania 1961; Monetary policy in Iraq, 1950-1963. *American.*

BRYAN A. BAILEY, B.A. Clark 1959. Multiple correlation and regressions effects of CD's on the composition of bank assets, liabilities, liquidity positions, and profitability. *Brown.*

JOHN W. BAY, B.A. St. Ambrose 1961. Cyclical changes in commercial bank investment portfolios and their implications for monetary policy. *Boston College.*

R. L. BOLSTER, B.S. American; M.B.A. New York. The relationship of monetary policy to the stock market. *American.*

VICTOR A. BONOMO, B.S. Duquesne 1962. Expectations and liquidity premiums in the term structure of interest rates. *Brown.*

ROBERT D. BOWERS, B.A. Lebanon Valley 1956; M.A. Pennsylvania State 1960. The Lexington bank merger case. *Western Reserve.*

DONALD T. BUCK, B.S. New Hampshire 1955; M.A. 1961. Money, inflation and economic growth in less developed countries—an empirical study. *Pennsylvania.*

ROBERT H. CHANDROSS, B.S. New York 1962. Competition and entry in banking. *Princeton.*

WILLIAM G. COLBY, JR., B.A. Amherst 1960; M.A. Yale 1963. Correspondent balances and the theory of bank liquidity. *Yale.*

PAUL A. CROWE, B.A. Wabash 1959. Deposit turnover and legal requirements of Louisiana banks. *Tulane.*

WILLIAM W. CURTIS, B.S. College of Idaho 1961; M.A. Illinois 1962. Characteristics of savings account holders. *Illinois.*

GEORGE DALY, B.A. Miami 1962. A theoretical investigation of the savings and loan industry. *Northwestern.*

JEROME C. DARNELL, B.S. Southwest Missouri 1957; M.B.A. Indiana 1963. Chain banking as a form of banking concentration. *Indiana.*

EDWARD E. DARROW, B.S. Purdue 1960; M.S. Ohio State 1963. The structure of the farm mortgage credit market in Ohio. *Ohio State.*

JOHN V. DONOVAN, B.A. Boston College 1961. Inflation in Peru. *Virginia.*

ARTHUR R. DORSCH, B.S.M.E. Rutgers 1942; M.B.A. American International 1958. Consumer installment sales credit in the United States economy and its relationship to monetary policy. *Florida.*

WILLIAM J. FRAZER, JR., B.A. Huntingdon 1950; M.A. Columbia 1953. The liquidity structure of firms and monetary economics. *Columbia.*

BARRY L. FRIEDMAN, B.A. Chicago 1962. Current or permanent income in the demand for money. *Mass. Inst. Technology.*

DAVID A. GOLD, B.S. Cornell 1961. Lags in monetary policy: the evidence from commercial bank behavior. *Columbia.*

GEORGE D. GREEN, B.A. Stanford 1961; M.A. 1963. Banking and finance in ante-bellum Louisiana (1812-1861): their influence on the course of economic development. *Stanford.*

NATHANIEL GREENSPUN, B.A. Harvard 1951; M.A. 1953. The importance of mergers to structural change among the three hundred largest commercial banks 1950-1963. *Harvard.*

JOHN H. HAND, B.A. Swarthmore 1962. The effect of credit rationing on corporate investment. *Mass. Inst. Technology.*

A. GILBERT HEEBNER, B.A. Denver 1948; M.A. Pennsylvania 1950. Negotiable time certificates of deposit. *Pennsylvania.*

HEINZ HELLER, B.A. Parsons 1961; M.A. Minnesota 1962. Adequate and optimal international reserves. *California (Berkeley).*

RANDELL H. HOEMKE, B.B.A. Detroit 1953; M.A. Michigan State 1962. Branch banking and economic growth. *Michigan State.*

JOSEPH J. HORTON, JR. B.A. New Mexico State 1958; M.A. Southern Methodist 1965. The theory of the money supply. *Southern Methodist.*

GEORGE K. KARDOUCHE, B.S.C. London School of Economics 1957; M.A. Clark 1959. Monetary developments and policy in Egypt, 1952-1962. *Brown.*

KWAN H. KIM, B.S. Pennsylvania 1959; M.A. Stanford 1964. Demand for money and quasi-money in sixty countries for the postwar period: time-series and cross section analyses. *Stanford.*

MICHAEL KLEIN, B.A. Northwestern 1962. Member bank portfolio response patterns to open-market operations: a flow-of-funds analysis of the cyclical behavior of the money and government securities markets. *Northwestern.*

BUMSUN LEE, B.S. Seoul National 1952; M.B.A. New York 1957. Monetary control and central banking in Korea. *New York.*

HARVEY LEWIS (earlier degree not supplied). An analysis of sources of bank funds and income, state banks in Arkansas, 1954-1963. *Arkansas.*

ROLF LUDERS, M.A. Chicago 1960; M.B.A. 1960. Monetary history of Chile. *Chicago.*

ANTHONY MACH, B.A. Washington & Jefferson 1952; M.B.A. Pennsylvania 1956. A comparison of federal reserve check collections with bank debits as an empirical estimate of the transaction velocity of money. *Boston College.*

DAVID L. MCKEE, B.A. St. Francis Xavier 1958; M.A. New Brunswick 1959. An economic analysis of credit unions in Michigan. *Notre Dame.*

CHARLES L. MILLER, JR., B.B.A. Iowa 1962; M.A. Yale 1963. Interdistrict monetary integration in the United States. *Yale.*

JOHN S. MURPHY, M.A. Nebraska 1958. An empirical study of the relationship between monetary variables and the cost of capital. *New York (Buffalo).*

NEIL B. MURPHY, B.S. Bucknell 1960; M.S. 1961. An analysis of the national banking market. *Illinois.*

VIRGINIA L. OWEN, B.S. Illinois State 1962; M.S. Illinois 1963. The economic effects of computer technology on commercial banks. *Illinois.*

MORRIS PERLMAN, B.B.A. City (New York) 1961. International differences in liquid asset portfolios. *Chicago.*

H. KIRKWOOD ROBERTS, B.S. Indiana 1961; M.A. (no year). Critique of statistics derived from demand functions for money. *Indiana.*

GERALD SAZAMA, B.A. Marquette 1960; M.S. Wisconsin 1964. State government industrial loan authorities: their impact on investment completion. *Wisconsin.*

MARC C. SCRIVEN, B.A. California (Berkeley) 1960; M.A. 1964. The demand and supply of money and velocity. *California (Berkeley).*

AHARON SHAPIRO (earlier degree not supplied). British balance of payments and monetary theory, 1958-65. *New School.*

JAMES F. SMITH, B.A. Southern Methodist 1961; M.A. 1964. Determinants of the volume and structure of consumer instalment credit 1947-1964: an econometric analysis. *Southern Methodist.*

ANDREW STERN, B.A. New York 1960; M.A. 1961. Impact of international reserve asset flows on domestic monetary sector of economies. *Columbia.*

JAMES J. SULLIVAN, B.A. California (Santa Barbara) 1961; M.A. 1963. A simultaneous equations monetary model: with emphasis on the supply of money. *Johns Hopkins.*

MAUREEN SULLIVAN, B.A. Smith 1962. Monetary instrument variables as regressors in expenditure functions. *Northwestern.*

SAMUEL H. TALLEY, B.A. Syracuse 1953; M.B.A. 1958; M.A. Michigan 1962. A critique of the present method of regulating bank-type financial intermediaries and an examination of alternative methods. *Syracuse.*

RICHARD K. VEDDER, B.A. Northwestern 1962; M.A. Illinois 1963. History of the Federal Deposit Insurance Corporation. *Illinois.*

DAVID VROOMAN, B.A. Cornell 1959. Bank mergers and public policy. *Northwestern.*

JAN P. WOGART, Abitur, Gymnasium Hamburg 1958; M.A. Texas 1965. The problem of hyperinflation in advanced and underdeveloped countries: the German inflation 1923—the Brazilian inflation 1963. *Texas.*

COLIN WRIGHT, B.S. Brigham Young 1960. Savings and the rate of interest. *Chicago.*

Public Finance; Fiscal Policy

Degrees Conferred

MARIO BORINI, Ph.D. New York 1964. A practical guide to tax planning through Subchapter S.

LORA S. COLLINS, Ph.D. Harvard 1964. Expenditures for public assistance in the United States.

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WALTER W. BENNETT, Ph.D. Florida 1964. Institutional barriers to the utilization of women in top management.

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Sui N. Wong, Ph.D. California (Berkeley) 1965. A theoretical model of capital budgeting.

Theses in Preparation

Faisal Al-Khalaf, B.C. California (Berkeley) 1954; M.A. San Francisco State 1955. Production in marketing problems of Iraqi dates. *American.*

Thomas H. Bates, B.A. Southern California 1951; M.S. San Francisco State 1959. An analysis of the pattern of supply to the United States sugar market and its efficiency as to allocation will be undertaken. *California (Berkeley).*

Alfred J. Boyd, Jr., B.A. Maryland 1962; M.B.A. George Washington 1963. Policies and practices within a Japanese industry in evaluating requirements for and sources of automated systems. *American.*

William H. Brickner, B.S. Case Inst. Technology 1949; M.B.A. Miami 1958. Factors affecting the price of new industrial products. *Stanford.*

Victor J. Cook, Jr., B.A. Florida State 1960; M.S. Louisiana State 1962. Multiple brand control and the center of discretionary market power. *Michigan.*

Arthur V. Corr, B.S. New York 1955; M.B.A. 1956. Profitability of segments of a business: its measurement and application for evaluating management performance. *New York.*

Lawrence Duewer, B.S. Illinois 1960; M.S. 1962. Market simulation of economic behavior in the meat products industry. *Iowa (Ames).*

Joseph N. Fry, B.Sc. Alberta 1958; M.B.A. Western Ontario 1960. An analysis of the influence of family branding on consumer brand choice for selected, frequently purchased packaged goods. *Stanford.*

Elon H. Gilbert, B.A. Stanford June 1961; M.A. 1963. Marketing of staple foods in Northern Nigeria. *Stanford.*

Jacques, Harlow, B.A. Dartmouth 1950. Management as an information system. *New York.*

Ronald L. Heim, B.S. Rutgers 1960; M.S. (Ag. Ec.) 1961; M.S. (Statistics) 1963. Operations analysis of supermarket purchase allowances. *Cornell.*

Brian Hindley, B.A. Chicago 1961. The economic effects of separation of ownership and control in the large corporation. *Chicago.*

Bernard L. Hinton, B.S. Wayne State 1960; M.B.A. 1962. A model of creative problem solving behavior and performance under various psycho-environmental and motivational conditions. *Stanford.*

William H. Kaven, B.S. Ohio State 1946; M.B.A. Kent State 1962. A study of the relationships between food chains and ice cream manufacturers with regard to private label. *Cornell.*

Raymond J. Lawrence, B.A. Pembroke College 1949; M.A. 1963. Statistical regularities in consumer behavior. *California (Berkeley).*

ALAN J. LEVY, B.S. Columbia 1949; M.S. 1952. Decline in business. *New York.*

JOHN L. LIVINGSTONE, B.Com. Witwatersrand 1957; B.Com. South Africa 1958; M.B.A. Stanford 1963. Effects of alternative accounting methods on managers' behavior in the control of operations. *Stanford.*

JAMES B. MCCOLLUM, B.A. Georgia Inst. Technology 1953; M.S. 1954. Information, expectations and entrepreneurial choice. *Tulane.*

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Industrial Organization; Government and Business; Industry Studies

Degrees Conferred

JALEEL AHMAD, Ph.D. Mass. Inst. Technology 1965. Investments in electric power systems for rural supply: a study in mathematical programming.

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KATHLEEN CAMIN, B.A. Smith 1957. Costs of treating meat packing waste. *Missouri.*

THOMAS C. COMMITTEE, B.A. Bethany 1947; LL.B. West Virginia 1949; M.A. Texas Christian 1964. An economic analysis of the federal regulation of the natural gas industry. *Alabama.*

BRUNO CONTINI, Laurea in Scienze Economiche, University Bocconi, (Milan) 1960; M.S. Carnegie Inst. Technology 1962. Arbitration schemes for organizations with multiple goal structures. *Carnegie Inst. Technology.*

DANE J. COX, B.A. Harpur College 1958. Allocation of turnpike costs in the short run. *Cornell.*

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ISIDORE J. MASSE, B.A. Assumption 1961. The impact of government research and development expenditures. *New York (Buffalo).*

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EDWARD L. PHILLIPS, B.A. Washington 1959; M.A. 1964. Internal pricing in a multiproduct, vertically integrated forest product firm. *Washington.*

WILLIAM A. REYNOLDS, B.A. William and Mary 1938; M.A. New York 1940. Effects of innovation in the U.S. carpet industry: 1947-1963. *Columbia.*

RAYMOND C. ROBAK, B.A. Texas 1958; M.A. 1962. The application of the antitrust laws to various regulated industries. *Texas.*

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HARVEY SCHWARTZ, B. Com., Toronto 1960; M.A. 1961. The North American agricultural implement industry—a study of industrial integration under free trade. *Illinois.*

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THOMAS J. SHENLEY, B.A. Providence 1950; M.A. Catholic 1961. Competition for the New England natural gas market. *Notre Dame.*

ALLEN SINAI, B.A. Michigan 1961. A theoretical and empirical study of fixed investment in the post-war United States steel industry. *Northwestern.*

AJIT SINGH, B.A. Punjab 1958; M.A. Howard 1960. A study of post-war take-overs in U.K. and a contribution towards a theory of take-over bids. *California (Berkeley).*

FRANK SMITH, B.A. Massachusetts 1957; M.A. Northwestern 1960. The economics of water pollution and the wood pulp industry. *Northwestern.*

WALTER A. SMITH, B.S. Tulsa 1940; M.A. Oklahoma 1963. Water usage in the hotel and motel industry in Oklahoma. *Oklahoma.*

SIMON TEITEL, Industrial Engineering Buenos Aires 1956; M.S. Columbia 1962. Growth patterns of manufacturing industries. *Columbia.*

CECIL THOMPSON, B.A. California (Los Angeles) 1959; M.A. 1961. The economic impact of ultra high frequency transmissions. *California (Los Angeles).*

JOHN F. TINSLEY, M.A. Rutgers 1954. Some aspects of operations of autonomous public authorities. *Rutgers.*

DAVID I. VERWAY, B.A. Denver 1958; M.A. Michigan State 1960. Factors affecting the consumption of personal services: an areal cross-section analysis. *Michigan State.*

CARL E. WADE, B.S. Marshall 1952; M.S. West Virginia 1954. Analytical appraisal of monopolistic price discrimination. *Iowa.*

GARY M. WALTON, B.A. California (Berkeley) 1963; M.A. Washington 1964. A quantitative study of the colonial shipping industry. *Washington.*

ALBERT P. WILLIAMS, B.S. U.S. Naval Academy 1957; A.M. Fletcher School 1963; M.A.L.D. 1964. Industrial diversification and Venezuelan public policy. *Fletcher School.*

Land Economics; Agricultural Economics, Economic Geography; Housing

Degrees Conferred

RUFUS O. ADEGBOYE, Ph.D. Iowa (Ames) 1964. Improving land use in Nigeria through removing defects in land inheritance.

SABBAH AL HAJ, Ph.D. Ohio State 1964. Quality comparisons for greenhouse and competing tomatoes.

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THE U.S. DOWNTURN OF 1948

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Although the upper turning point of 1948 in the United States has been explained reasonably satisfactorily, the explanation lacks rigor. Since the circumstances preceding the 1948 downturn were unusual, none of the general-purpose models of the business cycle fits the facts adequately, and nobody has devised a model for this special purpose. The present paper comments on the contributions of Hamberg [5], Bratt and Ondrechen [3], Blyth [1] [2], and Hickman [7]; it makes a minor contribution to the empirical story; and it attempts to add rigor by presenting a special-purpose model. Section I briefly reviews the facts. Section II discusses prior contributions. Section III gives a verbal explanation of the 1948 turn. Succeeding sections and the Mathematical Appendix set forth the model.

I. *The Facts*

The suppressed inflation of World War II left consumers and businesses in 1946 with a very large amount of liquid assets compared to income and output; and the diversion of output to war goods greatly aggravated the deficiencies that had started to accumulate during the depression of the 1930's—deficiencies of durables and semidurables in the hands of consumers of buildings (both residential and nonresidential), of producers' durable equipment, and of business inventories, all relative to the amounts desired at full-employment rates of output and income. With the partial exception of business inventories, only a little had been done to make good the deficiencies when, in 1946, inflation burst into the open following abolition of price control. GNP in money terms rose about one-third between the beginning of 1946 and the end of 1948. Something like three-quarters or four-fifths of the rise was attributable to price increases. The rise in output, hampered by bottlenecks, was quite modest.

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The economy did not expand at uniform speed over these years. The rise in both industrial production and prices (though not real GNP) paused in the second quarter of 1947. There was another pause early in 1948 (this one affecting real GNP). A decline in inventory investment from a peak in the second quarter of 1946 to less than zero in the middle quarters of 1947 was responsible for the first pause[1] and gives evidence that inflation had not generated expectations of further inflation (which would have led to speculative accumulation of stocks) but, if anything, the reverse. (This point is relevant to appraising the hypothesis of Bratt and Ondrechen.) The pause of early 1948 occurred in spite of a steady expansion of inventory investment between the third quarters of 1947 and 1948 and was associated with a sharp break in the prices of farm products resulting from expectations of large crops. These facts also suggest that inventory investment was not based on expectations of rising prices.

Before the end of 1948, the inflationary forces had spent themselves. The most pressing of the deficiencies had been made good. The ratio of liquid assets to GNP had fallen, primarily because prices had risen but also because the federal budget had had surpluses. Although the Federal Reserve System had generally played the role of permissive parent toward the inflationary behavior of the economy, there was some tightening of monetary policy in 1948, notably the reimposition of consumer credit controls in September. As a result of such developments and also an income tax cut in the spring of 1948, personal saving—which had almost vanished in the second quarter of 1947—rose to a proportion of disposable income not much short of what has come to be typical of the postwar era.[2] Though business fixed investment in current

[1] A more detailed investigation of 1946-48 than I have made would have to look into the pause of 1947 more carefully. Hickman's explanation [7, pp. 53-63] is not altogether satisfactory. He treated the decline of inventory investment [7, pp. 57 and 59] as if it were something new in early 1947; actually the decline between the second quarter of 1946 and the third quarter of 1947 was continuous (though not steady). Moreover, he did not look into the role of the agricultural sector. The decline in farm income accounts for virtually the whole of the decline in personal income in the monthly figures for 1947 [10, p. 148]; and in the second quarter of 1947, when the lull was most marked, there was no decline in nonfarm inventory investment, the entire decline in total inventory investment evidently being in the farm component. Even in the first quarter, when the decline in nonfarm inventory investment was large, the decline in total inventory investment was significantly larger. All this suggests some special influence at work in the agricultural sector.

[2] Between the second quarter of 1947 and the last of 1948, personal consumption expenditures in 1954 dollars rose slowly and steadily at a rate that averaged three-quarters of a billion per quarter (seasonally adjusted annual rate). The increase in the second quarter of 1948 was only a trifle greater than the average ($0.9 billion), that of the third quarter somewhat less ($0.4). The income tax cut affected the statistics most noticeably in the second quarter of 1948, when a rise in personal income in current dollars of only

dollars rose steadily throughout 1947 and 1948, the general rise masked declines within the aggregate, notably in spending on new plant and equipment for manufacturing [7, p. 66]. Exports fell heavily from the spring of 1947 to the end of 1948 under the influence of forces similar to those affecting domestic spending: foreign countries had depleted their liquid assets and had overcome some of their most serious deficiencies. Although housing shortages still persisted, investment in residential construction leveled off in the middle of 1948 and actually declined toward the end of the year, partly in response to somewhat less easy money conditions. For a time the general expansion kept going, propelled by a steady rise in government spending (federal as well as state and local) and increasing investment in inventories; but inventory investment reached its peak in the third quarter, at about the same time as prices, and went into a decline in the fourth quarter, dragging the rest of the economy with it. The business-cycle peak, as determined by the National Bureau of Economic Research, came in November. Just prior to the peak, the rise in prices decelerated while the rise in stocks accelerated.

II. *Prior Contributions*

Hamberg, while discussing tighter money and laying some stress on falling exports, emphasized most of all the fall in the consumption function (rise in personal saving), which led first to "a sharp rise in the rate of inventory accumulation, one which, in the light of other factors, we can assume to have been considerably of the unplanned variety" [5, p. 8] and then to reduced output and the recession. Bratt and Ondrechen offered the alternative hypothesis that inventory investment fell because of changed expectations when prices ceased rising

6.9 billion (seasonally adjusted annual rate) was accompanied by a rise in disposable income of $9.3. In this quarter personal saving rose $6.6 (from $4.8 to $11.4). It appears that the initial impact of the tax cut was to hasten the rise in personal saving, the short-run marginal propensity to spend on consumer goods and services being very low, but it also appears that the rise in personal saving was quite a bit greater than could be accounted for by the tax cut. Personal saving was still only 6 per cent of disposable income. In the third quarter, the rise in disposable income was again greater than the rise in personal income, though the excess was much smaller. Again the rise in personal saving was large compared to the rise in consumption, though less so, and personal saving exceeded 7 per cent of disposable income. All this perhaps makes greater use of first and second differences than the quality of the statistics warrants, but it gives adequate basis for the statement in the text. (The statistics cited here are all from [10, pp. 124 and 146-47].)

Comparisons of personal saving with the second quarter of 1947 give an exaggerated impression. A large drop in farm income in that quarter actually reduced personal income, but consumption went on rising. Then, as in the second quarter of 1948, consumption evidently was not much influenced in the very short run by fluctuations in income. Hence, the very low figure for personal saving must be regarded as due to special circumstances.

and gave signs of falling [3]. Blyth attributed the fall in inventory investment to an earlier decline in real business fixed investment [1] [2]. Hickman's explanation does not lend itself to such overly simplified condensation as has just been given for the others because it is less "tight" and more descriptive [7].

Hamberg's explanation cannot be deemed entirely adequate, and his use of the word "underconsumptionist" puts the emphasis in the wrong place. Why should a return of personal saving to a normal percentage of disposable income, at a time when investment opportunities were still abundant,[3] cause a recession? Whatever the answer, it is misleading to characterize the situation as underconsumption. Nevertheless, Hamberg was nearer the mark than most of his successors. He put some of the key forces into the forefront even though he did not tell the whole story.

The alternative hypothesis of Bratt and Ondrechen, that the end of inflation and the signs of deflation so altered expectations that inventory investment ceased, is not convincing. They give no direct evidence for their hypothesis, relying on the power of the hypothesis to explain the general course of events. But 1946-48 contrasts strikingly with 1919-20, when the boom was aggravated by speculative accumulation of stocks based on expectations of rising prices [8, pp. 18-19]. In 1946-48, inventory investment was inhibited by widespread expectations of a postwar depression. Expectations were not, however, for a drastic deflation of prices such as occurred in 1920-21. The experience of 1937-38 and the changes in the structure of the U.S. economy had already convinced businessmen that prices would be sticky in the downward direction. Inventory investment declined for more than a year after inflation broke out into the open, then slowly rose as

[3] I am not sure whether I am here departing from the views on investment opportunities of Hamberg, whose comments do not seem to be entirely consistent. At one point, he wrote, "If now, the question is asked, why didn't a major downswing develop out of the inventory recession, the answer lies (just as it did during the two minor inventory recessions of the twenties) in a strong and increasing rate of autonomous investment," stressing deficit spending by the government and mentioning also residential construction and the automobile industry [5, p. 11]. Later he implied a shortage of investment opportunities, writing, "there appears to have been a lack of *autonomous* investment, particularly the type emanating from some great innovation(s) . . . a distressing amount of investment appears to have been of the *'induced'* variety. . . . This would bode ill for the future . . ." [5, p. 14, Hamberg's italics]. Presumably the inconsistency is purely verbal. In the first passage, Hamberg included government spending in autonomous investment, in the second excluded it. On this interpretation, there is justification for calling Hamberg's explanation underconsumptionist. My view that investment opportunities were still abundant in 1948-49 is a judgment on which it is impossible to get conclusive evidence one way or the other. From the perspective of 1965, more economists would agree with it than would have in 1952 (but we should beware of changing our minds too readily in response to the vagaries of recent events).

inflationary pressures moderated, reaching its peak at about the same time as the wholesale price index and remaining high after that index fell.[4] The hypothesis of Bratt and Ondrechen is plausible for 1920 but not for 1948, except as a very minor part of the story.

The statistical basis for Blyth's argument is open to doubt.[5] Even if we waive such doubts, his contribution supplements rather than contradicts Hamberg's. Blyth's contribution lay in a disaggregated analysis showing that the fall in inventory investment was concentrated in durables and could be traced to a prior fall in real fixed investment. Now, all major components of GNP were increasing in money terms until almost the very end of the expansion. Although real output in the aggregate was increasing too, it is only to be expected that there were shifts within the total. It is not especially surprising if a fairly significant item showed an actual decline or if the particular kinds of inventories that were reduced in the recession were closely associated with the item that started declining, in real terms, before the peak. Hamberg told a limited story, not going behind the aggregates. A reasonable person, accepting his story and speculating about what went on within the aggregates, might have anticipated something of the general sort that Blyth discovered, but he would not have known what particular items behaved as anticipated and would have been grateful to Blyth for filling in details. In a sense, in fact, Blyth tended to confirm Hamberg by telling a story about the details that fits in with Hamberg's explanation of the totals.

Hickman's discussion is difficult to summarize. He did not comment on the prior contributions, making it hard to tell how far he disagreed

[4] This behavior, however, was especially striking for farm inventories. The break in farm prices that occurred in early 1948 and received widespread publicity as possibly hailing the end of inflation was the occasion for a switch from decumulation (which had gone on in every quarter of 1947) to accumulation (which continued throughout every quarter of 1948) [10, pp. 120 and 124, the difference between "Change in business inventories—total" and "Nonfarm only"]. If the accumulation of farm inventories merely reflected the farm price support program, it would not tell so heavily against the Bratt-Ondrechen hypothesis. (But on a base of 1910-14 = 100, the index of prices received by farmers was higher than the index of prices paid even after the break in prices and did not fall below it until 1949.)

[5] The sum of "Producers' durable equipment" and "Other construction" in 1954 dollars rose steadily to a peak in the fourth quarter of 1948 [10, p. 124]. This sum differs from Blyth's "private non-farm capital expenditures" or "Business Expenditures on New Plant and Equipment" [1, p. 494], the peak in which came in the first quarter, by including farm and apparently also by including business outlays charged to current account. Blyth wrote his article before the Department of Commerce published quarterly figures in constant dollars and he apparently did his own deflating. Blyth's argument depends on the peak in fixed investment coming early in 1948 rather than late so that its decline can account for the turning point in November through a decline in investment in stocks of durables. A more detailed investigation might or might not bear Blyth out.

with them. His own account shifts from description to analysis and back again in a way that leaves doubt as to what his explanation is. His contribution can best be conveyed by a quotation:

> During the second half of 1948, the forces of deflation gained final ascendancy. The downturn developed gradually from an accumulation of depressing factors, each mild in itself, but in sum sufficient finally to cause a decline in aggregate activity. The decline of residential construction after midyear has already been mentioned. Another depressant was added when retail sales leveled off during the third quarter. . . . The retardation affected particularly sales of nondurables, and since stock-sales ratios were being closely controlled by retailers. . . , new orders were immediately trimmed to prevent further accumulation of stocks, wholesale trade in nondurables declined after July and factory sales after September, and manufacturing production of such goods eased during the third quarter and again in the fourth [7, p. 69].

Hickman's comments on one detail are of special interest:

> The early decline of manufacturing investment probably reflected diminished needs for deferred replacements and for capacity expansion to meet postwar levels of demand; modernization and expansion programs had progressed more rapidly in manufacturing than in railroading or electrical utilities, to name two important sectors in which investment continued upward during 1948. Some reduction of investment demand was to be expected once firms had attained satisfactory postwar relationships between capacity and output. . . . In some industries the retardation during 1947 was due to inadequate plant capacity or shortage of materials, but in many product lines it must be traced to a diminution of consumer demand as backlog needs were satisfied for durable or semidurable goods [7, p. 66].

The preceding quotations convey the flavor of Hickman's discussion. He did not formulate a hypothesis that could be compared with the other hypotheses or tested in order to accept or reject it. His discussion is best regarded as suggestive, as a useful source of ideas, not as a subject for critical appraisal.

III. *Verbal Explanation*

In verbal terms, the explanation of the upper turning point of 1948 runs as follows. The open inflation of 1946-48 was bound sooner or later to come to an end. As the most urgent needs of consumers and businesses were met and as inflation reduced the real value of liquid assets, personal saving was bound to accelerate and investment spending to decelerate, with similar developments in foreign trade. If the inflation had generated expectations of further inflation, a speculative boom in inventories, similar to 1919, would have developed, to be fol-

lowed inevitably by a bust analogous to 1920-21 though differing in details.[6] The memory of the great depression of the 1930's may have prevented any speculative boom. Businessmen, instead of raising prices more rapidly or rationing customers more stringently, permitted their stocks to remain below the amounts appropriate to the volume of business they were doing. This policy, though less pernicious than that of 1919, made an ultimate cyclical downturn probable if not inevitable. It meant that, as the inflationary pressures moderated, inventory investment increased until stocks approximated desired levels. Then output was cut back (to prevent stocks from rising too much) more quickly than factors of production could be shifted to such other activities as automobiles where shortages still prevailed.

Business inventories are held, among other things, to provide an alternative to raising prices or rationing customers under circumstances of excess demand. If the prices of goods are administered and sticky, it is normal for excess demand to be met by allowing inventories to run off or to rise less rapidly than was intended and desired. That inventory investment behaved "normally" in the abnormal circumstances of 1946-48 may be deemed peculiar, the result of the memory of the great depression, reinforced by prophecies of imminent collapse. Businessmen were sensible enough to base most decisions on the obvious discrepancies in their own particular fields between output and demand, but the magnitude of the general inflation took people by surprise. Nobody thought it could last. Hence, although the main adjustment to excess demand lay in raising prices, a small part of the adjustment took the form of permitting inventories to remain a little below the economically desirable relation to output and sales. This was why inventory investment was one of the last components of output to reach its peak (automobile production being the main exception) in spite of its comparative immunity to bottlenecks. And this was why there was a downturn when inflation spent itself instead of a smooth transition to steady growth or a rolling readjustment;[7] for inventory investment is highly volatile.

Naturally, all industries did not behave in the same way. On account of bottlenecks, some deficiencies were met more quickly than

[6] The bust would have been less severe, particularly with respect to prices. The Federal Reserve System would not have pursued a deflationary policy after cyclical contraction began. Structural changes in the economy since 1921 would have prevented a drastic fall in prices, and knowledge of those changes would have forestalled (as in fact it did) the self-confirming kind of deflationary expectations that prevailed in 1920-21.

[7] By rolling readjustment, I mean a situation in which one line of activity after another turns down without depressing aggregate output, the declining industries releasing factors of production to expanding ones. Such readjustments can be made when the changes are small and not closely bunched.

others. Specifically, investment in manufacturing, though by no means satiated, was relatively well taken care of by early 1948, and stocks of durables related to such investment reached satisfactory levels so soon that they increased no further in real terms during that year.[8] On the other hand, automobile production was still hampered by bottlenecks after cyclical contraction began. As 1948 wore on, one industry after another found itself in a position to raise stocks, as inflationary pressures tapered off. One after another had subsequently to cut production back (in some cases only after stocks rose too much)[9] until aggregate output became affected. The cutbacks were encouraged by the changing trend of prices.

For all major categories of GNP except government, the story is essentially the same: an initial stock of physical capital too low compared to income (or output or sales) and liquid assets; an inflationary period during which, at rates that varied on account of bottlenecks, deficiencies (and liquid assets in real terms) were reduced; and then a cutback of output and spending. Though details varied, this is the story not only for building construction, producers' durable equipment, and inventories, but also for consumption and exports.

IV. *Simplest Version of the Model*

Metzler's model [9] obviously does not fit the facts of the 1948 downturn. His model has no ceiling. Models using the capital stock adjustment principle in conjunction with the multiplier and a ceiling are more appropriate.[10] Since such models are in real terms, however, they provide only a starting point.

At the outset, let us divide real output (Y) into the usual two categories of investment and consumption. For purposes of the Mathematical Appendix and numerical illustrations, consumption expenditures (C) are defined to include all purchases of new durables and to exclude ser-

[8] Presumably the firms concerned allowed their work forces to decline, mainly by not replacing employees lost through attrition. Hence, other firms that were still trying to expand real output were able to hire more workers. This suggests a test of the hypothesis offered in the text. A disaggregated study should reveal appropriate industries behaving in this way.

[9] The controversy between Hamberg and Bratt-Ondrechen devoted a good deal of fruitless attention to the question of whether, as Hamberg alleged, the inventory investment at the cyclical peak was "considerably of the unplanned variety" [5, p. 8]. Unhappily, no statistics are compiled on the extent to which inventory investment is unintentional, even though the books of at least one large corporation I know of are kept in a way that lend themselves to the extraction of such information. I do not believe, however, that any of the hypotheses offered for 1948 stand or fall on whether a considerable amount of inventory investment was unintentional.

[10] See, for instance, [4]. Clark, however, has an inventory equation unnecessarily complex for explaining 1948.

vices from durables already owned by consumers at the start of the period.[11]

Within the category of investment, it is necessary to treat inventories separately, since the crux of the story is different behavior by inventory investment. At the outset, therefore, we divide real output, Y, into consumption, C, and two forms of investment, one form being inventory investment, ΔV, with the other, ΔK, representing everything else. Both categories of investment are looked on as changes in an existing physical stock, V standing for inventories, K for everything else (including part of government purchases and foreign).

So long as there is no deficiency of aggregate demand, real output, Y, is equal to—and grows at the same rate as—maximum or capacity output, Y^m. For simplification, it is assumed for the moment that excess demand (the excess of C, ΔK, and desired inventory investment over Y^m) is met by inventory investment falling short of the desired rate. (In fact, the main impact of excess demand fell on prices; and, to some extent, excess demand was thwarted by nonprice rationing.) Over time, as the capacity of the economy grows, the sum of consumption plus noninventory investment $(C + \Delta K)$ grows less rapidly than capacity, permitting a gradual rise in inventory investment, ΔV. Ultimately, the actual amount of inventories, V, gets raised to the wanted or desired amount, V^w, whereupon output gets cut, precipitating recession. The desired amount of inventories is assumed to grow with real output, though less than proportionately. It is assumed that initially there is excess demand and that inventories are less than desired $(V < V^w)$.

Real consumption expenditures, as usual, are assumed to depend on real national output and, in the short run, to grow less than proportionately to it. That is, the marginal propensity to spend on consumer goods and services is less than the average propensity. This assumption conforms to the facts of 1947-48.[12]

For convenience of exposition, it is at first assumed that noninventory investment, ΔK, in real terms is so hampered by bottlenecks that it

[11] For other purposes. however, consumption should be regarded as including services from durables, old and new, not purchases of durables. Though the average propensity to spend disposable income on consumer goods and services may have been close to 1.0 during part of 1947, the average propensity to consume nondurables and services (including services of durables) could have been well below 1.0 (and the ratio of actual consumption to disposable income lower still).

[12] Since expenditures on durables yield a much lower amount of current services than nondurables, more rapid growth of spending on durables as they became available in greater quantities implied a slower growth in consumption when defined to include services from all durables, old and new, instead of current expenditures on them.

does not grow at all. This assumption has some empirical justification, but it will be dropped in the next section.

The equations underlying the model appear in the Mathematical Appendix. Table 1 gives a numerical illustration of the first, most simplified version. For arithmetic convenience, it is assumed that capacity, Y^m, grows at a constant absolute amount (5) in each period. The marginal propensity to consume is 0.6, and the growth of the amount of stocks wanted or desired, V^w, is 20 per cent of the growth in output, Y. All variables are in real terms. Excess demand at first depletes inventories even though they are below the amount desired. As output grows, inventory investment becomes increasingly large until business inventories in Period 6 achieve the desired level; then output gets cut, initiating recession. (The model is not designed for exhibiting the mechanism of contraction, which is another story.)

It will be observed that the model results in a downturn in spite of the absence of lags, exogenous disturbances, and unintentional inventory accumulation. To incorporate such complications, particularly lags, would make the model more realistic (and lags would be indispensable for the downswing), but they are not an essential part of the story.

V. *Rising Fixed Investment*

The next step is to introduce the effect of an increase in fixed investment, ΔK. Of course, a sudden jump in fixed investment just at the time when inventories catch up with what is wanted could forestall a downturn provided resources could be transferred rapidly enough. In Table 1, a jump in Other Investment (ΔK) from 25 in Period 6 to 32 in Period 7 would prevent output from falling. But this is to say noth-

TABLE 1—NUMERICAL ILLUSTRATION OF MODEL, SIMPLEST VERSION

Period	Capacity	Real Output	Consumption*	Other Investment	Inventories		
					Investment	Total Stocks**	Stocks Wanted**
t	Y^m	Y	C	ΔK	ΔV	V	V^w
1	105	105	83	25	−3	37	47
2	110	110	86	25	−1	36	48
3	115	115	89	25	1	37	49
4	120	120	92	25	3	40	50
5	125	125	95	25	5	45	51
6	130	130	98	25	7	52	52
7	135	95	77	25	−7	45	45

* Marginal propensity to consume is 0.6.

** End of period. Wanted Inventories grow at 20 per cent of the increase in Real Output.

ing more than that an exogenous disturbance can head off a decline provided that it is timed perfectly and is large enough. Under the circumstances of 1946-48, the rate of increase in fixed investment would tend to diminish, if left to itself, as the most urgent needs were met.

It is equally clear that the downturn will be postponed if the growth of consumption plus the growth of Other Investment equals the growth of output, for in that case, there is no opportunity for inventory investment to rise. The gap between actual and wanted inventories simply keeps growing. Such a situation is necessarily temporary. Under the conditions that followed World War II, the growth of Other Investment sooner or later had to moderate, permitting inventory investment to rise and ultimately bring about a downturn.

Table 2 is the same as Table 1 except that ΔK rises 1 each period (20 per cent of the rise in capacity) and for arithmetic convenience the initial amount of Wanted Inventories was reduced slightly. The number of periods required to bring about a downturn is greater, but otherwise the results are closely similar. In this version ΔK is permitted to continue rising after the cyclical peak to illustrate an essential point, namely, that plenitude of investment opportunities could not prevent the downturn.

VI. *Prices*

So far, all the variables in the model have been in real terms. Excess demand was assumed to dissipate itself in each period solely by reducing inventory investment below the desired rate. This drastic simpli-

TABLE 2—NUMERICAL ILLUSTRATION, RISING FIXED INVESTMENT

Period	Capacity	Output	Consumption	Other Investment*	Inventories		
					Investment	Total Stocks**	Stocks Wanted**
t	Y^m	Y	C	ΔV	ΔV	V	V^w
1	105	105	83	25	−3	37	46
2	110	110	86	26	−2	35	47
3	115	115	89	27	−1	34	48
4	120	120	92	28	0	34	49
5	125	125	95	29	1	35	50
6	130	130	98	30	2	37	51
7	135	135	101	31	3	40	52
8	140	140	104	32	4	44	53
9	145	145	107	33	5	49	54
10	150	150	110	34	6	55	55
11	155	125	95	35	−5	50	50

* $\Delta K = 24 + t$.

** End of period. $\Delta V^w = 0.2\ \Delta Y$.

fication was made to exhibit the main element in the explanation of the upper turning point, the rise in inventory investment that went on until total stocks overtook desired stocks, leading to an abrupt fall in investment in inventories. In point of fact, excess demand in 1946-48 dissipated itself primarily through rising prices, its impact on inventory investment being comparatively small.

Table 3 is the same as Table 1, but with the addition of four columns which show real aggregate demand (D), real excess demand (E), the price index (P, which is the deflator for Y), and undeflated national product (Y^u), i.e., national product in current prices. Table 1 rather than Table 2 was chosen as the basis for Table 3 because in it noninventory investment (ΔK) does not rise. Inasmuch as net exports fell heavily between the third quarter of 1947 and the third quarter of 1948, stability for noninventory investment as here defined, rather than a rise, seems to be nearer reality on an aggregate basis.

Excess demand makes national product in current prices rise faster than capacity and real output. Excess demand is defined as the amount by which maximum output falls short of the sum of consumption, noninventory investment, and the gap between actual and desired stocks ($E = D - Y^m$ where $D = C + \Delta K + V^w - V_{-1}$). The rate of inflation is assumed to depend on the amount of excess demand (except when excess demand is negative). In the short run, i.e., within the time of the periods used in the tables, the rise in prices has only a negligible effect on real demands, although in the longer run it reduces them by lowering the real value of liquid assets. Therefore, in Table 3 as in the previous tables, the short-run burden of adjustment to the inability of output to satisfy all real demands falls solely on inventories.

This analysis assumes that price is the only important rationing device for everything except stocks. In spite of bottlenecks and gray markets, the assumption is by and large justified by the facts.[13] The

[13] It is true that automobiles, e.g., were in short supply at the list prices set by manufacturers. Although new-car dealers took advantage of the situation to allow several hundred dollars less than the wholesale value of trade-ins, it is still true that they sold new cars on terms less favorable to themselves than the market would bear. But a new-used-car market sprang up on which the buyer could get what he wanted if he was willing to pay the price. What kept consumers from buying all the cars they wanted was not nonprice rationing but the expectation that the list prices of manufacturers were closer to future prices than new-used-car prices. (They were also deterred, however, by unfamiliarity with the new institution. People were accustomed to buying new cars from new-car dealers and to expect that a new car driven from a new- to a used-car dealer depreciated rather than appreciated.) Similar remarks could be made about steel, which was available in gray markets even though steel producers were rationing their customers. There may have been nonprice rationing of particular kinds of capital goods, particularly items that had to be made on order, but this exception probably would not constitute an important qualification to the assumption that inventories bore the brunt of excess demand.

TABLE 3—NUMERICAL ILLUSTRATION, PRICES

Period	Capacity	Real Output	Consumption	Other investment	Inventories			Demand		Prices	Output in Current Prices
					Investment	Total Stocks	Stocks Wanted	Aggregate	Excess		
t	Y^m	Y	C	ΔK	ΔV	V	V^w	D*	E†	P‡	Y^{*}
1	105	105	83	25	−3	37	47	115	10	109.5	115.0
2	110	110	86	25	−1	36	48	122	12	121.5	133.6
3	115	115	89	25	1	37	49	127	12	134.1	154.2
4	120	120	92	25	3	40	50	130	10	145.2	174.3
5	125	125	95	25	5	45	51	131	6	152.2	190.3
6	130	130	98	25	7	52	52	130	0	152.2	197.9
7	135	95	77	25	−7	45	45	95	−40	152.2	144.6

* $D = C + \Delta K + (V^w - V_{-1})$.

† $E = D - Y^m$.

‡ $\frac{\Delta P}{P_{-1}} = \frac{E}{Y^m}$

analysis of the inflationary process here contrasts with the usual one where forced saving occurs. In the usual model, there are lags between the earning of income and its receipt and expenditure. In the meantime, prices rise, depriving the income recipient of real income. Real consumption thereby is depressed, and investment (in some sense) is greater. Although the absence of lags in Table 3 is a simplification made for convenience, the circumstances of 1947-48 (including the rate of inflation, the volume of liquid assets, and the cost and availability of credit) rendered lags and forced saving unimportant.

VII. *Conclusion*

We have now arrived at as good a stopping place as could be found for a short paper on this subject. The model, though exceedingly simple, accounts for the major facts of the period. The model could and should be elaborated to explain an ever-increasing amount of data and to become an ever-closer approximation to reality, with an econometric structure the ultimate aim. A definitive monograph would be well worth undertaking. A short statement of the main argument, such as this one, also seems worth doing. But to enter into the middle ground would be merely to introduce successively less important complications (the effect of liquid assets on aggregate demand, an equation for noninventory investment that would make it more than a function of time, unintentional inventory investment, disaggregation, etc.). The result would have the virtue of neither simplicity nor comprehensiveness.

The method used here is the typical one of economics: a set of highly simplifying assumptions is made to get at the essence of the problem; then they are relaxed in successive steps to get a closer approximation to reality. It is surprising that this approach did not long ago become standard operating procedure for those like myself who study business cycles as historical individuals. Although high standards of rigor and precision became normal long since for those who construct theories of the business cycle, as well as for econometric work, historical studies have continued to be not only verbal but often fuzzy, so that one sometimes is at a loss as to exactly what explanation is being offered. It is high time for those of us who use the historical approach to do better. Although I do not disparage other approaches to the study of business cycles, all of which have significant contributions to make, it seems to me quite possible that we shall solve the riddle of the business cycle only by constructing tailor-made explanations for each one and then examining them for the common elements running

through them all. Such a hope cannot be fulfilled unless the individual explanations are precise and rigorous.

MATHEMATICAL APPENDIX

The simplest version of the model, explained in Section IV and illustrated by Table 1, consists of seven equations:

(1) $$Y = C + \Delta K + \Delta V$$

(2) $$C = C_0 + cY, 0 < c < 1 \quad \text{and} \quad C_0 > 0$$

(3) $$Y^m = Y_0^m + gt, g > 0 \quad \text{and} \quad Y_0^m > 0$$

(4) $$\Delta K = \Delta K_0 + kt, k \geqq 0 \quad \text{and} \quad \Delta K_0 > 0$$

(5) $$\Delta V = V^w - V_{-1} \text{[subject to constraint (11)]}$$

(6) $$V^w = V_0^w + wY, 0 < w < 1 \quad \text{and} \quad V_0^w \geqq 0$$

(7) $$V_{-1} = V_0 + \sum_{1}^{t-1} \Delta V$$

This system is subject to the following additional constraints:

(8) $$Y_0^m + g < \frac{C_0 + \Delta K_0 + k + V_0^w - V_0}{1 - c - w}$$

(9) $$w + c < 1$$

(10) $$(1 - c)g > k$$

(11) $$\Delta V \leqq Y^m - C - \Delta K$$

An alphabetical list of the symbols used and their definitions is at the end of this appendix.

In Table 1, $C_0 = 20$, $c = 0.6$; $Y_0^m = 100$, $g = 5$; $\Delta K_0 = 25$, $k = 0$; $V_0^w = 26$, $w = 0.2$; and $V_0 = 40$. The second version (Section V and Table 2) has the same equations. The values of the parameters and the predetermined variables in Table 2 are the same as for Table 1 except that $\Delta K_0 = 24$, $V_0^w = 25$, and $k = 1$.

The constraint imposed on equation (3) by (8) ensures excess demand in Period 1 so that the constraint expressed by (11) operates for at least one period. As long as the constraint is effective, the solution of the model is:

(12) $$Y = Y_0^m + gt$$

$$(13) \qquad C = C_0 + c(Y_0^m + gt)$$

$$(14) \qquad \Delta V = V_0^w + w(Y_0^m + gt) - V_{-1}$$

$$(15) \qquad V^w = V_0^w + w(Y_0^m + gt)$$

The equations for the other unknowns are already in terms of predetermined variables only.

After a number of periods that depends on the initial conditions, excess demand vanishes because $\Delta V^w/\Delta Y$ is a constant fraction, w, whereas $\Delta V/\Delta Y$ can grow without limit. Sooner or later inventory investment exceeds the growth of wanted inventories. When excess demand vanishes, constraint (11) ceases to operate, and the solution of the system becomes:

$$(12A) \qquad Y = \frac{C_0 + \Delta K_0 + kt + V_0^w - V_{-1}}{1 - c - w}$$

$$(13A) \qquad C = \frac{(1 - w)C_0 + c(\Delta K_0 + kt + V_0^w - V_{-1})}{1 - c - w}$$

$$(14A) \qquad \Delta V = \frac{(1 - c)(V_0^w - V_{-1}) + w(C_0 + \Delta K_0 + kt)}{1 - c - w}$$

$$(15A) \qquad V^w = \frac{(1 - c)V_0^w + w(C_0 + \Delta K_0 + kt - V_{-1})}{1 - c - w}$$

To introduce prices into the model (Section VI and Table 3), let D stand for aggregate demand, Y^u for Y in current prices, and P for the deflator for Y^u. It is assumed that all prices rise proportionately. D and E, like all the other variables except P and Y^u, are in real terms, that is, in terms of P_0. Then,

$$(16) \qquad D = C + \Delta K + (V^w - V_{-1})$$

$$(17) \qquad E \equiv D - Y^m$$

$$(18) \qquad \frac{\Delta P}{P_{-1}} = \frac{pE}{Y^m}, \quad \begin{matrix} p > 0 & \text{when} & E > 0, \\ p = 0 & \text{when} & E \leqq 0 \end{matrix}$$

$$(19) \qquad E = V^w - V_{-1} - \Delta V$$

$$(20) \qquad Y^u \equiv \frac{PY}{100}, \; P_0 = 100$$

Equation (19) expresses the requirement that in the end, no matter how much inflation, inventory investment gets depressed by the amount of the

excess demand. Equation (18) makes the speed of inflation depend on the amount of excess demand, an assumption that is fairly realistic, provided the length of the time periods is not too short. As long as constraint (11) is effective, the solution to equation (18) is:

$$(21)\qquad \Delta P = pP_{-1}\left[c + w - 1 + \frac{C_0 + \Delta K_0 + kt + V_0^w - V_{-1}}{Y_0^m + gt}\right].$$

When constraint (11) ceases to be effective, i.e., when E ceases to be positive, a restriction must be imposed on the system to represent the fact that prices are stickier in the downward than in the upward direction. For simplification, it is assumed that $p=0$ when $E\leqq 0$. In Table 3, $p=1$ as long as there is excess demand.

Symbols used:

C = real consumption expenditures
c = marginal propensity to consume
D = real aggregate demand
E = real excess demand
g = increase in capacity GNP (Y^m) per period of time
K = accumulated stock of real investment goods other than inventories
ΔK = real noninventory investment
k = increase in ΔK each period of time
P = price index
p = parameter relating degree of excess demand to rate of inflation
t = time (period)
V = accumulated real stock of inventories (end of period)
ΔV = inventory investment
V^w = desired level of V (end of period)
w = marginal desired stock-output ratio
Y = real net national product
Y^u = net national product in current prices (undeflated)
Y^m = maximum or ceiling Y

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ECONOMIC DISCRIMINATION AND UNEMPLOYMENT

By HARRY J. GILMAN*

The proposition that members of minority groups are "the last to be hired and the first to be fired" has gained wide acceptance. This proposition will be interpreted here as saying that the burden of short-time layoffs and unemployment will fall disproportionately on workers, such as nonwhite workers, who are members of minority groups. Given this interpretation, the aggregate unemployment rates for white and nonwhite workers support the proposition in two respects: (a) the level of unemployment has been persistently greater for nonwhite than for white workers, and (b) the absolute cyclical variability of unemployment rates has been greater for nonwhite than for white workers.

However, differences in aggregate unemployment rates are not necessarily the result of differential hiring and firing of nonwhite workers. Instead, such differences may be accounted for by some characteristics of the nonwhite labor force which also produce higher unemployment rates for subgroups of the white labor force. Nonwhite workers, for instance, are employed disproportionately in the occupations in which unemployment rates for white workers are highest in level and in the absolute size of the cyclical swings. Also, within occupations nonwhite workers have lower levels of education (see Table 3) and less on-the-job training[1] than do white workers.

Moreover, although there is ample evidence of economic and political discrimination against nonwhite persons in the United States, it is important to distinguish the effects of discrimination on, for example, the level of training and the occupational distribution from its effects on unemployment rates, given the level of skill. The cure for discrimination at the points of hiring and firing may be quite different from the cure for higher unemployment rates due to low levels of skill.

This paper examines only the difference in levels of unemployment rates between white and nonwhite experienced male workers.[2] The

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[1] See Jacob Mincer's estimates of investment in "On-The-Job Training" [8].

[2] Comparisons between the "color" unemployment rate differentials during peaks and troughs of business cycles reveal that nonwhite workers are neither laid off earlier nor in

1940 and 1950 census data show a greater impact of unemployment on nonwhite than on white workers. This differential impact continues into the 1960's. For instance, in the period October, 1953-October, 1961, the average unemployment rates for white and nonwhite male wage and salary workers were 5 and 10.4 per cent, respectively. The two questions for which this paper attempts to provide answers are these:

1. To what extent can the persistently higher level of the unemployment rate for nonwhite than for white male workers be accounted for by differences between the two groups in their distributions by occupation, education, age, industry, region, and like characteristics?

2. Are the residual differences—those that remain after standardization for such differences as skill—related to other aspects of market discrimination against nonwhites that have been revealed in other studies [3] [29] [5] [10]?

The first of these questions is examined in Part I, the second in Part II. Part III is a summary of findings.

I. *Measurement of the Nonwhite-White Unemployment-Rate Differentials*[4]

The purpose of this section is to estimate the effect of the characteristic "color" on the level of unemployment rates. This is achieved through standardization, e.g., by removing that part of the difference in unemployment rates which is attributable to factors that make for high unemployment rates for whites as well as for nonwhites. For example, the aggregate data are adjusted for differences between white and nonwhite workers in their educational attainment, age composition, and occupational, industrial, and regional distributions.

The analysis is based mainly on unpublished unemployment data

greater proportion than are comparable white workers. But such comparisons merely show that the observed excess in nonwhite over white unemployment is independent of the level of business activity. For a report on several tests of the cyclical behavior of the nonwhite-white unemployment-rate differential, see Gilman [6] [7, Ch. 3].

[4] The term "differential" is used throughout this paper to denote the absolute difference (*NW-W*) in unemployment rates. The absolute rather than the relative differential (*NW/W*) is used because the former measure seems better able to detect differential hiring and firing. This comment has particular relevance for comparisons of unemployment rate differentials between two or more periods of time. If nonwhite workers had a higher unemployment rate than did white workers in some initial period, proportionate increases in the two rates would not have affected their ratios. However, under this assumption, the absolute increase in unemployment would have been greater for nonwhite than for white workers. At any rate, the conclusions reached in this paper would have been the same had I used the relative differentials instead. The patterns among the absolute differentials discussed below are observed also among the relative differentials [7, Tables 3, 5, and 8].

TABLE 1—UNEMPLOYMENT RATES BY COLOR FOR BOTH SEXES AND FOR MALES BY CLASS OF WORKER 1950 AND AVERAGE OF OCTOBER 1953–OCTOBER 1961
(in per cent)

Workers Covered	1950			October 1953–October 1961[a]		
	NW (1)	*W* (1)	*NW−W* (3)	*NW* (4)	*W* (5)	*NW−W* (6)
Both sexes	7.9	4.5	3.4	9.8	4.8	5.0
All males	7.8	4.7	3.1	10.2	4.6	5.6
Male, experienced labor force	7.7	4.6	3.1	9.4	4.2	5.2
Male, experienced wage and salary workers only				10.4	5.0	5.4

[a] This is an average of the January, April, July, and October observations for each year beginning October, 1953. The series has been adjusted for the change in the definition of unemployment that occurred in January of 1957. The adjustment was made by linking the two periods on the basis of the two January, 1957 observations.

Sources: The 1950 entries were computed from the 1950 U.S. Census of Population, Bulletin PC-1 *U.S. Summary,* Table 118, and Special Report, PE-1B, *Occupational Characteristics,* Table 3; the 1953–61 averages were computed from U.S. Census, *Current Population Reports,* Series P-50 and P-57, and from unpublished BLS tabulations, "CPS Tabulation Specifications (Univac I)," September, 1953–February, 1959, Tables 18 and 21; "CPS Univac 1105 Tabulation Specifications" (Beginning March, 1959), Table 23.

from the monthly sample for the Current Population Survey by the Bureau of the Census. The study also uses data from the 1940 and 1950 decennial censuses.[4]

Columns (3) and (6) of Table 1 show the excess of the nonwhite unemployment rates over those for white workers in 1950 and in the average for October, 1953-October, 1961. In both April, 1950, and in 1953-61, nonwhites had higher unemployment rates than whites, both in the aggregate and for males alone. This was also true in April of 1940, though the differentials have increased over time. The absolute differences in unemployment rates in April, 1940, were only 2.7 per cent and 2.9 per cent for both sexes and for males, respectively.

These positive differentials, however, are not surprising, given that nonwhites are concentrated in low-skill occupations which have a high incidence of unemployment for whites as well as for nonwhites. For instance, the percentage of nonwhite males in the unskilled or semiskilled major occupation groups is more than twice the percentage for white males (74.3 per cent to 35.7 per cent).

[4] The decennial census data (April, 1940 and April, 1950) are not strictly comparable to those for 1953-61 because of the change of definition in unemployment that occurred in January of 1957 and because of the difference in sources. For a discussion of the comparability of CPS and decennial census data see [19, p. xiii] [1, pp. 72-84] [21]. See also note 5.

TABLE 2—NONWHITE-WHITE MALE UNEMPLOYMENT DIFFERENTIALS, BY MAJOR OCCUPATION GROUPS FOR 1940, 1950, AND AVERAGE OF OCTOBER 1953–OCTOBER 1961

Major Occupation Group	1940	1950	October 1953-October 1961
	NW−W	*NW−W*	*NW−W*
Professional and kindred workers	7.5%	1.4%	1.9%
Managers and proprietors, exc. farm	1.3	1.4	1.4
Clerical and kindred workers	6.1	3.4	3.8
Sales workers	4.3	3.2	3.9
Craftsmen, foremen, and kindred workers	8.6	4.4	5.3
Operative and kindred workers	2.7	2.1	3.6
Private household workers	1.2	1.0	−0.5
Service workers, exc. private household	4.0	1.5	3.7
Farm laborers and foremen	−6.9	−0.8	1.9
Laborers. exc. farm and mine	−2.7	1.0	2.4

Sources: The 1940 entries were computed from the 1940 *U.S.* Census of Population, the Labor Force, *Usual Occupation*, Table 4; the 1950 entries were computed from the source given in Table 1; the entries for October, 1953–October, 1961 were computed from Tables 23 and 31 and from Table 24, respectively, of the unpublished sources given in Table 1.

The figures in Table 2 are the unemployment differentials for males by major occupation group, for 1940, 1950, and the average for the period October, 1953, to October, 1961 (four observations per year). In all periods, the differentials tend to be smaller for the unskilled or semiskilled than for the skilled or white-collar occupation groups. For laborers and domestics they are frequently negative. The differentials are consistently highest for craftsmen. The patterns among these differentials will be discussed in greater detail in Part II. It is important to note here that the average effect of standardization of the data by major occupation groups is to reduce the excess in the aggregate nonwhite male unemployment rate by roughly 100 per cent in 1940, by 42 per cent in 1950, and by about 38.5 per cent in the average figures for October, 1953, to October, 1961. Thus, in recent periods, the inequality in the occupational distributions (major occupations) between nonwhite and white male workers accounts for less than half of the difference in their unemployment rates.

However, nonwhite males have lower levels of educational attainment than white males within each of the major occupation groups [7, Tables 2 and 4]. For white males there is an inverse relationship between their educational attainment and their level of unemployment. This suggests that differences in education may partly account for the generally positive unemployment differentials within major occupations shown in Table 2. The correlations between the unemployment

differentials and the differences in educational attainment (see Table 3), however, are small.

Similarly, within major occupation groups, nonwhite males are concentrated in the detailed occupations that are relatively less skilled;

TABLE 3—NONWHITE-WHITE MALE UNEMPLOYMENT AND EDUCATION DIFFERENTIALS BY INTERMEDIATE OCCUPATION GROUPS FOR 1950 AND AVERAGE OF 1957–1961

Intermediate Occupation Group	Unemployment Differentials		Education
	Average 1957–1961	1950	1950
	NW-W	*NW-W*	*W-NW*
	(percentage points)	(percentage points)	(years)
Technical engineers	0.4	3.8	?[b]
Medical, salaried only	2.6	1.7[a]	?
Other professional, salaried only	2.5	1.7	2.3
Managers, salaried only	2.9	2.5	2.5
Stenos, secretaries, and typists	3.8	4.5	−0.2
Other clerical workers	4.3	3.4	0.2
Salesmen, retail trade	5.2	3.0	2.6
Salesmen, other trade	4.3	3.4	2.5
Machinists and metal craftsmen	4.6	3.1	2.1
Mechanics and repairmen	3.7	2.2	1.5
Construction craftsmen	6.0	3.8	2.1
Other craftsmen	4.1	8.3	1.2
Foremen	1.8	1.6	1.4
Drivers and deliverymen	3.4	1.8	1.8
Mine and durable-goods operatives	5.3	1.9	1.8
Nondurable-goods operatives	4.7	3.1	1.9
Nonmanufacturing operatives	2.3	2.1	1.9
Private household workers	1.8	1.0	1.7
Protective service workers	3.9	0.4	1.0
Waiters, cooks, and bartenders	0.6	0.8	0.7
Other service workers	4.4	1.0	0.5
Paid farm laborers	1.3	−0.9	3.4
Construction laborers	3.4	2.4	2.7
Manufacturing laborers	2.2	1.3	2.6
Other laborers	3.3	0.2	2.7

[a] The 1950 differentials for the medical and the other professional groups are not comparable to the average differentials for 1957–61. The 1950 figures will be smaller in part because they include self-employed.

[b] The median number of school years completed was 16+ for both white and nonwhite engineers and medical personnel.

Sources: The unemployment differentials for 1957–1961 and 1950 were computed from the sources given in Table 1. Education differentials were computed from the 1950 U.S. Census of Population, Special Report P.E. No. 1B, *Occupational Characteristics*, Tables 10 and 11.

occupations with high unemployment rates for whites as well as for nonwhites [7, Table 6].

Another factor that may be partly responsible for the excess nonwhite unemployment is that nonwhite males of given major occupations may be concentrated, more than white males, in industries with high levels of unemployment. Unfortunately employment or unemployment data are not available simultaneously by color, occupation, and industry, and thus this hypothesis cannot be fully tested. However, some of the occupation groups in the intermediate occupation classification are heavily concentrated in particular industry groups. Thus some standardization for the unequal distribution by skill and industry within major occupation groups can be achieved by comparing white and nonwhite male unemployment rates in the intermediate occupation groups. The unemployment differentials for these groups are reported in Table 3.[5]

The pattern of the differentials revealed here is similar to that for the major occupation groups: the differentials are positive but low for the unskilled, higher for the higher-skilled or white-collar workers, and then lower for professional workers. Standardization by intermediate occupation groups reduces the excess in the nonwhite male unemployment rate by roughly 58 per cent in April, 1950, and 45 per cent in 1957-61.

In an effort to achieve a greater degree of control over unemployment factors other than color, I have computed several multiple regressions involving these factors simultaneously. The regressions, which cover only male workers, were run separately for all occupations in the intermediate-occupation-groups classification, and within them for higher- and lower-skill occupation groupings. The dependent variable is the average unemployment rate (in per cent) for an occupation (U).[6] The independent variables are color (C), industry (I), education (E), per cent of workers between the age of 25 and 55 (A), and the per cent of wage and salary workers (WS).

The color variable is a "zero" or "one" dummy variable with "one" assigned to all nonwhite observations.

The industry variable is the "expected" unemployment rate for each occupation group. For each occupation group it was computed by weighting the average white male unemployment rates, Wi, in the 22

[5] The samples for the nonwhites in the high-skill occupations are very small. Data for 1950 are based on a sample of 3⅓ per cent of the population. The CPS data are based on a sample of 35,000 households. However, the CPS data presented here are averages of 20 observations: January, April, July, and October observations for the period 1957-61.

[6] It is an average for 1957-61.

major industry groups[7] by the percentage, P_i, of employees in the given occupation group who were in the industry groups. The "expected" unemployment rate for each occupation group on the basis of its industrial distribution is then:

$$\sum_{i=1}^{22} \frac{P_i W i}{100}.$$

A rise of 1 per cent in this variable should result in a rise of approximately 1 per cent in the occupational unemployment rate. Since there are no data available on the occupational distribution between industries by color, I have assigned to both white and nonwhite males the weights of their combined industrial distributions.

The preliminary findings suggest that for the intermediate occupation breakdown this procedure will not bias the results significantly.[8]

The education variable is the median number of school years completed by occupation group and color in 1950.[9] The age variable is the percentage, in 1950, of those employed in a given occupation group, by color, who were 25-55 years of age. The "wage and salary" variable is the percentage, in 1950, of those employed in a given occupation group, by color, who were wage and salary workers (rather than self-employed or unpaid family workers).

I expected negative signs for the coefficients of education (E), and age (A), and positive signs for those of industry (I), and "wage and salary" (WS). The size and sign of the "color" variable are of most interest, of course. The coefficient of this variable is an estimate of the average (over the occupational groups covered) absolute unemployment differential, NW-W, attributable to color.

The results of these regressions are reported in Table 4. All of them show a positive coefficient for color. The color coefficient across all occupations in the intermediate occupation classification is large (2.9 per cent) and statistically significant at the 1 per cent level or better. This coefficient, however, is the result of combining higher- and lower-skill occupation groups. This is shown in the results for regressions (2) and (3). For the higher-skill occupations, the color coefficient is even larger (3.17 per cent) and statistically significant at the 1 per cent level or

[7] The average industry unemployment rates are for periods that parallel those for the average occupation unemployment rates.

[8] See [7, Table 7].

[9] I computed several regressions in which I had introduced (full-time) income and education as proxies for "skill." Income for given years of schooling was meant to measure at least roughly the skill not accounted for by years of schooling. However, owing to the high degree of intercorrelation between these two variables, their statistical significance was low. Moreover, the inclusion of the income variable had little effect on the "color" coefficient or on R^2.

TABLE 4—MULTIPLE REGRESSIONS OF AVERAGE UNEMPLOYMENT RATES IN OCCUPATIONS ON COLOR AND LABOR DEMAND AND SUPPLY VARIABLES

Regression Number and Dependent Variable	Independent Variables						Number of Observations
	C	I	E	A	WS	R^2	
1) U_{io}	2.90 (.84)	1.21 (.19)	−.58 (.16)	−.14 (.05)	.083 (.078)	.725	50
2) U_{hs}	3.17 (.76)	.87 (.28)	−.33 (.16)	−.12 (.08)	−.04 (.08)	.762	26
3) U_{ls}	.84 (2.54)	1.20 (.29)	−1.16 (.83)	−.02 (.10)	.05 (.19)	.666	24

Symbols:

C = color variable, 0–1 shift, 1 for nonwhite observations
I = expected unemployment on the basis of the industry distribution (per cent)
E = number of school years completed (years)
A = per cent between the ages of 25 and 55 (per cent)
WS = per cent wage and salary employees (per cent)
U_{io} = unemployment rates in all occupations (per cent)
U_{hs} = unemployment rates in the occupations craftsmen through professionals (per cent)
U_{ls} = unemployment rates in low skill occupations, laborers through operatives (per cent)

Sources: Average unemployment rates were computed from the source quoted in Table 1. The occupational distribution among industries were computed from the 1950 U. S. Census of Population, Special Report, *Occupation by Industry*, Table 2. Age was computed from the 1950 U.S. Census of Population, Special Report P-E No. 1B, *Occupational Characteristics*, Tables 6 and 7. Per cent wage and salary employees was computed from the same source, Tables 12 and 13.

better, while for the unskilled or semiskilled groups it is substantially smaller (.84 per cent) and is statistically not significant at the 30 per cent level.[10]

The coefficients for the industry variable (I) are approximately unity[11] and are highly significant statistically.

The coefficients for the education (E) variable have the expected sign and except in the case of the regression across low-skill occupations they are statistically significant. The coefficients for the other variables—age and wage and salary distributions—generally have the expected signs; however, their statistical significance typically is low.

In summary: the available evidence indicates that much of the apparent excess in the nonwhite male aggregate unemployment rate is eliminated with standardization. For the averages for 1957-61, standardization by major occupation groups, intermediate occupation groups, and the regression across all intermediate occupations removed roughly 40, 45, and 50 per cent, respectively, of the *NW-W* male unemployment-rate differential for the United States. In earlier periods standardization reduced the male unemployment-rate differential by an even larger fraction. Thus, in 1950, standardization by major and intermediate occupation groups removed roughly 42 and 58 per cent of the differential, respectively, and in 1940 standardization by major occupation alone eliminated all of the gross unemployment differential in the U.S. average figures [13].

Regional standardization reveals that the inequality in occupational distribution is responsible for a larger fraction of the *NW-W* male unemployment-rate differential in the South than elsewhere. Thus, standardization by major occupations reduced the 1960-61 differential by roughly 68 and 43 per cent for the South and non-South respectively. However, in both regions much of the differential remains. Whether the residual differences are due to insufficient standardization for differences in skill or to some other causes is a question that is discussed in Part II of this paper.

II. *The Dispersion of Unemployment Differentials*

The amount of standardization achieved in Part I reduced the recent *NW-W* male unemployment-rate differentials by about 50, 43, and 68 per cent for the U.S. average figures, the non-South, and South,

[10] Relative to the mean unemployment rates for whites and nonwhites combined, the coefficient for the higher-skill occupations is 63.0 per cent, whereas for the lower-skill groups it is only 8.9 per cent.

[11] The difference between the observed and expected size (1.0) of this coefficient may be the result of (a) weighting the 1957-61 average unemployment rates with 1950 weights, and (b) using the same weights for white and nonwhite workers (see the discussion on the industry variable above.)

respectively. But these figures show the average effect of standardization across all major or intermediate occupation groups. We saw earlier (Tables 2 and 3 and regressions (2) and (3)) that standardization reduced the apparent excess in nonwhite unemployment in low-skill occupations by more than it did in the high-skill occupations. Also, standardization by major occupations had a greater impact on the unemployment differential in the South than in the non-South. This part of the paper examines the patterns among the unexplained differences in light of two alternative, though not necessarily competing, hypotheses about them.

A. *The Differential-Skill Hypothesis*

The first hypothesis examined is that the residual differences in *NW-W* male unemployment rates are due to incomplete statistical control over the skill factor. In particular, the residual differences within occupation groups (major or intermediate) may be due to differences in either the quantity or quality of education or to differences in the amounts of specific on-the-job training received by white and nonwhite workers [4] [10] [8]. This hypothesis implies, however, that the differences in unemployment rates should be directly related to differences in either of the above-named skill factors. It implies, for instance, the following: (1) Since differences in the quality of education are cumulative with respect to time spent in education, the *NW-W* unemployment differentials ought to be highest for groups in the highest-skill occupations or levels of education. (2) Since for each year of job experience nonwhites receive less on-the-job training than whites, we should observe a greater skill differential and hence a greater unemployment-rate differential for older than for younger groups of workers. (3) Since education differential (years of schooling in an occupation group for whites minus those for nonwhites) are greatest in the South, we should find higher unemployment-rate differentials in the South than in the non-South. (4) Since the educational differentials have declined over time, we should observe a secular decline in unemployment-rate differentials.

The observed patterns among the differentials will be discussed in the remaining paragraphs of this section. It is important to note here that except for some isolated observations the data contradict the hypothesis.

1. *Occupational and Educational Cross Sections*. Several pieces of evidence in support of the differential-skill hypothesis are, in my opinion, misleading because they are based on the pattern among the *NW-W* unemployment differentials across occupations or levels of education for the United States as a whole. For instance, the data examined

earlier show that increased standardization—by major occupation, by intermediate occupation, and the multiple regression across all intermediate occupations—reduced the 1957-61 male unemployment differentials for the United States by roughly 40, 45, and 50 per cent, respectively. It is possible that additional standardization, such as a multiple regression across all detailed occupations, would have reduced the differential by an even larger fraction.

The further reduction of the differential may be inferred from the low color coefficient in the regression (3) across the unskilled and semiskilled occupation groups. For it can be argued that had the regressions been run for a more detailed occupational breakdown, the color coefficient for the high-skill groups would have become as small as that for the low-skill groups. This argument is partly supported by evidence showing that within major occupation groups nonwhite males tend to be concentrated in detailed occupations that are relatively less skilled, and that this inequality in occupational distributions is greater, the higher the average of level of skill of the major occupation group [7, Table 2]. It can be assumed that intermediate occupation groups still have wide dispersions of skill and that the dispersions are wider within the higher-skill intermediate occupations than within the lower-skill intermediate occupations.

However, on the basis of the rising inequality in the occupational distributions of white and nonwhite males with increasing average levels of skill of occupations, we would expect the highest unemployment differentials for the highest-skill occupations. Instead we find (Tables 2 and 3) that the absolute unemployment differentials are consistently highest for craftsmen. Separate regressions for the upper- and middle-skill groups of the intermediate occupation classification would have revealed the same pattern; they would have revealed a larger color coefficient (than the present 3.17 per cent of regression (2)) for the middle groups and a lower one for the highest-skill groups. This is suggested by the residuals for the professional and managerial groups and for clerical, sales, and craftsmen groups separately. The nonwhite minus white residuals are negative (-2.02) for professional and managerial groups, but are positive (0.91) for the clerical, sales, and craftsmen groups.

For the United States data the inverted U-shaped patterns among the *NW-W* male unemployment-rate differentials are observed in 1940, 1950, and in the average figures for 1953-61 or 1957-61.[12] They are observed across major and intermediate occupations as well as across levels of education within occupations. Thus, in most major occupa-

[12] The patterns are somewhat weaker, but are present among the relative differentials as well. See also note 3.

tions, nonwhite males with low levels of education had (in April, 1940) lower unemployment rates than white males with similar education. The differentials generally became positive for groups with seven-to-eight years of education, increased for groups with one-to-three years of high school education, and declined for high school graduates and for groups with college education [7, Table 5].

A somewhat similar pattern was observed by Jacob Mincer in data for April, 1950, classified by age and education. Mincer observed that "Negro unemployment rates are higher in almost all classifications; the difference is negligible at the lowest education levels, and generally increases with education. The differentials remain positive, but decrease at the highest educational level" [8, p. 71].

Thus, even for the U.S. average figures, the differential-skill hypothesis may, in part, explain the rising unemployment differentials from the lowest-skill occupation groups through the craftsmen groups or from low to medium levels of education. It will not explain the lower differentials for the highest-skill groups or the highest-education levels.

Moreover, the rising unemployment differentials through the craftsmen groups are the result of combining the data for the South and non-South. This is apparent from the figures reported in Table 5, which are the 1960-61 average unemployment levels[13] and the differences in these levels, *NW-W,* separately for the South and non-South. These figures show that the unemployment differentials for the lower-skilled groups are small or negative for the South, but positive and substantial for the North and West. Indeed, there is a possibility that a separate regression for the North and West would show the opposite relationship between the level of skill and the size of the color coefficient.

The same conclusions can be drawn from the occupation data for April, 1940, and for data for April, 1950, by region and age-education classes [7, Tables 10 and 11]. For the lower-education groups, the differentials are small or negative for the South, but are positive and substantial in the non-South.

2. *Age Cross Sections.* The "differential skill hypothesis" is similarly deficient in explaining the behavior of the unemployment-rate differentials across age groups. The effect of differences in the amounts of on-the-job training received by whites and nonwhites of given levels of education should be to increase the unemployment-rate differentials with age. Instead Mincer's data show the opposite to be true [8, Table 7].

3. *Regional Cross Sections.* The regional data in Table 5 are sur-

[13] This is an average of eight observations.

TABLE 5—MALE UNEMPLOYMENT RATES BY COLOR, REGION, AND MAJOR OCCUPATION GROUPS, 1960–61

Major Occupation Group	Average 1960–61							
	South			Non-South			Non-South Minus South	Education
	(1) *W*	(2) *NW*	(3) (2)–(1)	(4) *W*	(5) *NW*	(6) (5)–(4)	(7) (6)–(3)	(8) *a*
Professional workers	1.4	2.2	0.8	1.9	4.6	2.7	1.9	b
Managers and proprietors, exc. farm	1.4	2.3	0.9	1.5	4.5	3.0	2.1	1.4
Clerical workers	2.9	6.3	3.4	4.0	9.4	5.4	2.0	1.2
Sales workers	3.2	8.6	5.4	2.9	5.5	2.6	−2.8	2.6
Craftsmen and foremen	6.1	10.2	4.1	5.5	10.5	5.0	0.9	1.5
Operatives	6.4	9.2	2.8	7.5	12.2	4.7	1.9	1.7
Service workers exc. private household	5.4	8.0	2.6	5.7	10.2	4.5	1.9	1.1
Farm laborers and foremen	6.9	6.3	−0.6	5.6	22.9	17.3	17.9	
Laborers, exc. farm and mine	12.7	12.9	0.2	12.9	21.2	8.3	8.1	0.7

[a] Median number of school years completed, *W-NW*, in the South minus the difference in these medians, *W-NW*, in the non-South.
[b] Both white and nonwhite workers had 16+ years of education.
Sources: Unemployment rates were computed from the unpublished sources quoted in Table 1. Median number of school years completed were computed from the 1950 U.S. Census of Population, Special Report, P-E, No. 5B, *Education*, Table 11.

prising in the light of the common conception of greater discrimination in the South than elsewhere. Column (7) of Table 5 shows the *NW-W* unemployment-differentials in the non-South minus the differentials in the South. All except one of the figures are positive, indicating consistently greater unemployment differentials, *NW-W*, in the non-South than in the South. The consistently greater unemployment differentials for the non-South than for the South are also revealed in the 1950 data classified by age and education, in 1940 data classified both by occupation and by age and education, and in 1940 data classified by city size.[14]

Even without taking account of regional differences in discrimination, we would expect greater unemployment differentials, by occupation, in the South than non-South. We expect this because both the education and income differentials [29, Table 16] are greater there than in the non-South and the regression coefficients for these two variables are negative. The absolute education differentials are reported in Column (8) of Table 5. The excess of white over nonwhite years of schooling is consistently greater in the South than in the non-South.[15]

4. *Trend.* The rising unemployment-rate differentials over time (see Tables 1, 2, and 3) also contradict the hypothesis. These have occurred, nothwithstanding the narrowing of the *W-NW* educational differentials within occupations.[16]

Thus, the patterns among the residual unemployment-rate differences described in the four preceding tests are inconsistent with the hypothesis that they are due to the incomplete control over the skill factor.

B. *The Differential-Wage-Rigidity Hypothesis*

The second hypothesis examined is that the residual differences in *NW-W* unemployment rates are due, at least in part, to the existence

[14] This last finding is based on unpublished unemployment data compiled by Rosanne Coale of the National Bureau of Economic Research.

[15] In a recent article on the decline in relative income of Negro men, A. B. Batchelder speculates [2, p. 538] that in the decade of the 1950's "the average quality of a year of school completed by Negroes living in the North and West was reduced." It is his guess that "the quality dilution exceeded quantitative gains for Negroes in the North and West." I would doubt that he would be equally willing to guess that the average difference in the quality of education is greater in the non-South than in the South. But if that were the case, we might observe higher unemployment-rate differences in the non-South, notwithstanding the greater difference in the number of school years completed in the South than in the non-South.

[16] Moreover, we observe over time a greater increase in expenditures for the instruction of nonwhite than of white students. For instance, between 1939 and 1954, the increase in expenditures, per pupil, in the South has been more than double for Negro than for white public elementary and secondary school students [26, Table 21].

of greater wage rigidity for nonwhite workers in the presence of discrimination against them. The implications of this hypothesis for the behavior of these differences among occupations or regions will be discussed after a brief review of the market forces that operate differentially against nonwhite workers in the United States.

The existence of economic discrimination against nonwhite workers need not lead to higher unemployment rates for nonwhite than for white workers. Under complete wage flexibility, for instance, the majority's preference for discrimination will result in higher wages for white than for nonwhite workers [3].

However, if there are legal or quasi-legal pressures towards nonwhite-white wage equality, discrimination may take the form of reducing the employment opportunities of nonwhite relative to white workers. Thus, we would expect a greater effect of such factors as statutory and union minimum wages on the employment opportunities of nonwhite than of white workers; employment opportunities should fall more for nonwhite than for white workers.

If unemployment differentials are related to the difference between equilibrium and actual wages, the excess of nonwhite over white unemployment will vary among occupation groups, education classes, or regions. Because of differences in degrees of discrimination against groups in different occupations or levels of education, the equilibrium wage ratio, NW/W, will be different for different levels of skill [3, pp. 71-74, 88-90, 124]. Similarly, the extent to which maxima are effectively imposed on discriminatory wage differentials will vary among occupation groups or regions, depending on such factors as the degree of applicability of legal minimum wages and the degree of unionization. Given a preference for discrimination, the greater the pressure in an occupation or region for nonwhite-white wage equality, the greater will be the gap between equilibrium and actual wages, and the greater will be the reduction in employment opportunities for nonwhite relative to white workers.

1. *Occupational Cross Sections.* The consistently lower unemployment-rate differentials (in both the South and non-South) for the highest-skill groups are probably the result of the greater ability to discriminate on the wage side for these groups than for other groups of workers. Clearly, statutory and union minimum wages are less applicable to the highest-skill groups. And, generally, there is more individual wage-bargaining for professional than, say, for craftsmen or factory workers.

2. *Regional Cross Sections.* These considerations are equally applicable to the differences in the nonwhite-white unemployment differentials between the South and non-South. Because of the lower

wages for nonwhite workers in the South, federal minimum-wage legislation and union-wage standardization, if equally applicable to the South and non-South, would tend to raise nonwhite wages more in the South than in the non-South. This effect by itself would tend to make the unemployment-rate differentials greater in the South than elsewhere.[17] However, the coverage of these minima is probably significantly smaller in the South. This is clearly true for the degree of unionization.[18] There is some evidence that it is true for federal statutory minima as well. Such minima do not apply to agricultural workers, and the unemployment-rate differential for these workers is very low or even negative in the South.[19]

Other low-skilled nonwhite workers are concentrated in noncovered industries, more so in the South than elsewhere. For instance, federal government minima are likely to apply, largely, to the mining, manufacturing, transportation, and utility industry groups. The percentages of the white and the nonwhite labor forces in these industry groups in the South are 31.8 and 27.7 per cent, respectively. The corresponding figures for the non-South are 41.4 and 42.4 per cent [19, Table 161]. Even within these industry groups the applicability of federal minima is related to firm size, and these are on the average smaller in the South than in the non-South.

Moreover, state and local legal minima are hardly present in the South, but are numerous in the non-South, particularly in the industrial states containing large fractions of nonwhites. Such minima are generally applicable to a broader range of industries than are the federal ones. Most important, however, may be the fact that social pressures for equal pay for equal work, in the context of the white-nonwhite comparison, are less great in the South than in the non-South.

All of the above-named factors suggest a greater ability to discriminate on the wage side in the South than elsewhere. We find partial support for this suggestion in income data for 1939 classified by color and region. In a study of white-nonwhite income differences in 1939, Morton Zeman finds that *NW/W* earnings ratios, by age and education,

[17] Indeed, most studies of the impact of minimum wages show a greater effect of such minima on employment in the South than in the non-South. However, these studies measured the regional employment effects in covered industries only. The effect of such minima on total employment in a region depends on the fraction of the total labor force in covered industries as well as on the magnitude of the change in wages effected by minimum wage legislation. The latter is also a function of the degree of compliance and enforcement of such legislation. BLS surveys indicate that the degree of compliance, at least, is significantly smaller in the South than in the non-South.

[18] See Leo Troy's estimates of regional differences in the extent of union organization [12, pp. 17-22].

[19] The positive differential for this occupation group in the non-South is based on an extremely small sample.

are about 20 per cent smaller in the South than in the non-South [29, Table 16]. Since the unemployment differentials are smaller in the South than in the non-South, the regional differences in the ratios of annual earnings understate the regional difference in wage ratios (*NW/W*). This understatement is greatest for the wage ratios of unskilled workers since the unemployment differential for these workers is small or negative in the South, but is substantial in the non-South (see columns 3, 6, and 7 of Table 5 above). The lower wage ratios (*NW/W*) in the South are probably due to a regional difference in the pressure toward nonwhite-white wage equality.[20] Thus, smaller unemployment-rate differentials in the South than non-South are consistent with the popular conception of greater discrimination in the South if the differences between equilibrium and actual wage ratios (*NW/W*) are smaller there than in the non-South.

3. *Trend.* It is not clear whether there has been an increase in differential-wage rigidity over time, but such an increase would tend to reduce the employment opportunities of nonwhites and widen the unemployment differential over time. Clearly there has been a secular increase in the activities of organizations dedicated to the elimination of discrimination.[21] If such activities do not parallel changes in tastes for discrimination, they will tend to make market adjustment more difficult; they may thus limit rather than increase the employment opportunities of nonwhites. Increased effectiveness or coverage of minimum wages or unions would tend to have the same effect.

The preceding pages have emphasized what I have called "differential wage rigidity" as a factor making for higher unemployment rates for nonwhite workers in the presence of discrimination. It may be that even in the absence of such differential-wage rigidity, discrimination would produce higher unemployment rates for nonwhite workers. For discrimination has the effect of restricting the range of employment opportunities more for nonwhite than for white workers. I would expect this, unless it is offset by increased knowledge to non-

[20] The lower wage ratios (*NW/W*) in the South than in the non-South are also consistent with greater skill differences between white and nonwhite workers or a greater degree of discrimination there than in the non-South. But if the differences in wage ratios are due to these two factors alone, we should also observe higher unemployment differentials in the South, not in the non-South.

[21] This would be the case if, and only if, such groups were more effective in getting equal pay than in getting equal employment opportunities. It might be argued that this has not been the case since civil rights groups have concerned themselves with equal employment opportunities. It is clear, however, that these groups have worked toward increasing the employment opportunities of nonwhite workers at the same rather than at lower wages than those for white workers. Concentration on employment opportunities may thus produce a greater effect on wages than on employment if wage discrimination—unequal pay for equal work—is the more apparent of the two forms of discrimination.

whites about jobs available to them, to create a nonwhite-white unemployment differential.

When discrimination is quite openly practiced, however, as it is in the South, information regarding the identity of employers who do hire nonwhite workers, at least in specified occupations, may be more widespread in the nonwhite labor force than in areas where discrimination, though practiced, is strongly frowned upon. This aspect of discrimination may also create regional differences in the nonwhite-white unemployment differential.

Another factor that may be involved is a higher fraction of recent migrants in the nonwhite labor force in the non-South than in the Southern nonwhite labor force.[22]

IV. *Summary*

This paper attempts to estimate the effect of the characteristic "color" on the level of unemployment rates. The characteristic "color" was given a narrow definition to separate differences in unemployment rates attributable to differences in levels of skill from the effects of discrimination on hiring and firing practices, given the level of skill. This study has not attempted to measure the total effects of discrimination on unemployment, for the lower levels of education or on-the-job training of nonwhite than of white male workers are probably themselves the results of discrimination.

With this limitation in mind, the degree of standardization achieved here removed roughly half of the difference between white and nonwhite unemployment rates that appears in the U.S. averages for these two groups in 1950 and 1957-61 (see Tables 2, 3, and 4). In the South the nonwhite-white unemployment differential appears to be small or even negative at both ends of the skill distribution, but is substantial in the middle levels of skill. In the non-South the excess of the nonwhite over the white unemployment rate by skill level declines as skill increases. However, the excess is consistently greater than in the South. These differences by skill and region in the unemployment differential also appear in the data for April, 1940. However, in that month standardization for skill accounted for all of the gross unemployment differential in the U.S. average figures.

[22] One must keep in mind, however, that unemployment is related to rural-urban population movements as well as to regional migration. In the non-South, the rural-urban flows take place largely within the white labor force. Also, the greater regional migration of nonwhites may be offset, at least in part, by equally greater nonwhite rural-urban flows in the South. Thus, the net effect of migration on the regional differences in the unemployment-rate differentials is likely to be small. Partial evidence for this proposition is provided by the results of Leonard Rapping's study of the effect of migration on the white-nonwhite unemployment differential [11].

The differences in the excess of nonwhite over white unemployment rates by skill level, region, and date (trend?) need further study. It may be that these differences are partly the result of incomplete control over the skill factor, including specific on-the-job training. I suspect, however, that a considerable part of the unexplained differentials is not due to such inadequate control, but rather is the result of differential-wage rigidity in the presence of discrimination against nonwhite workers. Such differential rigidity, as I pointed out in Part IIB, may produce moment-of-time difference in the nonwhite-white unemployment rates.

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FIRM SIZE, MARKET STRUCTURE, OPPORTUNITY, AND THE OUTPUT OF PATENTED INVENTIONS

By F. M. SCHERER*

During the past three decades hypotheses concerning the most favorable industrial environment for technological progress have proliferated like foliage in the Vietnam jungles.[1] Only recently have we begun to penetrate the rich theoretical growth with empirical insights. This paper contributes to the defoliation program, reporting on a statistical study of the relationships between inventive activity and technological opportunity, firm size, product-line diversification, and monopoly power.

The main sample consisted of 448 firms on *Fortune's* list of the 500 largest U.S. industrial corporations for the base year 1955.[2] Independent variables included three measures of firm size for 1955, profits for 1955 through 1960, liquid assets for 1955, an index of diversification, dummy variables differentiating industry and technology classes, and (for a special sample to be described later) four-firm concentration ratios. The principal dependent variable is the number of U.S. invention patents issued to the sampled firms in 1959 [33]. The year of patent issue is lagged four years from the base year 1955, whose inventive output is to be measured, because on the average nine months pass between the conception of an industrial invention and the filing of a patent application [17, p. 71] and because during the 1950's three and one-half years were required for the Patent Office to process an average application to the point of issue.

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[1] For surveys of the literature, see [7] [11] [13] [14].

[2] Fifteen firms on *Fortune's* 1955 list merged with other surviving firms between 1955 and 1959. Their data were consolidated, and so the sample includes 463 firms independent in 1955. Twelve firms were excluded because they were not primarily engaged in manufacturing; 16 because they could not be fitted into industries with sufficient observations; two because of inadequate data; two because they were liquidated by 1959; four because their merger records were too complex to unscramble; and one inadvertently.

I. *Patent Statistics as an Index of Inventive Output*

Unfortunately, no completely satisfactory index of inventive output is available to the researcher. A straight count of patents has two well-known limitations: (1) the propensity to patent an invention of given quality may vary from firm to firm and from industry to industry; and (2) the quality of the underlying inventions varies widely from patent to patent.

The most important countermeasure is to recognize the existence of these deficiencies, but other steps are also available. Interindustry differences in the propensity to patent will be analyzed through the use of dummy variables. Within a given major industry or field, interfirm differences in the propensity to patent can be viewed as a random disturbance which, unless correlated with some independent variable, imparts no bias to regression estimates of inventive output, only increasing the unexplained variance.

The underlying economic (or technological) significance of any sampled patent can also be interpreted as a random variable with some probability distribution. Here a more serious problem arises, however. Fragmentary data on the profitability of patents—one indicator of economic significance—reveal a distribution highly skewed toward the low private value side, with a very long tail into the high value side [18] [20]. A graphic test suggested the existence of a Pareto-type distribution of profits with an α coefficient of less than .5. Asymptotically such a distribution possesses neither a finite mean nor a finite variance, and so one cannot be sure that the mean economic value of any particular sample of patents converges (under the weak law of large numbers) toward the true population mean value if large enough samples are drawn [10]. This forces us to acknowledge that patent statistics are likely to measure run-of-the-mill industrial inventive output much more accurately than they reflect the occasional strategic inventions which open up new markets and new technologies. The latter must probably remain the domain of economic historians.[3]

Given the limitations of patent data, many of the hypotheses explored in this paper were also tested with respect to 1955 research and development employment (including supporting staff members) for a subsample of 352 firms on which data were available. One finding of this double-barreled attack deserves immediate mention. The number of patents received in 1959 by the 352 firms was (by cross-section

[3] In accepting this limitation, we in effect cut off the most valuable tail of the invention value distribution, eliminating the cause of the infinite mean and variance problem. This is analogous to limiting the number of plays in defining the value of a St. Petersburg game. See [3, pp. 228–37].

standards) fairly strongly correlated with the number of 1955 R&D employees, with an r^2 of .72.[4] More generally, as we shall see, the results of the analyses were similar whether patents or R&D employment was taken as the index of inventive activity.

II. *Demand-Pull and Technology-Push Hypotheses*

Let us consider now the relationship between patenting and the gross size of the 448 firms. Regression of the ith firm's patents P_i on its 1955 sales S_i (in billions of dollars) gave the equation:

$$(1) \qquad P_i = 10.65 + \underset{(4.09)}{73.81}\, S_i;$$

with r^2 of .422. (Standard errors of regression coefficients will be given in parentheses.) In comparable regressions, the r^2 was .431 when 1955 assets were the scale variable and .480 with 1955 employment as the scale measure. Although least suitable in terms of a crude goodness-of-fit test, 1955 sales will be used as the scale variable in most of the analyses which follow.[5] This choice is taken for three main reasons. First, the sales variable is most likely to be responsive to short-run changes in demand. Second, the sales variable is essentially neutral with respect to factor proportions—an important attribute, given the fact that more than three-fourths of U.S. industrial research and development is directed toward new products, as opposed to new internal production processes [6]. Third, interview studies show that sales are the principal scale variable considered in company research and development budget decisions.

The correlation between patenting and firm size can be interpreted, along lines suggested by Schmookler and Brownlee [26] and extended by Schmookler and Griliches [27], as reflecting the response of inven-

[4] An even higher r^2 of .89 was obtained in a correlation with patents and R&D employment of the 352 firms aggregated into 14 two- and three-digit industry groups. This gain in explained variance is undoubtedly due to the canceling out of special, perhaps transitory, factors influencing individual firm patenting. When the ratio of government R&D support for the various industries in 1956 was added as a second independent variable, R^2 increased to .94 in this aggregated correlation. The more government support an industry received, the less patenting it did relative to R&D employment, no doubt because exclusive rights cannot be retained for patents received in connection with government contracts. Both regression coefficients were statistically significant at the .01 level.

[5] The reasons why different scale variables give different results and a more complete argument for using sales are presented in [22]. The differences in correlation coefficients are due to peculiarities in the industry distribution of firms. Notably, when each industry assumes its own best-fitting slope and intercept, the over-all fit is closer with sales than with employment.

Ideally we should like to use value added rather than sales to eliminate any biases due to differences in vertical integration, but value-added data for a large sample of firms are not available.

tive output to the over-all pull of demand. Presumably, the greater the sales of a firm in any given market, the more incentive and resources the firm has to generate patentable inventions related to that market.

Nevertheless, systematic differences between industries not related to mere sales volume also affect corporate patenting in an important manner. This can be seen in the effect of estimating separate linear regressions for each of the 14 two- and three-digit industries covered by my study. The results are summarized in Table 1 and in the Appendix.[6] By letting each industry assume its own best-fitting slope and intercept, we are able to "explain" 84.7 per cent of the total variance in corporate patenting about its grand mean. This represents an incremental gain of 42.5 percentage points over the simple regression of patents on sales for 448 firms together.[7] Thus, interindustry differences accounted for almost exactly the same amount of variance as interfirm variations in sales volume.

These observed interindustry differences in patenting, holding firm sales constant, can have a number of underlying causes. Perhaps most important is a set of influences best described under the heading "technological opportunity." Technological opportunity in this context could relate partly to industry traditions or to demand conditions not manifested in mere sales volume, but it seems most likely to be associated with dynamic supply conditions dependent in turn upon the broad advance of scientific and technological knowledge. Thus, the high sales regression coefficients for the electrical and general chemicals industries are undoubtedly due mainly to the vigorous scientific climate of those fields, which has assured a continuous supply of new technical possibilities exploitable to satisfy existing or latent demands. In effect, science and technology exert a push on inventive output in these fields. On the other hand, common observation suggests that the low-coefficient paper, food products, and conventional textile and clothing industries face a much more limited (although not empty) barrel of new technological possibilities.

At least part of the observed interindustry differences in patent out-

[6] A further implication of Table 1 is worth noting. The slopes of the industry regression equations are correlated positively with the r^2 for those equations, with a rank correlation coefficient of .69. The higher an industry's average patent output per sales dollar is, the less variable patenting tends to be relative to size. My interpretation of this result is that in technically progressive fields like electrical equipment and general chemicals, technological competition forces firms to match each others' inventive efforts. But in unprogressive fields like textiles, food products, paper, and fertilizers, soap, and cosmetics, invention is only a business strategy option.

[7] See A-7 of the Appendix, which provides a step-by-step analysis of covariance. Actually, as Appendix step A-12 shows, most of the patenting variance can be explained with even fewer variables, and 20 of the slope and intercept terms allowed to vary in Table 1 contribute little incremental variance reduction.

TABLE 1—LINEAR REGRESSIONS OF PATENTING ON SALES, BY INDUSTRY

Industry	Intercept	Regression Coefficient	N	R^2	Total Patents
Food and tobacco products	− .4	+ 18.05 (2.04)	75	.52	366
Textiles and apparel	2.8	− .48 (7.00)	25	.00	70
Paper and allied products	4.5	+ 7.11 (6.14)	21	.07	120
General chemicals[a]	8.1	+262.48 (25.68)	41	.73	3,316
Misc. chemicals[b]	13.0	+ 19.33 (20.50)	14	.07	231
Petroleum	4.5	+ 81.10 (10.50)	30	.68	2,194
Rubber products	7.3	+ 52.32 (11.29)	8	.78	303
Stone, clay, and glass	−12.4	+200.92 (25.03)	19	.79	434
Primary metals	.4	+ 23.21 (2.50)	50	.64	486
Fabricated metal products and miscellaneous[c]	5.9	+ 61.86 (16.56)	31	.32	516
Machinery	6.1	+ 90.40 (12.58)	46	.54	967
Electrical equipment and communications	22.5	+311.06 (17.61)	35	.90	5,036
Transportation equipment, except aircraft	2.7	+ 59.72 (5.28)	30	.82	1,685
Aircraft and parts	6.8	+ 70.38 (22.39)	23	.32	739
All industries combined	10.7	+ 73.81 (4.09)	448	.42	16,463

[a] Includes S.I.C. 281, 282, and 283 (inorganic, organic, and drugs).
[b] Includes S.I.C. 284, 285, 287, and 289 (soap, paints, fertilizer, and misc.).
[c] Includes, in addition to fabricated metal products, ordnance, watches and clocks, optical equipment, and the S.I.C. 39 miscellaneous category.

put is nonetheless attributable to a different set of causes: systematic variations in the propensity to patent inventions actually made. It is well known, for example, that aircraft makers and other defense specialists seldom seek patent protection on inventions made under government contract, since they must give their principal customer either exclusive rights or at least a royalty-free license in any event [24, pp. 80-82]. Conversely, patent attorneys reported in interviews that in such fields as organic chemicals and petrochemicals, the ease of gaining differentiated market positions through "manipulated molecule" patents encourages profuse patenting. A crude indicator of differences in the propensity to patent is differences in average patent output per unit of engineering input. As Table 2 demonstrates, the average num-

ber of 1959 patents received per thousand 1955 research and development employees varied from a low of 27 in the rubber goods industry to a high of 150 in the machinery industry.[8]

TABLE 2—1959 PATENTS PER THOUSAND 1955 R&D EMPLOYEES FOR 352 FIRMS, BY PRINCIPAL INDUSTRY

Industry	Mean P/R&D	Number of Firms
Machinery	150.0	34
Stone, clay, and glass	134.6	18
Petroleum	119.5	21
Fabricated metal products and misc.	117.9	25
General chemicals	111.1	37
Primary metals	95.6	39
Electrical equipment and communications	84.5	31
Paper and allied products	84.2	18
Textiles and apparel	81.3	17
Transportation equipment, exc. aircraft	72.1	18
Food and tobacco products	61.8	55
Miscellaneous chemicals	55.1	14
Aircraft and parts	33.4	19
Rubber products	26.6	6
All industries	83.4	352

Further analysis suggests that these differences in the propensity to patent account for only a minority of all systematic interindustry differences in patenting. To see this, we assume that varying propensities to patent should show up as differences in patent outputs per unit of R&D input. As indicated in Section I, a simple regression of patent outputs on R&D employment inputs for 352 firms yielded an r^2 of .72. If slope (not intercept) dummy variables for 14 industries are introduced into this regression, permitting each industry to have its own input-output coefficient, the multiple R^2 rises to .84. This 12-percentage point gain in patenting variance explained affords a maximum estimate of the effect of interindustry differences in the propensity to patent.[9]

We may consequently partition the over-all variance in corporate

[8] The extremes of this distribution deserve further comment. If we delete the 3,919 R&D employees of Goodyear Aircraft Company, a subsidiary specializing in defense work which accounted for 34 per cent of all rubber industry R&D personnel, the industry's *P/R&D* ratio rises to 40.5. Other rubber companies were also deeply involved in defense work. At the other extreme, I suspect that the craft-oriented machinery industry obtained many patented inventions from employees not in formal R&D organizations.

[9] It is a maximum estimate because other factors such as the type of R&D done also affect patenting per R&D employment input. For instance, the auto industry's preoccupation with styling change may lead to a low output of patentable inventions per engineering man-year.

patenting as follows: 42 per cent to interfirm differences in sales; less than 12 per cent to interindustry differences in the propensity to patent; more than 30 per cent to interindustry differences in technological opportunity, broadly defined; and 16 per cent as (thus far) unexplained residual. Even if this 12-30 split of the 42.5 percentage point interindustry component is inaccurate by a wide margin, it is impossible to avoid a conclusion that interindustry differences unrelated to mere sales volume account for a major proportion of the variance in corporate patenting.

III. *Corporate Bigness and Inventive Output*

We have seen that patenting is an increasing function of firm size, with or without technological opportunity taken into account. We turn next to a question more in the Schumpeterian vein: Does patenting increase more than proportionately with firm size, less than proportionately, or is the relationship essentially linear? Disciples of Schumpeter argue that inventive output ought to increase more than proportionately with firm size due to the scale economies and more effective incentives associated with bigness. Others have postulated the opposite relationship, pointing mainly to the stultifying effects of bigness on incentives and initiative.

Tests of the neo-Schumpeterian scale hypothesis encounter a number of methodological problems. One is that the results depend to some extent on the choice of a scale variable. For reasons analyzed in [22], the neo-Schumpeterian bigness contention receives greatest support when total employment is chosen as the scale measure and least when assets are chosen. Here the sales variable is emphasized partly because it compromises the extremes, but chiefly because it is considered most frequently in actual R&D budget decisions and because it is neutral with respect to factor proportions.

The relationship between sales and patenting or R&D employment can be studied by computing concentration measures or through nonlinear regression methods. The concentration approach is less elegant but more lucid, so let us use it first. Table 3 presents cumulative sales, patenting, and R&D employment shares for selected groups from my subsample of 352 firms on which fully comparable data were available. Firms are ranked on the basis of 1955 sales for all cumulations. Two findings stand out. First, sales volume is persistently more concentrated among the largest firms than R&D employment, which in turn tends to be slightly more concentrated than patenting. The top 20 firms by sales, for example, made 41.5 per cent of the sales of all 352 firms, but employed only 36.7 per cent of the R&D personnel and received only 32.9 per cent of the patents. Second, it follows by subtraction that

smaller firms were responsible for a higher relative share of inventive activity than sales. The implication is that inventive inputs (R&D employment) and outputs (patents) increase less than proportionately with sales.

TABLE 3—CONCENTRATION OF SALES, PATENTS, R&D EMPLOYMENT, AND TOTAL EMPLOYMENT IN A SAMPLE OF 352 CORPORATIONS

Number of Firms Included, Ranked by 1955 Sales	Percentage of Total for All 352 Firms			
	1955 Sales	1959 Patents	1955 R&D Employment	1955 Total Employment
First 4	19.9	10.4	9.7	16.1
First 8	27.5	16.8	16.4	24.1
First 12	32.8	24.9	25.9	28.3
First 20	41.5	32.9	36.7	35.0
First 30	49.0	42.9	44.7	42.4
First 40	55.0	45.0	50.4	48.8
First 50	59.9	50.8	57.8	55.1
First 75	69.3	64.5	68.1	63.9
First 100	75.9	71.0	71.9	71.9
First 150	84.9	81.3	81.7	81.8
First 200	90.8	89.4	90.0	88.9
First 250	94.9	94.2	94.8	93.5
First 300	97.7	97.6	97.8	97.2
All 352	100.0	100.0	100.0	100.00

The last column of Table 3 gives cumulative total employment for the 352 companies, again ranked by sales volume. It illustrates the earlier suggestion that a somewhat different result is obtained when employment is taken as the scale variable, R&D employment in particular being more concentrated than total employment for firms ranked below the 12 sales leaders.

Further insight into the role of size can be gained by introducing outside data. The 448 firms in my full sample accounted for 56.9 per cent of the sales of all U.S. manufacturing corporations in 1955, as estimated by the Federal Trade Commission. They accounted for 55.1 per cent of the 29,888 patents issued to U.S. corporations in 1955.[10] These figures are not quite comparable, since the population of all U.S. corporations includes nonmanufacturing corporations. But the latter do very little patenting. It is estimated that nonmanufacturing corporations received no more than 600 patents in 1959.[11] Thus manufactur-

[10] This figure was provided by P. J. Federico, examiner-in-chief, U.S. Patent Office.

[11] Total patents issued in 1959 to the 150 corporations on *Fortune's* 1955 lists of leading retailers, transportation firms, and public utilities amounted to 55. The sampled firms accounted for about one-fifth of all nonmanufacturing corporate sales, suggesting an

ing corporations excluded from my sample—mostly firms with sales of less than $55 million in 1955—accounted for approximately 43.1 per cent of manufacturing sales and 43.9 per cent of patents issued to manufacturing corporations.

This interpolation is a bit surprising, in view of National Science Foundation data showing that industrial firms with fewer than 5,000 employees—a population closely analogous to the population of firms excluded from my sample[12]—accounted for a much smaller share of U.S. research and development expenditures (about 14 per cent) than their share of sales and employment [32, pp. 23-24]. At least three explanations for the disparity between inventive input and output shares among small firms seem relevant. First, government-contract R&D spending, which leads to few patents, is much more concentrated among large firms than private R&D expenditures. Second, small firms apparently obtain a higher proportion of their patented inventions from employees not assigned to formal R&D activities [21]. Finally, there is weak evidence that small firms conduct their R&D with greater cost-consciousness than large firms, and therefore they may tend to generate more inventions per unit of input [2]. In any event, the data suggest that smallness is not necessarily an impediment to the creation of patentable inventions and may well be an advantage.

Nonlinear Regression Analyses

For a more detailed analysis of the relationship between patenting and sales, nonlinear regression methods were employed. If, when P_i is regressed nonlinearly on S_i, the second derivative d^2P/dS^2 of the estimated function is positive, P_i is increasing at an increasing rate with S_i, and so patenting must generally be increasing more than proportionately with sales.[13] A negative second derivative implies the opposite relationship. For expositional convenience let us call the positive d^2P/dS^2 case the "increasing returns" case and the negative d^2P/dS^2 case

estimate of 300 patents for all such corporations. A count based upon several published lists indicated that 153 additional patents were issued to universities, nonprofit research institutions, and nonprofit patent management corporations. Doubling this last figure and adding the two estimates leads to the over-all estimate of roughly 600 patents issued to all nonmanufacturing corporations. This is consistent with National Science Foundation data showing that nonmanufacturing firms do 2 per cent of all industrial R&D [32, p. 19].

[12] Out of the 448 firms sampled, 341 had 5,000 or more employees in 1955, and only three had fewer than 1,000 employees. The firms with 5,000 or more employees had 95.8 per cent of all sample employees and 97.0 per cent of all sample patents.

[13] The last statement is strictly true only when the intercept term is equal to zero. In fact, all the intercept terms for regressions discussed in this section were insignificantly different from zero. The two statements are also not strictly consistent in the neighborhood of inflection points.

the "diminishing returns" case, taking care not to confuse this use of terms with the somewhat different usage accepted in the standard theory of production.

Considering first all 448 firms together, P_i was estimated as a function of S_i with squared and cubic S_i terms:

$$P_i = -3.79 + \underset{(14.44)}{144.42\, S_i} - \underset{(4.83)}{23.86\, S_i^2} + \underset{(.316)}{1.457\, S_i^3}; \tag{2}$$

with R^2 of .454. This is the nonlinear analogue of equation (1). Introduction of the two nonlinear terms permitted a statistically significant incremental variance reduction of 3.22 percentage points (see the Appendix). When the second derivative of (2) is taken, we find diminishing returns dominating up to a point of inflection at 1955 sales of $5.5 billion, after which increasing returns set in. Since only three firms in the sample of 448 (General Motors, Standard Oil of New Jersey, and Ford) had 1955 sales greater than $5.5 billion, and since the cubic (increasing returns) coefficient is of doubtful statistical significance,[14] the indication is that patent outputs generally increase less than proportionately with increases in sales among corporations large enough to appear on *Fortune's* 1955 list.

To determine whether this result was influenced by the industry distribution of firms, separate regressions of P_i on S_i with nonlinear terms were estimated for each industry with 19 or more observations. In 10 out of 12 industries, the equations were of the same form as (2), with negative squared-term coefficients and positive cubic-term coefficients. In the two nonconforming cases (paper and aircraft) diminishing returns set in with firms of intermediate size and continued all the way out to the largest firms. However, due to the small number of observations for many industries and the high intercorrelations among S_i, S_i^2, and S_i^3 terms, most of the nonlinear coefficients were not statistically significant by conventional standards.[15] Some regressions with few observations were also dominated by one or two extreme values.

[14] The standard error estimates in equations (1) and (2) are biased downward because of heteroscedasticity. When equation (2) was recomputed, letting each centered observation be weighted by the inverse of the standard error of estimate for firms of its size, the squared term's coefficient was significant in a two-tail test at the .09 level and the cubic coefficient at only the .25 level. The heteroscedasticity problem was not nearly as severe for the individual industry regressions, since standard errors of estimate were typically smaller and less correlated with firm size.

[15] Intercorrelation coefficients among the three sales variables ran from .930 to .995, causing the standard errors of all coefficients to explode. For example, the unsquared sales term's t-ratios fell by an average of 70 per cent from the values obtained for the regressions in Table 1. This is a common result of multicollinearity which cannot be avoided without getting into even more serious specification problems discussed in [22].

To combat these statistical problems while taking into account differences in technological opportunity, the original list of 14 industries was consolidated into four groups, depending upon the linear regression coefficients reported in Table 1. The group with coefficients from 0 to 23, including food, textiles, paper, miscellaneous chemicals, and primary metals, will be called the unprogressives. The group with coefficients from 52 to 90 will be called the moderates. Stone, clay, and glass was combined with general chemicals to form a third group, while the electrical industry stood alone in the fourth group. Separate regressions of patents on sales for these four groups explained 83.6 per cent of the total variance in patenting about its grand mean, closely approaching the 84.7 per cent reduction achieved with the 14 separate industry regressions of Table 1. This indicates that the four-group classification was able to allow for the most significant interindustry differences in patenting relative to sales.

Cubic regression equations estimated for the four groups were as follows:

(3) ELECTRICAL
$$P_i = -21.9 + \underset{(98.16)}{582.87\, S_i} - \underset{(95.88)}{177.41\, S_i^2} + \underset{(22.13)}{26.67\, S_i^3}; \quad R^2 = .94;$$

(4) CHEMICAL
$$P_i = -23.1 + \underset{(131.45)}{432.04\, S_i} - \underset{(221.07)}{203.85\, S_i^2} + \underset{(85.59)}{58.42\, S_i^3}; \quad R^2 = .74;$$

(5) MODERATES
$$P_i = 5.7 + \underset{(12.98)}{82.85\, S_i} - \underset{(3.78)}{10.63\, S_i^2} + \underset{(.235)}{.789\, S_i^3}; \quad R^2 = .77;$$

(6) UNPROGRESSIVES
$$P_i = 1.9 + \underset{(6.53)}{21.04\, S_i} - \underset{(5.61)}{10.18\, S_i^2} + \underset{(1.06)}{3.00\, S_i^3}; \quad R^2 = .55.$$

However, as Appendix steps A-9 and A-10 show, the introduction of the 12 squared and 12 cubic sales variables led to variance reductions each significant in F-ratio tests at the 1 per cent point.

All four equations, plotted in Figure 1, display the initially dimin ishing and then increasing returns *pattern of equation* (2).[16] Despit multicollinearity, three of the four squared (diminishing returns terms' coefficients are significant at the .10 level or higher. In the in creasing returns stage dominated by the positive cubic coefficients, th unprogressive group included five large firms, the moderates three chemicals and stone-clay-glass two, and the electrical industry onl one. Thus, the indication again is one of diminishing returns except fo a few giant firms which led their two-digit industries in sales.

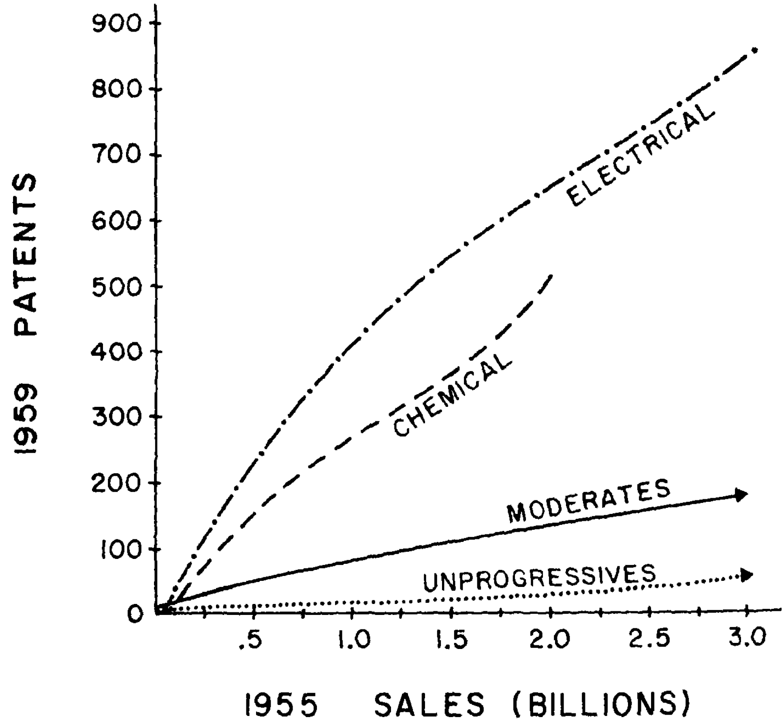


FIGURE 1

All of the regression equations discussed thus far are dominated to some extent by the observations for large firms. This is not wholly undesirable, since the largest firms account for such a substantial proportion of the full sample's sales and presumably had the market penetration and resources to support a similar lion's share of inventive activi-

[16] An alternative way of interpreting the figures is to sweep a straight line from the origin to points on the plotted curves. The slope of the line intersecting any point shows estimated patenting per billion dollars of sales. It is readily seen that P/S generally attains a maximum at sales levels below $500 million and a minimum at higher sales levels.

The upward bending part of the moderates' curve, including only General Motors, Jersey Standard, and Ford, is excluded from Figure 1 to maintain a larger drafting scale.

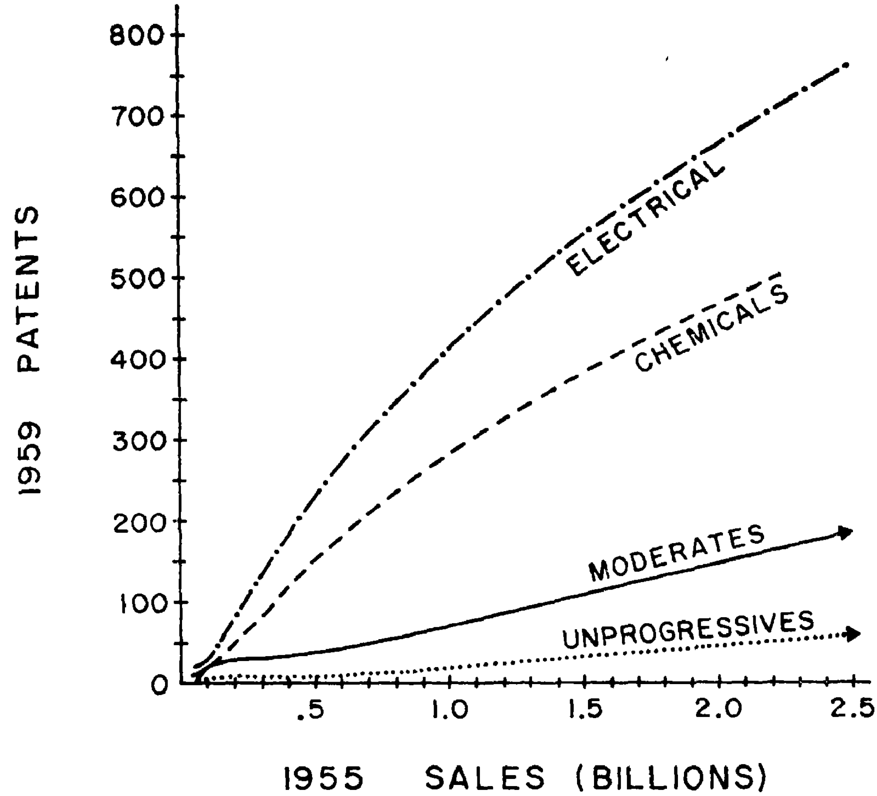


FIGURE 2

ty. Still for the sake of accuracy two further analyses were executed which either avoided large firm biases or erred in the opposite direction.

First, along lines suggested by Edwin Mansfield [11], logarithms of the sales variables were taken to compress the large-firm tail of the sales distribution, and then P_i was regressed on log S_i, $(\log S_i)^2$ and $(\log S_i)^3$ for each of the four groups. Because the equations are almost impossible to interpret intuitively, the results are presented graphically in Figure 2.[17] Diminishing returns in patenting are evident for both chemicals and electrical equipment—the most progressive groups. For the unprogressives there is a tendency toward increasing returns

[17] For the unprogressive and moderate groups, all three regression coefficients were statistically significant at the .01 level. None of the chemical and electrical regression coefficients was significant at even the .20 level, even though the three coefficients together permitted reductions in variance significant in F-ratio tests at well beyond the 1 per cent point. The latter result was again due to extreme collinearity among the three independent variables, with intercorrelation coefficients running between .980 and .996. Also, because of the logarithmic transformations, the more concavely nonlinear the functions appear when plotted on arithmetic grids, the more the regressions are likely to be linear on semi-log grids.

up to sales of about $1 billion, and then initially imperceptible diminishing returns set in. This relationship, however, is probably due in part to the greater diversification of the largest firms in unprogressive fields, as discussed in Section IV.[18] For the moderates slightly increasing returns prevail up to sales of $800 million, at which point a faint diminishing-returns effect begins to dominate.

Second, each group was broken down into from three to five size subgroups, and separate linear regressions were estimated for each subgroup. The results are shown in Figure 3. Although many of the coefficients were not statistically significant, the diverse pieces splice together well enough to suggest that something more than random phenomena was at work. For both chemicals (with stone, clay, and glass) and electrical equipment, there may be slightly increasing returns up to sales of roughly $500 million, but beyond that point a definite flattening out of the patenting function is evident. For the moderates very slightly decreasing returns appear to prevail continuously. The pattern for the unprogressives is so irregular that no conclusions can be drawn.

By way of synthesis, two possible interpretations can be attached to the full set of regression results, depending upon one's attitude toward statistical significance tests generally and the disutility of Type I as opposed to Type II inferential errors. Where significance is doubtful by traditional standards, one may incline toward the Scotch verdict that corporate patenting has not been shown to increase either more or less than proportionately with sales. But if the regressions are accepted as best estimates of some true behavioral pattern, it would appear that after a stage of slightly increasing returns extending to 1955 sales of approximately $500 million, corporate patenting tends to increase less than proportionately with sales, except in the case of a few giant firms which lead their two-digit sectors in sales. The least vigorous patent recipients relative to their size appear to be non-leader firms with sales over $500 million.

Conditions Underlying the Observed Relationships

The relevance of these observed relationships might be questioned if one could show that the propensity to patent is inversely correlated with firm size. It is conceivable that large firms receive proportionately fewer patents than smaller firms, not because they generate fewer inventions, but because they patent a smaller proportion of what they invent.[19] This could be so, for instance, because their inventive output

[18] Notably, in a regression of unprogressive group patents on S_i, S_i^2, S_i^3, and a diversification index, the diversification variable had by far the highest t-ratio and (positive) partial correlation coefficient and raised R^2 to .594 from the value of .545 for equation (6).

[19] It is also possible that large firms tackle more complex problems than small firms, and so their patents reflect greater inventive input. The former statement is probably

includes a higher fraction of unpatentable contributions to pure knowledge, or because patents afford less marginal benefit than they do for smaller and more vulnerable firms, or because government antitrust policies have been hostile to the exploitation of patent power by large firms [24]. But arguments for the opposite conclusion are also compelling. Notably, restrictive power may grow more than proportionately with the accumulation of patents, and for large firms with a staff of in-house patent attorneys, the short-run marginal cost of patenting may be less than for smaller firms dependent upon outside counsel.[20]

It is impossible to choose between these arguments on a priori grounds, so we must resort to two slender reeds of data. First, an intensive questionnaire survey of more than 500 randomly selected patents issued in 1938, 1948, and 1952 showed no tendency for the proportion of inventions actually produced or used in production to vary

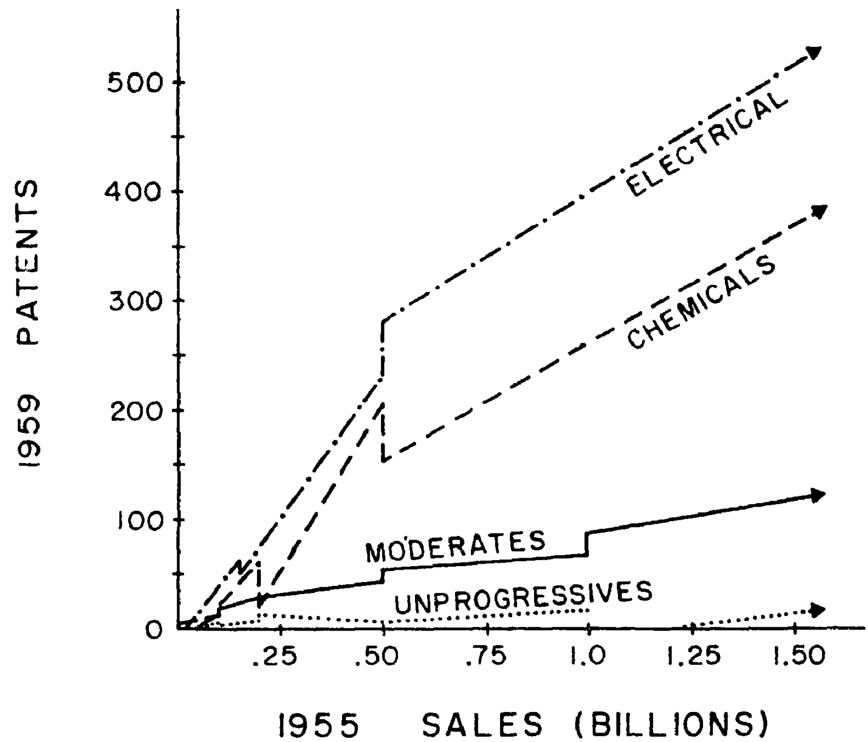


FIGURE 3

valid, but the latter is doubtful. Complex "system inventions" are usually broken down into many elementary parts for purposes of patenting, and I see no reason to expect the quality of these elementary inventions to vary with firm size. For example, RCA has well over a thousand patents on color television transmitting and receiving devices.

[20] Patents assigned in 1959 to the 448 firms in my sample were correlated with an incomplete count of in-house patent attorneys derived from the Martindale and Hubbell 1955 law directory, with a resultant r^2 of .78. Although 365 of the 448 firms received patents in 1959, only 161 were found to employ in-house patent attorneys.

with firm size for firms with 1949 sales exceeding $55 million [18]. There was, however, evidence that the percentage of patents in *current* use varied inversely with firm size.[21] One would expect firms which patent profusely to have a lower utilization rate than firms which patent only significant inventions, and so the survey results imply either a constant or (for current utilization) an increasing propensity to patent as firm size increases. Second, and in partial conflict with the survey results, the data underlying Table 3 show that the 40 largest corporations ranked by sales received an average of 74.7 patents per 1,000 R&D employees, compared to the over-all average of 83.4 patents for 352 firms (as in Table 2). Similar results are found for the 20 largest, 30 largest, and 50 largest. On the other hand, industry leaders Swift, New Jersey Standard Oil, U. S. Steel, Pittsburgh Plate Glass, International Harvester, and General Electric each received more than 120 patents per 1,000 R&D employees. These findings suggest that marked differences in the propensity to patent among the largest firms may in part explain both the relatively poor showing of large firms which did not lead their two-digit sectors in sales and the increasing returns associated with leading firms.

Variations in the propensity to patent clearly do not tell the whole story, however. Nonlinear regressions of R&D employment on sales presented in [22] generally paralleled those estimated here for patenting on sales. A global regression analogous to equation (2) had coefficients with the same signs as (2), but the nonlinearities were more pronounced. Cubic regressions for seven industries with 20 or more observations and grouped regressions similar to equations (3) through (6) revealed a general tendency toward diminishing returns, although the chemicals industry was an exception with R&D employment increasing more than proportionately with sales.[22] There was somewhat less evidence of an upturn in R&D employment for industry sales leaders (consistent with their high ratio of patents to R&D employment) and somewhat more evidence of increasing returns for firms with sales under $500 million.

Thus, one is led to believe that the diminishing returns observed in connection with patenting are related at least in part to similar tendencies in research and development employment, especially for firms with sales exceeding $500 million in 1955. It is not completely clear why this is so. My guess is that a firm's optimal competitive strategy tends to vary systematically but not monotonically with both absolute

[21] This may be the more relevant result due to lower response error and because of its closer proximity in time to my sample. The survey also showed that the largest corporations had a somewhat shorter mean duration of utilization for their patents.

[22] Recognition of this exception brings my results into full agreement with Mansfield's findings concerning major inventions, major innovations, and R&D spending in the petroleum, steel, chemicals, and glass industries [11] [12].

and relative size. Relatively small firms have strong incentives to do product-differentiating R&D and to patent the results in order to secure their positions against large rivals with economies of scale in promotion, distribution, and perhaps production. Giant firms in positions of industry sales leadership *may* also have effective incentives to support R&D—possibly on prestige grounds, or to guard against the hazard of organizational stagnation and decline, or because they inherited a research orientation which was partly responsible for their growth. At the same time such firms also tend to have a high propensity to patent. Large firms which do not lead their two-digit sectors in sales, on the other hand, appear to be least active relative to their size in both R&D employment and patenting, perhaps because they find it advantageous to focus their main attention on promotional activities and production, counting upon rapid imitation or licensing of others' inventions to guard against technological displacement.

Two further observations on the relationship between R&D inputs and patent outputs must be recorded. First, there was no evidence that patenting increased either more or less than proportionately with R&D employment. A regression of P_i on $R\&D_i$ with quadratic and cubic terms was very nearly linear. Second, there was some indication of diminishing returns to R&D input intensity, as suggested in the following regression, where S_i is scaled in billions of dollars and $R\&D_i$ in thousands of employees:

$$(7) \qquad P_i/S_i = (14 \text{ constants}) - \underset{(7.92)}{12.54}\, S_i + \underset{(7.87)}{61.33}\, R\&D_i/S_i - \underset{(.51)}{2.84}\, (R\&D_i/S_i)^2,$$

with R^2 of .54. The last two terms are of primary interest here. The more R&D employees per million dollars of sales a firm retained, the more patents per billion dollars of sales it received, but with diminishing returns. The function attains a maximum at 16.7 R&D employees per million dollars of sales—an input intensity exceeded only by the Raytheon Corporation in 1955. It would appear that for firms with sales under $500 million, which tended to have the highest ratios of R&D employees to sales, patenting was damped by the intensity effect of equation (7), and so some of the increasing returns found in R&D employment did not show up in patent outputs. The diminishing returns relationship of equation (7) is in turn probably due to organizational problems associated with high R&D input intensity, although the precise mechanism of this phenomenon is not clear.[23]

[23] Also involved are some peculiarities in the distribution of propensities to patent. The six firms with the highest *R&D/S* ratios in 1955 were all predominately engaged in defense contracting, and so they undoubtedly had low propensities to patent. Ideally, the

In conclusion, the evidence does not support the hypothesis that corporate bigness is especially favorable to high inventive output. If anything, the results show that firms below the half-billion dollar sales mark generate more inventions relative to their size than do giant firms. Such a conclusion is not necessarily grounds for a campaign of breaking up large firms, since the observed tendencies are less than completely uniform. It is also possible that large size does confer advantages for the development and integration of complicated "systems"—activities less likely to yield patentable inventions. Small firms at the same time may enjoy a comparative advantage at inventing and developing the more readily patentable component parts for such systems. My results do suggest, however, that a heavy burden of proof must be sustained by firms emphasizing research and development potential as a justification (i.e., in merger cases) for bigness.

IV. *The Role of Diversification*

One of the most interesting neo-Schumpeterian hypotheses assigns a potentially important role to diversification as a stimulus to invention. The idea, as formulated by Richard Nelson [15], is as follows: Research, and especially basic research, is an uncertain activity, yielding inventions and discoveries in unexpected areas. The firm with interests in a diversity of fields will generally be able to produce and market a higher proportion of these unexpected inventions than a firm whose product line is narrow. Therefore, the expected profitability of speculative research is greater for highly diversified firms, and such firms will tend to support more research.

Even though patentable inventions are undoubtedly not the leading product of basic industrial research, uncertainty is an important component of all research, and so the diversification hypothesis seemed worth testing. The usual list of 447 S.I.C. four-digit manufacturing industries was consolidated into a more technologically meaningful list of roughly 200 industries. A diversification index was compiled for each of the 448 companies sampled by counting the number of consolidated industries in which the company operated.[24] The index varied from 43 (for General Electric) down to 1, with a mean of 5.7.

industry dummy variables should have compensated for this, but five of the six firms were defense specialists from the electrical industry, which on the whole was much less defense-oriented. Still when equation (7) was estimated for six separate industries (chemicals, food, petroleum, metals, machinery, and electrical), the diminishing returns phenomenon appeared for all but the chemicals industry.

[24] My index is analogous to, but because of consolidation not the same as, one of the indices developed for a smaller number of companies by M. Gort [5, pp. 155-57]. Our samples overlapped for 88 companies. The r^2 when his indices were correlated with mine was .71. An industry-by-industry comparison of his indices with mine yielded an average Spearman rank correlation coefficient of .83.

The results of the global 448-firm analysis were striking. Although the diversification index was correlated with sales, with r^2 of .20, it was much more strongly correlated with patents, with r^2 of .37. When the index was added to a regression of patents on sales, the variance in patenting was reduced by 13 percentage points. Similar results were obtained when the diversification variable was introduced into regressions of P_i/S_i on sales, $R\&D_i$ on sales, and P_i on $R\&D_i$. In every case the diversification variable permitted a significant reduction in unexplained variance, and the t-ratio of its regression coefficient was on the order of 5.0 or more.

The picture changed considerably when each of the 14 two- and three-digit industry groups was permitted to assume its own best-fitting sales and diversification coefficients. For all industries summed, inclusion of the diversification variable accounted for an incremental variance reduction of 1.05 percentage points beyond the contribution of the simple linear sales regressions in Table 1—an increment just significant at the 1 per cent point in an F-ratio test.[25] This decline in explanatory power suggests that the diversification index acted partly as a surrogate industry dummy variable, and that two- or three-digit industry groups with high patenting tended to be the home base of firms operating in a greater number of narrow fields, as defined in my consolidation procedure.

Within the two-digit groups, diversification played an uneven role. For five industries, including the three (electrical; general chemical; and stone, clay, and glass) with the highest patenting per billion dollars of sales (Table 1), the diversification coefficients were negative but statistically insignificant. For nine industries, patenting was positively correlated with diversification, in most cases with t-ratios exceeding 2.0; and the diversification variable accounted for an appreciable reduction in variance not explained by the simple regressions of Table 1. These included, with the incremental variance reduction percentage in parentheses, textiles and apparel (33), paper (26), aircraft (26), rubber (12), miscellaneous chemicals (10), petroleum (5), food products (4), machinery (4), and land transportation equipment (1). With the exception of machinery, petroleum, and perhaps aircraft, these were for the most part industries whose members obtained few patents per billion dollars of sales.

A similar result was experienced when the diversification variable was introduced into regressions of R&D employment on sales for seven

[25] See Appendix steps A-8 and A-11. When the diversification variables were added to the set of industry regressions with quadratic and cubic sales terms, the incremental variance reduction was 1.24 points. In total, the set of linear and nonlinear sales and diversification variables for 14 industries accounted for 91 per cent of the variance in company-patenting about its grand mean.

two-digit industry groups with 20 or more observations. R&D employment did not increase at all with diversification for the general chemicals and electrical groups, which were by far the most R&D-oriented of the seven. It increased most for food products, which ran a close second to primary metals in fewness of R&D personnel per million dollars of sales. (The metals diversification coefficient was also positive, but significant at only the .20 level.)

The main reason for these relationships becomes clear when one examines in detail the product structure of diversified companies classified in a parent two-digit industry with relatively little private research and development and patenting. The more diversified such a firm is, the higher is the probability that it will operate also in a more progressive industry—that is, one with ample opportunities for profitable private investment in new technology. For instance, of the food-products firms with sales of less than $500 million, the leading patent recipient was National Distillers Products, which manufactured basic chemicals in addition to its more thirst-quenching specialities. Sales leaders in the food products, textiles, paper, and miscellaneous chemicals groups tended to be a good deal more diversified than most of their followers, and despite some collinearity the diversification index for these firms picked up incremental variance in patenting. And for a final example from the government-oriented sector, aircraft-makers General Dynamics and North American, which were in 1955 either established in or moving aggressively into electronics, atomic energy, and chemicals, received many more patents than less diversified Douglas and Boeing, which had higher sales at the time. It would appear then that diversification was not per se a structural condition necessarily favorable to patentable invention. Rather, the diversification variable's impressive performance shows chiefly the effect of operaton in dynamic industries by firms whose home base is not conducive to patenting and (except for aircraft) research and development.

V. *Market Power and Patented Inventions*

We consider now market-power's effect on corporate inventive output. There are two broad hypotheses assigning a favorable role to monopoly. No attempt is made to test the older but still controversial hypothesis that the expectation of a future monopoly position (e.g., through a patent grant) is necessary before risk capital will be committed to invention or development.[26] We will examine only the assertion more original to Schumpeter that monopoly power already at-

[26] But see [24], which deals with this problem at length.

tained is a favorable base from which to make advances in modern technology.

Schumpeter's writings on this subject consist of little more than *dicta* demanding further interpretation. One construction is that profitable exploitation of market power is the easiest way to assemble the funds necessary for investment in new technology [28, p. 87]. This can be subdivided into two related hypotheses: that technological output (or input) increases with corporate profitability, or that technological output increases with corporate liquidity. The latter is not explicitly Schumpeterian, but it presupposes the same assumptions.

To test these hypotheses, data on base year 1955 profits and liquid assets were collected for the 448 corporations.[27] None of a variety of tests showed any noteworthy tendency for 1959 patenting or 1955 research and development to increase with either 1955 profitability or liquidity. The r^2 between patents per billion dollars of sales and 1955 profits as a percentage of sales was .001. Between P_i/S_i and liquid assets as a percentage of total assets it was .003. The 1955 profit rate, measured in relation to both sales and total assets, was included in some 26 regressions involving raw and deflated patents, R&D personnel, and an assortment of other variables for both the full sample and particular industries. Although there was a tendency for more of the coefficients to be positive than negative, in only five cases was a *t*-ratio of more than 1.0 achieved, and there were no profitability *t*-ratios of 2.0 or more.[28] Similarly negative results appeared when the liquid asset /total asset ratio was included in the same equations.

Rejection of the profitability and liquidity hypotheses does not, however, exhaust the list of proposals ascribing a progressive bias to market power. Schumpeter argued that the temporary security and longer-range perspective afforded by a monopoly position facilitated shooting at the rapidly and jerkily moving targets of new technology [28, pp. 88 and 103]. This implies that technological output should tend to increase with industrial concentration, other things being equal. More recently it has been proposed that oligopolists colluding overtly or tacitly on the price dimension have especially vivid incentives to engage in new product rivalry [4, p. 575]. This implies that technological output should tend to increase with concentration up to a point, but that it may decline if too much of an industry's output becomes concentrated in the hands of a single dominant seller.

Prior attempts to test these hypotheses quantitatively [1] [7] [8]

[27] The principal items included in the liquid-assets definition were cash, marketable securities, government securities, foreign currency, and bank acceptances.

[28] There *was* a tendency for lagged profits to increase with patenting. This is discussed in [23].

aggregated both the technological progressiveness indices and the concentration indices into 20 or fewer two- and three-digit industry categories. While the multitude of sins underlying such a procedure may well cancel out, one never really knows what has been cooked in the stew. Therefore, a completely different approach was tried.

The only published source which links firm names with concentration ratios for a large sample of industries is a Federal Trade Commission report covering the base year 1950 [31]. That report lists the four leading firms in four- and five-digit industries, along with industry concentration ratios and sales. From the report's listings a sample of 48 industries was drawn[29] based upon several selection criteria: (1) The industry definition had to be as meaningful as possible in terms of economic analysis. (2) The Census Bureau's coverage of industry producers had to be at least 75 per cent for industries with four-firm concentration ratios below 50 per cent and 85 per cent for industries with higher concentration ratios. (3) Primary-product sales had to comprise at least 75 per cent of reported sales for the industry. (4) The broad parent industry had to be reasonably well suited for making and patenting inventions. On the strength of this criterion the food and tobacco, apparel, conventional textile, lumbering, furniture, and publishing two-digit sectors and all primarily defense-oriented industries were excluded. (5) The industry's technology had to be such that classification of patents was feasible. (6) To the extent that all other criteria could be satisfied, a wide range of technologies, concentration ratios, and industry sizes was sought.

The index of inventive output P_{4i} employed in this analysis was the number of industry-related patents issued in 1954 to the leading four firms in the ith industry. A four-year lag from the 1950 base year was used to approximate the mean lags between invention, patent application, and patent issue. Each patent assigned in 1954 to each of 152 different firms occupying leadership positions in the 48 industries was

[29] The sample includes hard-surface floor coverings; pulp, paper, and paperboard; paperboard boxes; sanitary paper food containers; sanitary paper health products; plastics materials; synthetic fibers; soaps and organic detergents; paints and varnishes; inorganic color pigments; printing ink; dentifrices; carbon black; tires and inner tubes; leather; footwear (excluding house slippers and rubber footwear); flat and laminated glass; glass containers; vitreous plumbing fixtures; porcelain electrical supplies; gypsum products; abrasive products; iron, steel, and rolling mill products; primary, rolled, drawn, and extruded aluminum; tin cans; razors and razor blades; files and rasps; power boilers; wire springs; wheeled tractors and farm machinery; track-laying tractors; machine tools; textile machinery; industrial pumps; conveyors and conveying equipment; household mechanical washing machines; vacuum cleaners; valves and fittings; ball and roller bearings; motors, generators, and steam turbines; transformers; electrical welding apparatus; household radios, television sets, and phonographs; electron tubes and transistors; storage batteries; telephone and telegraph equipment; motor vehicles and parts; and locomotives.

separately examined to determine whether or not it pertained primarily to a sampled industry. Some 5,000 patents were examined in this manner; 2,202 patents were assigned to the sampled industries and the rest were thrown out. This procedure was fairly straightforward for most industries, but in a few cases (especially in those involving by-products and substantial vertical integration) a good deal of judgment had to be applied, and the count was subject to uncertainty.

Independent variables included the value of shipments S_{4i} of the leading four firms in the ith industry in 1950, four-firm concentration ratios M_i for 1950, and slope (not intercept) dummy variables to differentiate industries operating primarily in chemical and electrical technologies from those facing largely mechanical technologies.[30]

The analysis revealed a positive but very modest and statistically insignificant influence on patenting for the market share variable. The simple linear r^2 between patenting and M_i was .069, with a t-ratio of 1.85 for the corresponding regression coefficient. In a regression of the logarithms of P_{4i} on the logarithms of M_i, r^2 was .051 and the t-ratio 1.57. The t-ratios and partial correlation coefficients for market share declined when additional variables, especially the value of shipments S_{4i}, were introduced.[31] Because one would expect the effects of scale, market share, and technology to be multiplicative rather than additive, and in order to suppress the undesirable effects of heteroscedasticity, the regression model most suitable on theoretical and statistical grounds appeared to be:

$$(8) \qquad P_{4i} = aS_{4i}^{\alpha}M_i^{\beta}C_i^{\gamma}E_i^{\delta}u_i;$$

where C_i is a chemical technology slope dummy variable, E_i is an electrical technology dummy variable, and u_i is the error term. The fitted regression equation in logarithmic form was:

$$(9) \qquad \log P_{4i} = \underset{}{1.51} + \underset{(.133)}{.680 \log S_{4i}} + \underset{(.302)}{.064 \log M_i} + \underset{(.181)}{.192 \log C_i} + \underset{(.193)}{.498 \log E_i};$$

[30] Another approach to the interindustry technological opportunity problem, using deviations of actual patenting from what adjusted equations based on those in Table 1 would predict, gave results essentially the same as those reported here.

[31] Since M_i was correlated with S_{4i}, with an r^2 of .065 in the linear version and .049 in the logarithmic version, it is possible that the higher simple correlations between patenting and market share were partly spurious. Notably, introduction of M_i into regressions of P_{4i} on S_{4i} with the two dummy variables permitted an incremental reduction in unexplained variance of only 1.2 percentage points in the additive version and 0.1 points in the logarithmic version.

with R^2 of .47. The partial correlation for market share was .032—far lower than any of the other variables' partial r's. If despite the low t-ratio for market share we use (9) as a best estimator, we find that for four industry leaders with total shipments valued at $100 million, an increase in market share from 15 per cent to 90 per cent brings an increase of one patent. For four firms with sales of $1 billion, the same market share differential implies a difference of five patents.

It must be noted that, although (9) was deemed most suitable on theoretical and statistical grounds, it assigned a weaker role to market share than the analogous additive (nonlogarithmic) equation (in which the M_i coefficient's t-ratio was 1.06). But further analysis showed this difference to be largely the result of patenting and market shares associated with the telephone and telegraph industry (with $M =$ 92 per cent). When the observations for that industry were deleted, the market share coefficient's t-ratio in the additive regression fell from 1.06 to .09.[32]

To test further the sensitivity of my results to potential biasing factors, regressions were run without the observations for eight industries with marked patent classification and assignment uncertainties.[33] The general effect was a distinct but far from statistically significant increase in the t-ratios and partial correlation coefficients associated with M_i. For example, the M_i t-ratio for equation (9) rose from .21 with $n = 48$ to .67 with $n = 40$. Deletion of telephone industry observations in addition to those of the uncertain industries caused t to fall in this case from .67 to .51.

Additional additive regression equations were estimated with a quadratic market share term in order to test for nonlinearities. In every such regression a U-shaped relationship was found, with minimum patenting occurring at four-firm concentration ratios between 44 and 71 per cent. Since the nonlinear coefficients made little incremental contribution to the reduction of variance and had no t-ratio exceeding 1.46, and since no compelling theoretical rationalization for such a relationship is known, it seems likely that the observed U-shape was the result of chance elements.

In sum, the analysis suggests that if structural market power has a beneficial effect on the output of patented inventions, it is a very modest effect indeed. One highly monopolistic industry—telephone and telegraph equipment—was responsible for much of the observed corre-

[32] Because more nearly homoscedastic logarithmic regression (9) assigned much less weight to the telephone industry's deviant patent value, it estimated a weaker role overall for the market share variable.

[33] They were plastic materials, synthetic fibers, printing ink, tracklaying tractors, transformers, radios and related products, motor vehicles, and locomotives.

lation between market share and patenting, and that industry, dominated as it is by quasi-regulated Western Electric, is clearly a special case.

This conclusion would have to be reconsidered if there were indications that the propensity to patent varied inversely with market power. But although nothing even approximating proof is available, conventional wisdom holds that the opposite is more apt to be true. In many of the most concentrated industries sampled (i.e., gypsum products, razors, synthetic fibers, flat glass, telephones, and aluminum) the market power of the leaders was built partly upon a patent base, and this fact could scarcely have been forgotten by company executives anxious to preserve that power.

It is also conceivable that existing monopoly power is a uniquely favorable environment only for investment in basic and the most fundamental applied research, either because these activities carry the highest risks or because their benefits are apt to be largely external to more competitive firms. Since knowledge—presumably an important output of such activities—cannot be patented, patent statistics do not permit a sensitive test of this narrower interpretation.[34]

As a final caveat, it must be pointed out that my data assign a weaker beneficial role to market power than do the studies of two-digit industry R&D spending reported by Hamberg [7] and Horowitz [8]. Still the fact that their analyses were unable to take into account differences in technological opportunity may have permitted the correlations to attribute a spuriously strong influence to market power.

VI. *Conclusions*

The principal conclusions of this study are as follows: (1) Inventive output increases with firm sales, but generally at a less than proportional rate. (2) Differences in technological opportunity—e.g., differences in technical investment possibilities unrelated to the mere volume of sales and typically opened up by the broad advance of knowledge—are a major factor responsible for interindustry differences in inventive output. (3) Inventive output does not appear to be systematically related to variations in market power, prior profitability, liquidity, or (when participation in fields with high technological opportunity is accounted for) degree of product line diversification.

[34] And a narrow interpretation it is, since basic research expenditures have comprised only about four per cent of all industrial R&D outlays in recent years, and since private industry has done only about 30 per cent of all U.S. basic research. Patentable inventions may also emerge from basic and certainly from applied research, even though knowledge is the main output. For instance, the early basic transistor patents were one output of Bell Laboratories' fundamental semiconductor research.

These findings among other things raise doubts whether the big, monopolistic, conglomerate corporation is as efficient an engine of technological change as disciples of Schumpeter (including myself) have supposed it to be. Perhaps a bevy of fact-mechanics can still rescue the Schumpeterian engine from disgrace, but at present the outlook seems pessimistic.

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Appendix

Incremental Analysis of Covariance: 448-Firm Patenting

Step	Description		Sum of Squared Deviations	SSD as Per Cent of A-1 SSD	D.F.	F Ratio
A-1	No independent variables		3,928,423	100.00	1	
A-2	Global regression of P_i on S_i (Eq. (1))	Reduction from A-1 Residual	1,657,710 2,270,713	42.20 57.80	1 446	326
A-3	Global regression on S_i and Diversification	Reduction from A-2 Residual	498,012 1,772,701	12.68 45.12	1 445	125
A-4	Global regression on S_i and S_i^2	Reduction from A-2 Residual	23,440 2,247,273	.60 57.20	1 445	4.64**
A-5	Global regression on S_i, S_i^2, and S_i^3	Reduction from A-4 Residual	102,985 2,144,288	2.62 54.58	1 444	21.32
A-6	Global regression on S_i, S_i^2, S_i^3, and Diversification	Reduction from A-5 Residual	400,700 1,743,588	10.20 44.38	1 443	102
A-7	14 Industry regressions on S_i (Table 1)	Reduction from A-2 Residual	1,667,626 603,087	42.45 15.35	26 420	44.66
A-8	14 Industry regressions on S_i and Diversification	Reduction from A-7 Residual	41,314 561,773	1.05 14.30	14 406	2.13*

APPENDIX (*Continued*)

Step	Description		Sum of Squared Deviations	SSD as Per Cent of A-1 SSD	D.F.	F Ratio
A-9	14 Industry regressions on S_i and 12 S_i^2	Reduction from A-7 Residual	170,788 432,299	4.35 11.00	12 408	13.43
A-10	14 Industry regressions on S_i, 12 S_i^2, and 12 S_i^3	Reduction from A-9 Residual	30,494 401,805	.77 10.23	12 396	2.50*
A-11	14 Industry regressions on S_i, 12 S_i^2, 12 S_i^3, and Diversification	Reduction from A-10 Residual	48,700 353,105	1.24 8.99	14 382	3.76*
A-12	4 Technology Group regressions on S_i	Surplus over A-7 Residual	42,211 645,298	1.08 16.43	−20 440	1.44***
A-13	4 Technology Group regressions on S_i, S_i^2, and S_i^3 [Eqs. (3)–(6)]	Reduction from A-12 Residual	94,471 550,827	2.41 14.02	8 432	9.26
A-14	Global regression of P_i on $R\&D_i$ ($n=352$)	Original variance Reduction Residual	3,784,738 2,732,927 1,051,811	100.00 72.21 27.79	1 1 350	909
A-15	Regression of P_i on 14 R&D slope dummies	Reduction from A-14 Residual	458,905 592,906	12.12 15.67	13 337	20.03

*** Significant at the 10 per cent point, ** at the 5 per cent point, * at the 1 per cent point. *F* ratios with no asterisk are significant at well above the 1 per cent point.

NATIONAL DEBT IN A NEOCLASSICAL GROWTH MOD

By PETER A. DIAMOND*

This paper contains a model designed to serve two purposes, examine long-run competitive equilibrium in a growth model and tl to explore the effects on this equilibrium of government debt. Samı son [8] has examined the determination of interest rates in a sing commodity world without durable goods. In such an economy, inter rates are determined by consumption loans between individuals different ages. By introducing production employing a durable capi good into this model, one can examine the case where individuals p vide for their retirement years by lending to entrepreneurs. After (scribing alternative long-run equilibria available to a centrally plann economy, the competitive solution is described. In this economy, wh has an infinitely long life, it is seen that, despite the absence of all t usual sources of inefficiency, the competitive solution can be inefficien

Modigliani [4] has explored the effects of the existence of gove ment debt in an aggregate growth model. By introducing a governme which issues debt and levies taxes to finance interest payments ir the model described in the first part, it is possible to re-examine] conclusions in a model where consumption decisions are made in vidually, where taxes to finance the debt are included in the analys and where the changes in output arising from changes in the capi stock are explicitly acknowledged. It is seen that in the "normal" ca external debt reduces the utility of an individual living in long-r equilibrium. Surprisingly, internal debt is seen to cause an even larg decline in this utility level.

External debt has two effects in the long run, both arising from t taxes needed to finance the interest payments. The taxes directly redu available lifetime consumption of the individual taxpayer. Furth by reducing his disposable income, taxes reduce his savings and th the capital stock. Internal debt has both of these effects as well as further reduction in the capital stock arising from the substitution government debt for physical capital in individual portfolios.

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1. Technology

The economy being considered here is assumed to have an infinite future. Its unchanging technology is assumed to be representable by a constant returns to scale aggregate production function, $F(K, L)$.[1] Since the economy exists in discrete time, the capital argument of the production function is the saving of the previous period plus the capital stock employed in the previous period. (It is assumed that there is no depreciation and that, since capital and output are the same commodity, one can consume one's capital.)

Individuals in this economy live for two periods, working in the first while being retired in the second. Each person has an ordinal utility function $U(e^1, e^2)$ based on his consumption in the two years of his life.[2] Denoting the number of persons born at the start of the tth period by L_t, labor force, growth satisfies:

$$L_t = L_0(1 + n)^t.$$

2. Centrally Planned Economy

It is simplest to examine the production possibilities of this economy by examining the alternatives available to a central planning authority. With the capital stock in period t (which was determined in period $t-1$) and the labor force in this period (which is exogenous), output will satisfy $Y_t = F(K_t, L_t)$. At the end of the production process (and before the start of consumption in this period) the central authorities have command over the capital stock and the newly produced output, $K_t + Y_t$. This must be divided between the capital stock which will be available for production in the next period, K_{t+1}, and aggregate consumption in this period, C_t. This consumption must be further divided between members of the younger generation, E_t^1, and those of the older generation, E_t^2. Assuming that all members of the same generation consume the same amount, we have:[3]

$$\overset{1}{E_t} = \overset{1}{e_t} L_t, \quad \overset{2}{E_t} = \overset{2}{e_t} L_{t-1}.$$

The division of the resources on hand between the alternative uses can be stated algebraically:

$$(1) \qquad Y_t + K_t = K_{t+1} + C_t = K_{t+1} + \overset{1}{e_t} L_t + \overset{2}{e_t} L_{t-1},$$

[1] It is assumed that F is twice differentiable and exhibits positive marginal products and a diminishing marginal rate of substitution everywhere.

[2] The assumption of the absence of all bequests is important for intertemporal allocation conclusions. A relationship between changes in the size of the national debt and changes in bequests can alter the effects to be described.

[3] Note that a person born in period t would consume e_t^1 and e_{t+1}^2 in his two years of life.

or more conventionally,

$$Y_t - (K_{t+1} - K_t) = C_t = \overset{1}{e}_t L_t + \overset{2}{e}_t L_{t-1}. \tag{2}$$

Assuming that the central authorities decide to preserve a constant capital-labor ratio, $k_t = K_t/L_t$, and thus $K_{t+1} = (1+n)K_t$, aggregate consumption will satisfy:

$$Y_t - nK_t = C_t = \overset{1}{e}_t L_t + \overset{2}{e}_t L_{t-1}. \tag{3}$$

Denoting the output-labor ratio by $y_t = Y_t/L_t$, this can be rewritten as:

$$y_t - nk_t = C_t/L_t = \overset{1}{e}_t + \overset{2}{e}_t/(1+n). \tag{4}$$

Maintenance of a constant capital-labor ratio implies, of course, a constant output per worker over time. Thus, this equation describes the consumption possibilities in each year of any period during which the capital-labor ratio remains constant. In particular, if a given capital-labor ratio is held constant for all time, the economy is on what has become known as a Golden Age Path.

3. *Neoclassical Stationary States or Golden Age Paths*

A Golden Age Path for an economy is an expansion path on which the capital-labor ratio (and thus the capital-output ratio and marginal product of capital) is kept constant. From equation (4) we see that the central-planning authorities can maintain any capital-labor ratio for which the output-capital ratio is not smaller than n (which is equivalent to the condition that the savings rate not exceed one). From equation (4), again, one can derive the amount of consumption that is possible in each period and thus calculate the Golden Age Path for which this is maximized. Similarly we can examine the alternative divisions of this consumption between individuals of different generations. Assuming that all individuals have the same lifetime consumption pattern, the problem of selecting the optimal Golden Age Path, the Golden Age Path on which each individual would have the highest utility level, subject to the constraint that all individuals have the same level, can be written:

$$\text{Maximize } U(e^1, e^2) \qquad \text{subject to } e^1 + e^2/(1+n) = y - nk. \tag{5}$$

Thus the solution of the problem of selecting an optimal Golden Age Path treats the allocation of consumption over the lifetime of an individual in a similar fashion to the allocation of consumption, in a single year, between individuals of different ages. The selection between Golden Age Paths is, as is seen from (5), a selection which ignores initial conditions, and thus not a selection available to an economy, which

must weigh the advantages of a given long-run equilibrium against the costs of achieving it.

4. *The Golden Rule Path*

This maximization problem decomposes naturally into two separate problems, that of selecting the optimal capital-labor ratio, and thus the height of the consumption constraint, $y-nk$; and that of dividing this amount of consumption between the different individuals. The maximizing capital-labor ratio is seen from (5) to satisfy the condition that the marginal product of capital equal the rate of growth, $F_K=n$. This is the standard result on the nature of the Golden Rule Path, see, e.g., Phelps [6]. Note that the optimality of this capital-labor ratio is independent of the exact division of consumption (and selecting the optimal division is independent of the capital-labor ratio chosen). If the central planners choose a higher capital-labor ratio, they would be selecting an inefficient solution (in the standard sense including the problem of initial conditions, not just as a comparison of Golden Age Paths) in that they could discard capital, lowering the capital-labor ratio to the Golden Rule level, and preserve this capital-labor ratio forever, permitting a higher level of consumption in each period forever.[4]

Utility maximizing consumption allocation clearly requires that

$$\frac{\partial U}{\partial e^1} = (1 + n) \frac{\partial U}{\partial e^2} \cdot$$

This is the allocation that would occur if consumption decisions were individually made employing a rate of interest for consumer decisions equal to the rate of growth. In examining the division of consumption when the capital-labor ratio is held constant, we are equivalently examining a model in which there is only one factor of production, labor. Thus it is not surprising that the optimal allocation is the same as that found by Samuelson [8], which he called the biological optimum. Thus the optimal rate of interest is determined by the rate of population growth (which may or may not equal the marginal product of capital). This paradox arises from the comparison of stationary states. The shifting of one unit of consumption by an individual from his first to his second year is equivalent to removing one unit of consumption from each of the living members of the younger generation and giving this total to the contemporary older generation, of whom there are n per cent fewer members.

[4] Dynamic inefficiencies of this sort in models both with and without technical change are examined by Phelps [7].

5. *Competitive Framework*

To the technological possibilities which have been described above, it is necessary to replace the central planning framework by a market process for the determination of the saving rate in each period. The annual savings behavior of the economy will determine the long-run equilibrium to which the economy converges. In particular, we will be interested in comparing alternative Golden Age Paths to which the economy converges with different quantitites of government debt outstanding. Thus, only the long-run implications of national debt will be examined, thereby avoiding the problem of selecting a social welfare function for the evaluation of different individual utility levels (at the cost of failing to explore the total effects).

By following the life history of a single individual, born, say, in period t, it is possible to trace out the market relations. This individual works in period t, for which he receives a wage, w_t, which equals the marginal product of labor, $F_L(K_t, L_t)$. This wage he allocates between current and future consumption so as to maximize his utility function, given the rate of interest existing on one-period loans from period t to period $t+1$, r_{t+1}. Thus, the members of the younger generation make up the supply side of the capital market.

This individual will thus consume, in period t, the difference between his wage and the quantity he lends in the capital market, $e^1_t = w_t - s_t$. In period $t+1$, he will consume his savings plus the accrued interest, $e^2_{t+1} = (1+r_{t+1})s_t$.

Capital demanders are entrepreneurs who wish to employ capital for production in period $t+1$. Thus the equilibrium interest rate will equal the marginal product of capital, $r_{t+1} = F_K(K_{t+1}, L_{t+1})$.

6. *Factor-Price Frontier*

The existence of the constant returns to scale production function, $F(K, L)$, which can be written as $Lf(k)$, implies a relationship between the marginal products of labor and capital which will be denoted by $w = \phi(r)$.[5] From the definitions $r = f'(k)$ and $w = f(k) - kf'(k)$ we see that:

$$(6) \qquad \frac{dw}{dr} = \phi'(r) = -k \quad \text{and} \quad \frac{d^2w}{dr^2} = \phi''(r) = \frac{-1}{f''(k)} \cdot$$

7. *Utility Maximization*

Utility maximization,[6] given a wage level and a market interest rate,

[5] For a description of the factor-price frontier see Samuelson [9].

[6] It is assumed that the utility function has the following properties: no satiation, a diminishing marginal rate of substitution everywhere, and a shape which guarantees that consump-

implies that consumption will be allocated so that:

$$\frac{\partial U}{\partial e^1} = (1 + r)\frac{\partial U}{\partial e^2}.$$

Therefore, the quantity saved can be expressed as a function of the relevant wage and interest level, $s_t = s(w_t, r_{t+1})$. It will be assumed that s is a differentiable function. From the assumption of normality, we have $0 < \partial s/\partial w < 1$. However $\partial s/\partial r$ may be positive or negative.

In addition to writing individual savings as a function of the wage and interest rates, it is possible to express the utility function in terms of these variables. From this derived form of the utility function one has:[7]

$$(7) \qquad \frac{\partial U}{\partial w} = \frac{\partial U}{\partial e^1}, \qquad \frac{\partial U}{\partial r} = \frac{s}{(1 + r)}\frac{\partial U}{\partial e^1}.$$

8. Capital Market

From the discussion above, we know that we can write the supply schedule of capital, which is the sum of the individual savings functions, as:

$$(8) \qquad S_t = s_t L_t = L_t s(w_t, r_{t+1}).$$

The demand curve for capital, which relates the capital stock in period $t+1$ to the interest rate, is merely the marginal product of capital as a function of the capital-labor ratio:

$$(9) \qquad r_{t+1} = f'(K_{t+1}/L_{t+1}).$$

---

tion in each period is a normal good, i.e.,

$$0 < \frac{\partial s}{\partial w} < 1.$$

[7] Using the optimality condition, we have:

$$\frac{\partial U}{\partial w} = \frac{\partial U}{\partial e^1}\frac{\partial e^1}{\partial w} + \frac{\partial U}{\partial e^2}\frac{\partial e^2}{\partial w} = \frac{\partial U}{\partial e^1}\left[\frac{\partial e^1}{\partial w} + \left(\frac{1}{1+r}\right)\frac{\partial e^2}{\partial w}\right].$$

From the net worth constraint $e^1 + e^2/(1+r) = w$, we have

$$\frac{\partial e^1}{\partial w} + \frac{\partial e^2}{\partial w}/(1 + r) = 1,$$

which, upon substitution, yields equation (7). Similarly, the net worth constraint implies that

$$\frac{\partial e^1}{\partial r} + \frac{\partial e^2}{\partial r}/(1 + r) - \frac{e^2}{(1 + r)^2} = 0.$$

Thus

$$\frac{\partial U}{\partial r} = \frac{\partial U}{\partial e^1}\left[\frac{\partial e^1}{\partial r} + \left(\frac{1}{1+r}\right)\frac{\partial e^2}{\partial r}\right] = \frac{\partial U}{\partial e^1}\frac{e^2}{(1+r)^2} = \frac{s}{(1+r)}\frac{\partial U}{\partial e^1}.$$

Combining the demand and supply curves, equating S_t and K_{t+1}, we have the equilibrium condition in the capital market, which relates the interest rate to the wage rate of the previous period:

$$r_{t+1} = f'(S_t/L_{t+1}) = f'(s(w_t, r_{t+1})/(1 + n)). \tag{10}$$

From the assumptions made above, we know that the demand curve is downward-sloping, while the supply curve may have a positive or negative slope. This suggests that there are two cases which need to be treated separately as the demand or supply curve is more steeply negatively sloped.[8] This is shown diagrammatically in Diagram 1.

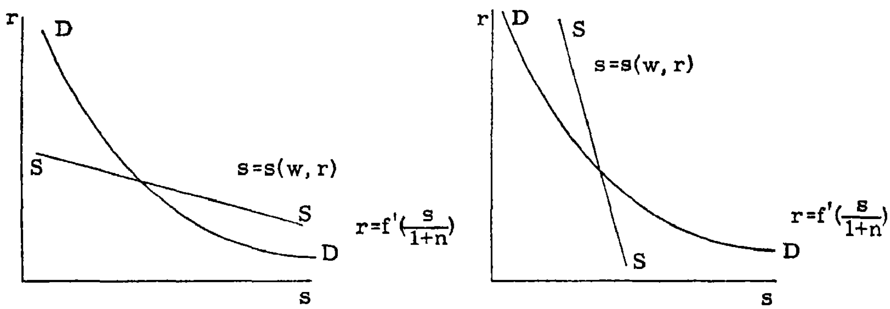


Diagram 1

The necessity of distinguishing the two cases is made clear by examining the relation between the equilibrium interest rate and the wage of the previous period. A higher wage in period t implies a greater quantity of saving at any interest rate, or a rightward shift of the saving curve in Diagram 1. However, whether this results in a higher or lower equilibrium level of saving depends on the relative slopes of the demand and supply curves. Geometrically, assuming $w' > w$, we have Diagram 2.

In the diagram on the right, which represents the "normal" case in the capital market, a higher income level results in a higher equilibrium level of saving. In the diagram on the left, where the elasticity of saving with respect to the interest rate is large and negative, a rise in the level of income results in a fall in the equilibrium level of saving. This somewhat perverse case leads to a reversal of the conclusions on the effect of debt (since taxes which reduce disposable income increase saving). Rather than complicate the text, we discuss this case in Appendix A.

By altering the wage levels in period t, we could trace out the equilibrium interest rates which would occur in period $t+1$. This relation

[8] A requirement of Walrasian stability in the capital market would permit an elimination of the case where the supply curve is steeper than the demand curve. Marshallian stability would not, of course, permit this elimination. In the absence of a dynamic theory of the capital market, it seems best to consider both cases.

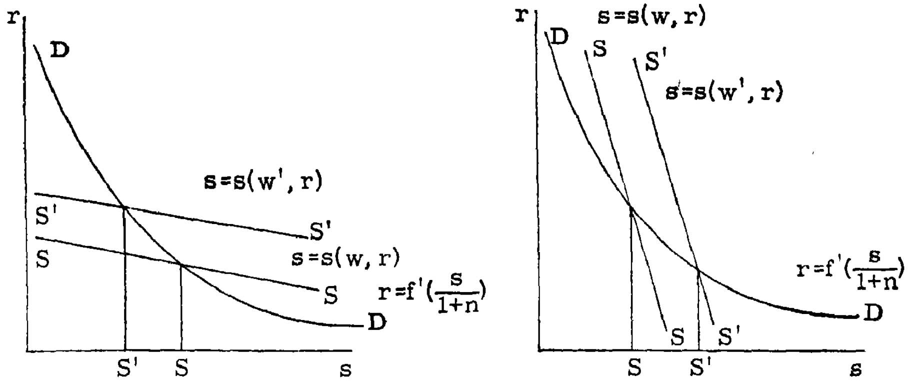


Diagram 2

will be denoted by $r_{t+1}=\psi(w_t)$. It will be assumed that ψ is differentiable. From the assumption on the relative slopes of the demand and supply curves for capital, we know that an increase in wages implies an increase in saving and thus a decrease in the interest rate. Taking the derivative of r with respect to w, we can express this as:

$$\frac{dr_{t+1}}{dw_t} = \psi' = \frac{f'' \dfrac{\partial s}{\partial w}}{1 + n - f'' \dfrac{\partial s}{\partial r}} < 0. \tag{11}$$

9. Competitive Solution

The history of this economy can be traced in Diagram 3 containing the ψ function (relating r_{t+1} to w_t) and the ϕ function (relating w_t and r_t).

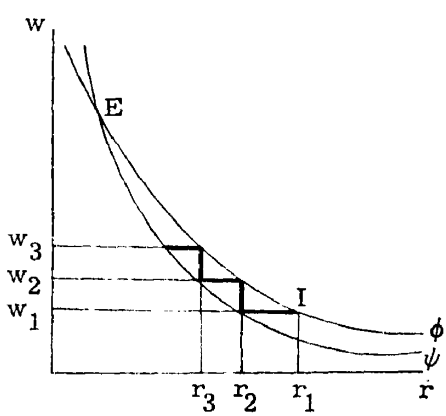


Diagram 3

Given a wage and interest pair in period one, (w_1, r_1), which is denoted by I in Diagram 3, the interest rate in the second period can be read from the ψ curve, given the wage in period one. With this interest rate in period two, the factor-price frontier, ϕ, gives the value of the wage in period two. The entire time path of the economy can be traced out in this diagram in similar fashion.

As portrayed in Diagram 3, and as will be assumed throughout this paper, the economy has a single, stable equilibrium point. In order to derive this stability condition (which will be used to derive the direction of changes in equilibrium values when debt is introduced), one first expresses r_{t+1} as a function of r_t: $r_{t+1}=\psi(\phi(r_t))$. Taking the derivative of this, and recalling equation (11) which implies that this derivative is positive, we can express the necessary condition for stability as:

$$(12) \qquad 0 < \frac{dr_{t+1}}{dr_t} = \psi'\phi' = \frac{-kf''\dfrac{\partial s}{\partial w}}{1+n-f''\dfrac{\partial s}{\partial r}} \leqq 1.$$

As is shown by the example in the next section, the competitive solution need not occur at an interest rate exceeding the Golden Rule level. Thus the competitive solution may be dynamically inefficient[9] since there exists a time after which the capital-labor ratio will exceed the Golden Rule level by a nonvanishing amount.[10]

10. An Example

As an example, consider an economy with Cobb-Douglas production and utility functions. The utility function can be expressed as:

$$U(e^1, e^2) = \beta \log e^1 + (1-\beta) \log e^2.$$

The saving function derived from this is independent of r:

$$s = (1-\beta)w.$$

Thus ψ can be written:

$$r_{t+1} = f'\left(\frac{(1-\beta)w_t}{(1+n)}\right).$$

With production satisfying:

$$y = Ak^\alpha,$$

[9] The possibility of an inefficient solution in an economy with infinitely many decision makers has been discussed by Koopmans [2].

[10] That this implies dynamic inefficiency is proved by Phelps [7].

ψ becomes:

$$r_{t+1} = \alpha A \left(\frac{(1-\beta)w_t}{(1+n)}\right)^{\alpha-1},$$

while ϕ can be written:

$$w_t = (1-\alpha)\alpha^{\alpha/1-\alpha}A^{1/1-\alpha}r_t^{\alpha/\alpha-1}.$$

Combining these we have:

$$r_{t+1} = \left(\frac{\alpha(1+n)}{(1-\beta)(1-\alpha)}\right)^{1-\alpha} r_t^{\alpha}.$$

The long-run equilibrium thus satisfies:

$$r^E = \lim_{t\to\infty} r_t = \frac{\alpha(1+n)}{(1-\alpha)(1-\beta)}.$$

Except if

$$n = \frac{\alpha}{(1-\alpha)(1-\beta)-\alpha},$$

this does not coincide with the Golden Rule. With a positive rate of growth of labor, different economies with different values of α or β can clearly have interest rates either larger or smaller than n.

II. Framework of Analysis

In examining the long-run effects of national debt, there are two approaches that might be taken, corresponding to the two concepts of incidence, balanced-budget incidence and differential incidence.[11] With balanced-budget incidence, the effects of a combination of changed expenditures and changed financing are examined, weighing the relative benefits and costs. Differential incidence refers to a comparison of alternative methods of financing a given expenditure level.[12]

In this model, there are two forms which government expenditures could take, a current consumption item (which might best be viewed as lump-sum gifts to part of the populace) or government purchase of physical capital (which would then be rented to entrepreneurs for use in the production process in each future period, with the rental payments distributed to the individuals as a social dividend). Combining these two forms of expenditures with the alternatives of tax or debt

[11] For a detailed discussion of these concepts, see Musgrave [5].

[12] The failure to distinguish between these separate questions has been the cause of some of the confusion in the literature on the public debt. See, e.g., Mishan [3].

finance gives four possible questions of balanced-budget incidence which might be asked.

However, although answers to some of these questions will arise, analysis will be restricted to the differential incidence question of substituting debt for tax finance for a given government expenditure.

This substitution could be employed while financing the purchase of physical capital. The long-run incidence question would then be resolved by comparing the long-run equilibrium arising when there is government capital and government debt with the long-run equilibrium occurring when there is only government capital. Since the simultaneous issuance of debt and purchase of capital would merely make the government a middleman between entrepreneurs and savers, this action would have no effect on the economy in either the short or long run. Thus the initial equilibrium would be compared to that arising when there is government-owned capital, but no debt outstanding.

Alternatively, the government could finance some windfall payment (such as veterans' bonuses) either from concurrent taxes or debt issuance. While tax-financed transfer payments would have an effect in the short run (depending on the relation between the recipients and the taxpayers), since it shifts neither ϕ nor ψ it would have no effect on the long-run equilibrium. Thus the original long-run equilibrium could be compared to the one arising when debt exists (which shifts ψ) but the expenditures had no permanent effect. Either of these differential incidence frameworks would lead to the same qualitative solutions, and the second one will be adopted.

12. National Debt

To avoid the problem of expected capital gains, it will be assumed that all government debt has a one-period maturity. It will also be assumed that the debt, which is refloated each period simultaneously with the achievement of equilibrium in the capital market, pays the current interest rate. For internally held debt this assumption is necessary, given the assumption of perfect certainty, for wealth owners to be willing to hold both debt and physical capital in their portfolios. The assumption is also made for externally held debt for the sake of symmetry in the comparison of the two types of debt. The, perhaps, more natural assumption of a supply curve of external capital is discussed in Appendix B. With the assumption of a horizontal supply curve at an interest rate equalling the equilibrium domestic rate before the issuance of further debt, the qualitative results of this case are identical to those of the case considered in the text.

Since the long-run effects of the debt depend on a permanent shift in ϕ or ψ, a fixed absolute amount of debt, in a growing economy, would

asymptotically have no effect. Therefore it will be assumed that the debt-labor ratio is held constant (that the quantity of debt grows at n per cent) by financing part of the interest cost by additional debt, while financing the remainder by taxes. (It should be noted that in the case of an inefficient competitive solution, where the rate of growth exceeds the rate of interest, this implies negative taxes.) The measure of the quantity of debt outstanding in any period will be the quantity outstanding at the start of the period (or equivalently, at the time of the production process), which is therefore the quantity issued in the previous period. Thus the denominator of the debt-labor ratio refers to the number of individuals in the tax base for financing the debt, rather than the number of savers entering the capital market to purchase the debt.

The taxes employed to finance interest costs (which are paid concurrently with the receipt of factor payments) will be assumed to be lump-sum taxes on the younger generation.[13]

13. External Debt

The effects of the existence of externally held debt on the domestic economy arise solely from the taxes needed to finance that part of the interest cost not covered by increased debt. Thus we would expect the utility of an individual living at the time of long-run equilibrium to decrease because of increased taxes (in the efficient case where the interest rate exceeds the growth rate) and to change because of the change in the equilibrium wage-interest rate pair caused by the impact of these taxes on the supply side of the capital market. Denoting the external debt-labor ratio by g_1, the taxes per worker in period t are $(r_t-n)g_1$. Therefore, the equation for the ψ function must be changed to relate savings to the wage net of taxes $\hat{w}_t$, which equals $w_t-(r_t-n)g_1$. Rewriting the condition for equilibrium in the capital market, equation (10), we have:

$$r_{t+1} = f'\left(\frac{s(w_t - (r_t - n)g_1, r_{t+1})}{1 + n}\right). \tag{13}$$

The new form of this equation implies a new stability condition which, together with the assumption on relative slopes in the capital market, is expressed in (14). (It is assumed that there is a single stable equilibrium both with and without the debt.)

[13] The case with lump-sum taxes on the older generation is equivalent to that with lump-sum taxes on the younger generation plus intergeneration transfers in each period. This transfer scheme (from old to young) increases saving in anticipation of taxes and counteracts the decreases described in the text, although not fully.

$$(14)\qquad 0 < \frac{dr_{t+1}}{dr_t} = \frac{-f''(k+g_1)\dfrac{\partial s}{\partial w}}{1+n-f''\dfrac{\partial s}{\partial r}} \lesseqgtr 1.$$

To examine the shift in the ψ curve, we can implicitly differentiate equation (13) again, this time taking the partial derivative of r_{t+1} with respect to g_1.

$$(15)\qquad \frac{\partial r_{t+1}}{\partial g_1} = \frac{f''(n-r_t)\dfrac{\partial s}{\partial w}}{1+n-f''\dfrac{\partial s}{\partial r}}.$$

From equation (14) we know that the sign of this expression is the same as that of $(r-n)$. Geometrically we have ψ shifting to the new curve ψ' as shown in Diagram 4. By combining the new ψ curve with

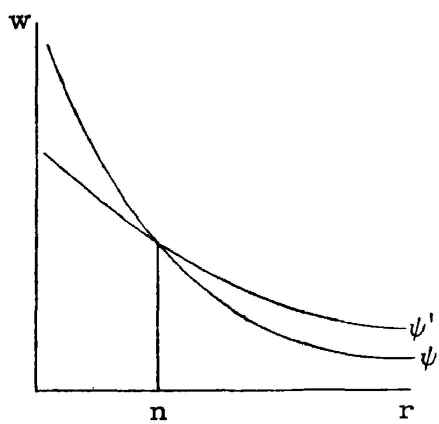


Diagram 4

the factor price frontier, which is unchanged, we can examine the change in the long-run equilibrium values of r and w. In Diagram 5 we see that, if the equilibrium interest rate was unequal to the growth rate, the existence of external debt increases the difference between the two.[14]

To examine the effects of external debt on utility levels in long-run equilibria, it is simplest to assume a given level of external debt and examine the changes arising from a derivative change in this quantity.

[14] Since, if $r=n$, additional debt issuance exactly covers interest payments, if this were the original equilibrium, the debt has no effect.

Using equation (13) and the constancy of the interest rate in long-run equilibrium, one can express the equilibrium interest rate as an implicit function of the quantity of debt outstanding:

$$r = f'\left(\frac{s(\phi(r) - (r - n)g_1, r)}{1 + n}\right). \tag{16}$$

From this relationship one can derive the change in the equilibrium interest rate arising from a change in the debt-labor ratio:

$$\frac{dr}{dg_1} = \frac{-f''(r - n)\dfrac{\partial s}{\partial w}}{1 + n - f''\dfrac{\partial s}{\partial r} + f''(k + g_1)\dfrac{\partial s}{\partial w}}. \tag{17}$$

As was described above, external debt moves the interest rate away from the Golden Rule solution. In terms of the capital market, we have that positive taxes, by decreasing the supply of capital, given any level of the wage, increase the equilibrium interest rate.

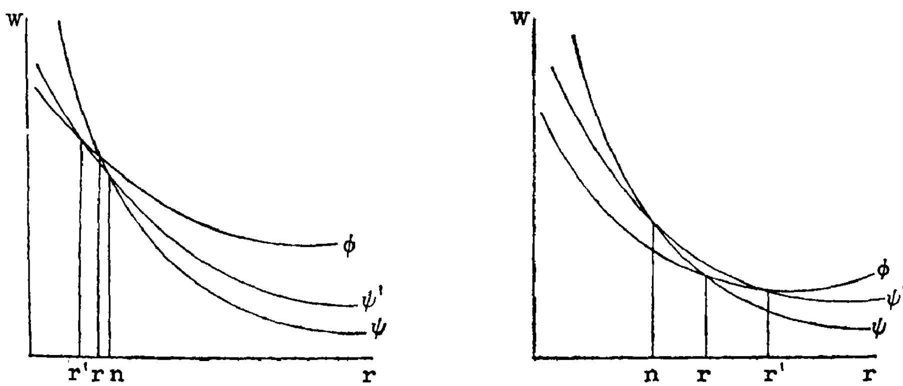


Diagram 5

The change in the utility level can be calculated by employing the expressions for the effects of changes in factor payments on the utility level, equation (7).

$$\frac{dU}{dg_1} = \frac{d\hat{w}}{dg_1}\frac{\partial U}{\partial w} + \frac{dr}{dg_1}\frac{\partial U}{\partial r} = \frac{\partial U}{\partial e^1}\left[\frac{d\hat{w}}{dg_1} + \frac{s}{(1 + r)}\frac{dr}{dg_1}\right]. \tag{18}$$

From the expression for the net wage, $\hat{w} = w - (r - n)g_1$, one can calculate the change in the net wage in terms of the change in the interest rate:

$$\frac{d\hat{w}}{dg_1} = -(k + g_1)\frac{dr}{dg_1} - (r - n). \tag{19}$$

Substituting this equation in the previous equation we have:

$$(20) \qquad \frac{dU}{dg_1} = -\frac{\partial U}{\partial e^1}\left[(r-n) + g_1\frac{dr}{dg_1} + \left(k - \frac{s}{1+r}\right)\frac{dr}{dg_1}\right].$$

The first term of this expression is the change in utility arising from the taxes needed to finance the addition to the outstanding debt, and is positive or negative as these taxes are positive or negative. The second term describes the change in the tax burden of existing debt occurring because of the change in the interest rate. Thus, both of these utility changes are positive if r exceeds n and negative if n exceeds r.

The third term can be explained by means of Diagram 6 containing derived indifference curves between w and r, denoted by II, and the factor-price frontier.[15]

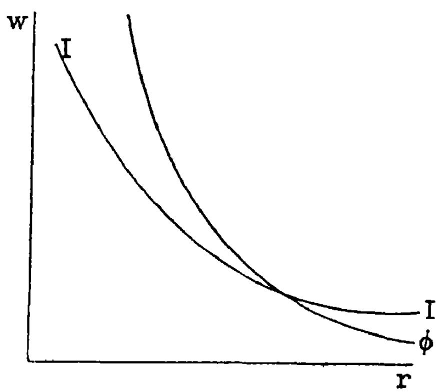


Diagram 6

The change in the interest rate is a movement along the factor-price frontier. The change in utility thus depends on the relative slopes of the factor-price frontier, the slope of which is $-k$, and of the indifference curve, the slope of which is $-s/(1+r)$. From the equilibrium condition for the capital market, $k=s/(1+n)$, this term can be rewritten as

$$\frac{dr}{dg_1}\left(\frac{k}{1+r}\right)(r-n).$$

Since dr/dg_1 has the same sign as $(r-n)$, the movement of the interest rate away from the Golden Rule level causes the utility from factor payments to fall.

Combining the three effects, we can conclude that in the "normal" case where the competitive solution is efficient, external debt causes a

[15] A rigorous treatment of this approach would require changes in the indifference levels because of taxes.

fall in the utility level of an individual living in long-run equilibrium. If the competitive solution is inefficient the effects of the debt work in opposite directions and so yield no a priori conclusion.

14. Internal Debt

With internal debt, the supply side of the capital market is altered in precisely the same fashion as with external debt, since an individual taxpayer is in the same position as a taxpayer whether his tax payments flow abroad or remain in the country. Denoting the internal debt-labor ratio by g_2, the savings function must be altered as before to read

$$s(w_t - (r_t - n)g_2, r_{t+1}).$$

It is also necessary to alter the equilibrium condition in the capital market to take account of the fact that the government enters on the demand side of this market. Denoting the quantity of internal debt to be floated in period t (and repaid in $t+1$) by G_{t+1}, the equilibrium condition becomes:

$$S_t = K_{t+1} + G_{t+1}. \tag{21}$$

Dividing this by L_{t+1}, we have the equilibrium condition expressed in terms of the ratios needed to describe the equilibrium:

$$\frac{s_t}{1+n} = k_{t+1} + g_2. \tag{22}$$

Comparing internal with external debt, we see that they both require taxes to be paid by each worker, while internal debt has a further effect in that it substitutes pieces of paper for physical capital in the portfolios of wealth owners, thus reducing output.

Recalling that the demand for capital by entrepreneurs is determined by the marginal productivity schedule of capital, we can combine this equation with equation (22) to obtain the new condition for equilibrium in the capital market:

$$r_{t+1} = f'\left(\frac{s(w_t - (r_t - n)g_2, r_{t+1})}{1+n} - g_2\right). \tag{23}$$

As before, by implicit differentiation of this equation we can express the conditions for stability and the sssumed slopes in the capital market:

$$0 < \frac{dr_{t+1}}{dr_t} = \frac{-f''(k+g_2)\dfrac{\partial s}{\partial w}}{1+n-f''\dfrac{\partial s}{\partial r}} \leqq 1. \tag{24}$$

To find the shift in ψ, we take the partial derivative of r with respect to g_2:

$$\text{(25)} \qquad \frac{\partial r_{t+1}}{\partial g_2} = \frac{-f''\left(\frac{\partial s}{\partial w}(r-n) + (1+n)\right)}{1 + n - f''\frac{\partial s}{\partial r}}.$$

From (24) and the normality of present consumption, $\partial s/\partial w < 1$, we know that this expression is positive and thus that ψ shifts upward for all values of r.

Following the same analysis as with external debt, we can calculate the change in utility arising from the change in the level of internal debt. We write first the locus of equilibria for different quantities of debt:

$$\text{(26)} \qquad r = f'\left(\frac{s(\phi(r) - (r-n)g_2, r)}{1+n} - g_2\right).$$

We can then differentiate this expression with respect to g_2 to obtain the change in the equilibrium interest rate arising from the change in the quantity of debt:

$$\text{(27)} \qquad \frac{dr}{dg_2} = \frac{-f''\left(1 + n + (r-n)\frac{\partial s}{\partial w}\right)}{1 + n - f''\frac{\partial s}{\partial r} + f''(k+g_2)\frac{\partial s}{\partial w}}.$$

Again, from equation (24), we know that the change in the equilibrium interest rate is positive. Employing equations (18) and (19) relating changes in utility and the net wage to changes in debt (which hold for either internal or external debt) and the equilibrium condition for the capital market, equation (22), we can express the changes in utility in two ways:

$$\text{(28)} \qquad \frac{dU}{dg_2} = -\frac{\partial U}{\partial e^1}\left[(r-n) + g_2\frac{dr}{dg_2} + \left(k - \frac{s}{1+r}\right)\frac{dr}{dg_2}\right]$$

$$\text{(29)} \qquad \frac{dU}{dg_2} = -\frac{\partial U}{\partial e^1}(r-n)\left[1 + \frac{k+g_2}{1+r}\,\frac{dr}{dg_2}\right].$$

Equation (28) expresses the change in utility in terms of the taxes needed to finance the increase in debt, the taxes needed to finance the increased interest payments on existing debt, and the changed value of factor payments. As before, the sign of the first two terms depends

solely on $(r-n)$. However, since $s=(1+n)(k+g_2)$, the third term, while decreasing utility when r is smaller than n, may increase or decrease utility when the competitive solution is efficient.

Equation (29), which combines the separate terms, shows that utility is decreased in the efficient case and increased in the inefficient case. Like the third term in the expression giving the change in utility from external debt, the sign of this expression is the opposite of the sign of $(r-n)dr/dg$. Separating the effects of debt issuance into those which alter the social consumption possibilities (the flow of interest payments abroad) and those that reflect a change in the allocation of consumption within the society, the total impact of all effects falling into the second group will increase or decrease utility as the interest rate is moved toward or away from the rate of growth. This is demonstrated geometrically in the next section.

15. Diagrammatic Discussion

The assumption that both present and future consumption are normal goods implies that, as one moves upward and to the left along a budget line, the indifference curves become steeper. Geometrically, this implies that the slope at A is algebraically greater than at B. See Diagram 7.

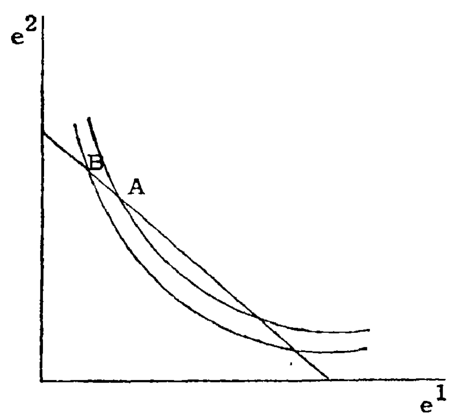


Diagram 7

Recalling equation (5), the consumption possibilities for a society with a given capital-labor ratio lie along the line $e^1+e^2/(1+n)=y-nk$. Since the interest rate is the marginal product of capital in a competitive society, from the interest rate we know the height of the consumption constraint line. Furthermore, since consumption is allocated over time in accordance with the market interest rate, we know that the

competitive equilibrium occurs where the slope of an indifference curve equals $-(1+r)$. These two facts permit us to locate a competitive equilibrium in Diagram 7, knowing just the equilibrium interest rate (and, of course, the production function). Since internal debt does not alter the consumption possibilities available to an economy, the utility associated with the equilibrium arising from varying quantities of internal debt can be located in this diagram. (This, of course, is not true for external debt.)

Combining these two facets of a change in the interest rate we can conclude that any movement of the interest rate away from the growth rate decreases utility first by diminishing the height of the consumption constraint line and second by moving along the lowered line in the direction of decreased utility. Assuming $r'>r>n$, this is shown in Diagram 8 where A is the equilibrium point associated with r while C is the one associated with r'.

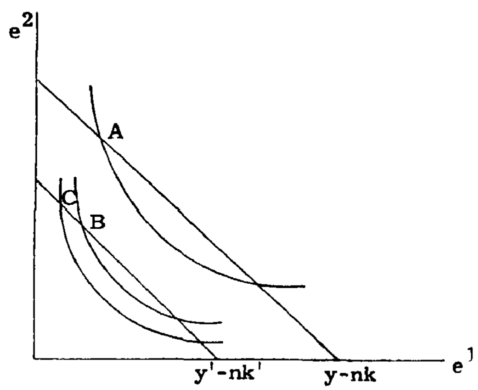


Diagram 8

Utility at B (where the slope of the indifference curve is the same as at A) is less than utility at A since B is on a lower constraint line. The slope at C, which equals $-(1+r')$, is less than at B, $-(1+r)$, implying a lower utility level at C than at B.

Thus internal debt raises or lowers the utility level as it moves the equilibrium interest rate towards or away from the growth rate. External debt has two effects, an alternation of consumption possibilities due to the flow abroad of interest payments and an alteration of utility arising from changes in the interest rate, given the level of interest payments, which is positive or negative as the interest rate is moved toward or away from the growth rate.

Thus the third term in dU/dg_1, which is the "purely domestic" effect of issuing external debt corresponds to the entire effects of internal debt, with the sign of the expressions equalling that of $-(r-n)dr/dg$.

16. *Internal and External Debt*

Having described the way each of them affects the equilibrium of the economy, it is now possible to turn to the complete model, in which there is both external and internal debt, and so make a direct comparison of their effects.[16]

Without stopping to repeat the analysis step by step, we can write down the relevant equations from the equations derived in the last three sections:

The condition for equilibrium in the capital market:

$$r_{t+1} = f'\left(\frac{s(w_t - (r_t - n)(g_1 + g_2), r_{t+1})}{1 + n} - g_2\right). \tag{30}$$

The locus of long-run competitive equilibria with different quantities of debt outstanding:

$$r = f'\left(\frac{s(\phi(r) - (r - n)(g_1 + g_2), r)}{1 + n} - g_2\right). \tag{31}$$

The necessary condition for stability and the assumption on the demand and supply curves for capital:

$$0 < \frac{dr_{t+1}}{dr_t} = \frac{-f''(k + g_1 + g_2)\dfrac{\partial s}{\partial w}}{1 + n - f''\dfrac{\partial s}{\partial r}} \leqq 1. \tag{32}$$

The change in the equilibrium interest rate arising from changes in debt:

$$\begin{aligned} \frac{dr}{dg_1} &= \frac{-f''(r - n)\dfrac{\partial s}{\partial w}}{1 + n - f''\dfrac{\partial s}{\partial r} + f''(k + g_1 + g_2)\dfrac{\partial s}{\partial w}} \\ \frac{dr}{dg_2} &= \frac{-f''\left((r - n)\dfrac{\partial s}{\partial w} + (1 + n)\right)}{1 + n - f''\dfrac{\partial s}{\partial r} + f''(k + g_1 + g_2)\dfrac{\partial s}{\partial w}}. \end{aligned} \tag{33}$$

[16] In Section 14, the change from external to internal debt was described as adding the effect arising from the substitution of paper for physical capital in portfolios. Reversing this comparison, external debt is internal debt plus annual foreign borrowing, with foreign capital receiving its marginal product. This does not directly alter net output, but it does alter relative factor prices which directly affects utility and which alters savings.

The changes in utility arising from changes in the quantity of debt:

$$(34)\quad \begin{aligned} \frac{dU}{dg_1} &= -\frac{\partial U}{\partial e^1}\left((r-n)+\frac{dr}{dg_1}(g_1+g_2)+\frac{dr}{dg_1}\left(k-\frac{s}{1+r}\right)\right) \\ \frac{dU}{dg_2} &= -\frac{\partial U}{\partial e^1}\left((r-n)+\frac{dr}{dg_2}(g_1+g_2)+\frac{dr}{dg_2}\left(k-\frac{s}{1+r}\right)\right). \end{aligned}$$

With these relations before us, it is possible to examine the differential incidence question arising from the effects of issuing internal debt to retire external debt, and to examine the relationships between some of the articles in the literature on the burden of the debt.

From equation (33) we can calculate the change in the equilibrium interest rate arising from this debt swap:

$$(35)\quad \frac{dr}{dg_2}-\frac{dr}{dg_1}=\frac{-f''(1+n)}{1+n-f''\dfrac{\partial s}{\partial r}+f''(k+g_1+g_2)\dfrac{\partial s}{\partial w}}.$$

From the stability condition, (32), we know that the denominator of this expression is positive and thus that the interest rate always rises. The effect of the debt swap involves no change in taxes, and so no change in the supply side of the capital market. However, the demand side is altered by the increase in government demand, causing a rise in the equilibrium interest rate and a fall in the capital-labor ratio.

The change in utility can be derived from (34) and expressed in different ways:

$$(36)\quad \frac{dU}{dg_2}-\frac{dU}{dg_1}=-\frac{\partial U}{\partial e^1}\left[\frac{dr}{dg_2}-\frac{dr}{dg_1}\right]\left[(g_1+g_2)+\left(k-\frac{s}{1+r}\right)\right],$$

$$(37)\quad \begin{aligned} \frac{dU}{dg_2}-\frac{dU}{dg_1} = -\frac{\partial U}{\partial e^1}\Bigg(&\left[-(r-n)+\left(\frac{dr}{dg_2}-\frac{dr}{dg_1}\right)g_1\right] \\ &+(r-n)\left[1+\left(\frac{dr}{dg_2}-\frac{dr}{dg_1}\left(\frac{(k+g_2)}{(1+r)}\right]\right)\right)\Bigg), \end{aligned}$$

$$(38)\quad \frac{dU}{dg_2}-\frac{dU}{dg_1}=-\frac{\partial U}{\partial e^1}\left[\frac{dr}{dg_2}-\frac{dr}{dg_1}\right]\left[\frac{(k+g_2)(r-n)}{(1+r)}+g_1\right].$$

Equation (36) divides the utility change into the part arising from the change in taxes and the part arising from the change in the utility of factor payments. Since the interest rate rises, taxes must rise, lowering utility. However, since

$$\left(k-\frac{s}{(1+r)}\right)\quad \text{is equal to}\ \frac{(r-n)k-(1+n)g_1}{1+r},$$

as in the discussion of internal debt, the change in utility coming from the change in factor payments may be positive or negative.

Equation (37) divides the utility change into the part arising from the change in the external interest payments, which may be positive or negative, and the part arising, as in the last section, domestically, from the change in equilibrium values, given the level of external payments. This term has the sign

$$-(r - n)\left(\frac{dr}{dg_2} - \frac{dr}{dg_1}\right),$$

which is, therefore, the sign of $n-r$. Thus, as before, for this term the rise in interest rates raises utility in the inefficient case but lowers it in the efficient case.

The third form of the equation, (38), is expressed to most easily give the sign of the utility change. When the solution is efficient, we have an unambiguous fall in utility from this debt swap. In the inefficient case the sign depends on the relative sizes of

$$g_1, \quad \text{and} \quad \frac{(k + g_2)(r - n)}{(1 + r)} \cdot$$

17. Conclusion

Thus we have seen that, where both types of debt exist, internal debt, which raises the interest rate, lowers utility in the efficient case but may raise or lower it in the inefficient case (if there were no external debt, utility would be raised in the inefficient case). External debt, which moves the interest rate away from the growth rate, lowers utility in the efficient case and may raise or lower it in the inefficient case (this remains true whether or not internal debt exists). Finally, the substitution of internal for external debt, which raises the interest rate, lowers utility in the efficient case, while being capable of raising or lowering it in the inefficient case.

There are two ways of classifying the effects of external and internal debt which shed some light on some of the effects described in the literature.

First, as in equation (34), they can be divided into utility changes arising from changes in taxes paid and from a change in the relative factor payments. This division shows that the taxes needed to finance either internal or external debt have the same impact on individuals living during long-run equilibrium.

Second, the change in utility from internal debt can be separated into the effects of external debt plus the effects of a debt swap. This

would imply four effects, the effects of the two changes in taxes, and the two effects on factor payments. These latter two effects can be distinguished by the fact that external debt affects only the supply side of the capital market, while the debt swap affects only the demand side.

In their discussion of the effects of debt, Bowen, Davis, and Kopf [1] concentrated on the tax effects of internal debt, and so described the first two of these four effects.

Modigliani [4] and Vickrey [10] discussed the fall in the capital stock arising from the substitution of debt for capital in the portfolios of wealth owners. As such, they were discussing the change in the demand side of the capital market[17] and the effects described are additive to those arising from taxes. It is only necessary to add the effects of taxes on the capital stock (and thus on factor payments) to complete the discussion.

Appendix A

Making the alternative assumption on the capital market, which, together with the stability condition, can be expressed as:

$$-1 \leqq \frac{dr_{t+1}}{dr_t} < 0,$$

we can re-examine the signs of the equations in Section 16. The denominators of the expressions giving the change in the equilibrium interest rate are now negative (where they were positive in the text). Thus external debt moves the interest rate toward the growth rate, while internal debt lowers its equilibrium value. Consequently the debt swap lowers the rate of interest.

Therefore, in the efficient case, increased debt causes positive taxes for the additional debt but lowers the taxes on existing debt and so may raise or lower the utility level. The debt swap raises utility by decreasing taxes

[17] Modigliani described a one-for-one replacement of capital by debt, assuming that total wealth remained constant. However, the fall in the capital stock, which causes a fall in output, would affect the equilibrium quantity of total wealth. (Modigliani acknowledges this but ignores its effects.) The change in the capital stock can be derived from equation (33):

$$\frac{dk}{dg_1} - \frac{dk}{dg_2} = \frac{1}{f''}\left(\frac{dr}{dg_1} - \frac{dr}{dg_2}\right) = \frac{-1-n}{1+n-f''\left(\frac{\partial s}{\partial r} - (k+g_1+g_2)\frac{\partial s}{\partial w}\right)}.$$

This differs from -1 because of the term

$$\left(\frac{\partial s}{\partial r} - (k+g_1+g_2)\frac{\partial s}{\partial w}\right).$$

This latter expression represents the partial effect on desired wealth (which is equal to savings) arising from the fall in the capital stock:

$$\frac{\partial s}{\partial k} = \frac{\partial s}{\partial r}\frac{\partial r}{\partial k} + \frac{\partial s}{\partial w}\frac{\partial \hat{w}}{\partial k} = f''\frac{\partial s}{\partial r} + \frac{\partial s}{\partial w}f''\frac{\partial \hat{w}}{\partial r} = f''\left(\frac{\partial s}{\partial r} - (k+g_1+g_2)\frac{\partial s}{\partial w}\right).$$

and increasing the utility of factor payments by moving the interest rate toward the Golden Rule level.

Appendix B

Assuming a supply curve of external debt which can be expressed by writing the interest rate, ρ, as a function of g_1 (this assumes that the source of foreign capital is also increasing at n per cent per year), the net wage can be expressed:

$$\hat{w} = w - (\rho - n)g_1 - (r - n)g_2.$$

This implies that the change in the net wage can be expressed as:

$$\frac{d\hat{w}}{dg_1} = -(k + g_2)\frac{dr}{dg_1} - (\rho - n) - g_1\frac{d\rho}{dg_1},$$

$$\frac{d\hat{w}}{dg_2} = -(k + g_2)\frac{dr}{dg_2} - (r - n).$$

Thus the utility change becomes:

$$\frac{dU}{dg_1} = -\frac{\partial U}{\partial e^1}\left(\rho - n + g_1\frac{d\rho}{dg_1} + \frac{dr}{dg_1}\left(k + g_2 - \frac{s}{1 + r}\right)\right),$$

$$\frac{dU}{dg_2} = -\frac{\partial U}{\partial e^1}\left((r - n) + \frac{dr}{dg_2}\left(k + g_2 - \frac{s}{1 + r}\right)\right).$$

If the supply curve of capital is horizontal at the prevailing internal interest rate, these two expressions differ from equation (34) only in the disappearance of the term $g_1(dr/dg)$ from both equations (and the somewhat different interest rate derivative). Thus the effect of a debt swap becomes:

$$\frac{dU}{dg_2} - \frac{dU}{dg_1} = -\frac{\partial U}{\partial e^1}\left(\frac{dr}{dg_2} - \frac{dr}{dg_1}\right)\left(k + g_2 - \frac{s}{1 + r}\right),$$

which depends in sign solely on whether the difference between the interest and growth rates is increased.

The change in the equilibrium interest rates can be derived from the locus of equilibria:

$$r = f'\left(\frac{s(w - (\rho - n)g_1 - (r - n)g_2, r)}{1 + n} - g_2\right)$$

$$\frac{dr}{dg_1} = \frac{-f''\dfrac{\partial s}{\partial w}\left((\rho - n) + g_1\dfrac{d\rho}{dg_1}\right)}{1 + n - f''\dfrac{\partial s}{\partial r} + f''(k + g_2)\dfrac{\partial s}{\partial w}}$$

$$\frac{dr}{dg_2} = \frac{-f''\left(1 + n + (r - n)\dfrac{\partial s}{\partial w}\right)}{1 + n - f''\dfrac{\partial s}{\partial w} + f''(k + g_2)\dfrac{\partial s}{\partial w}}.$$

Again assuming that $\rho = r$ and $d\rho/dg = 0$, these derivatives are qualitatively the same as those described in the text.

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COMMUNICATIONS

Professor Samuelson on Theory and Realism: Comment

In a recent issue of this *Review,* Fritz Machlup [3] comments on Paul Samuelson's discussion of "Problems of Methodology" at the 1962 AEA meeting [5], and this is followed by Samuelson's reply [6]. The exchange has exposed some fundamental issues in economic methodology. Of greater interest however is the resulting deadlock on these issues arrived at by two such brilliant theorists.

Machlup finds that Samuelson the methodologist really rejects "*all theory*" [3, p. 733], while at the same time Samuelson the theorist excels in the very activity he rejects i.e., he "produces his best work when he deduces from unrealistic assumptions general theoretical propositions . . ." [3, p. 735]. Samuelson, for his part, fails to see why Machlup should make such a fuss. "My position is so innocuous as to be platitudinous" [6, p. 736]. He can find no reason for altering his position and hence he merely reiterates his original view. This is in effect a rejection of the consequences of Milton Friedman's methodology as expounded in his well-known essay, "The Methodology of Positive Economics" [2]. Given Samuelson's response to Machlup's charges, there appears to be no reason why Machlup should change his views. Therefore, a deadlock has been reached.

Machlup holds that since most assumptions and theories in economics represent simplifications of reality, they are therefore unrealistic. Hence, we test a theory by the correspondence of its consequences with reality and by its usefulness in interpreting complex situations. Samuelson agrees that abstract, simplified theories may be useful as an aid in understanding reality. He insists however that a theory suffers to the extent that its assumptions contain factual inaccuracies. Samuelson further insists, and Machlup denies, that if the consequences of a theory are empirically valid (realistic), then the theory as such must also be empirically valid and so must its assumptions [5, p. 234]. That is, the assumptions imply the theory and the theory implies the consequences. But later on, Samuelson approvingly quotes the following statement by Heinrich Hertz: "All of Maxwell's theory boils down to the simple question of whether the observable measurements on light and waves do or do not satisfy Maxwell's partial differential equations" [6, p. 737]. Samuelson, in spite of himself, is here approaching Machlup's position. For if all that matters is a set of equations that fit the empirical data, we need not be particularly concerned about the reality of the assumptions. The problem is frequently one of seeking out relationships among already existing statistical series. In some cases trial and error will do. In other cases, not only may very simple assumptions be usefully employed, but more than one set of as-

sumptions may be consistent with the data. On the other hand, if Samuelson really means that empirically valid equations imply valid (realistic) assumptions, then the implied empirically valid consequences can *never* imply unrealistic (invalid) assumptions. Therefore, we may legitimately choose to concern ourselves with testing the realism of the consequences only; to then test assumptions would be redundant.

Samuelson claims that scientists merely *describe*, they never *explain*. "Scientists never 'explain' any behavior, by theory or by any other hook" [6, p. 737]. This position is presumably intended to buttress his approach. If scientists only describe phenomena, it is important that their descriptions bear a close resemblance to the phenomena. Hence, assumptions, theories, and consequences ought to be realistic. Samuelson believes, as do some authoritative philosophers and scientists, that since scientists do not possess *ultimate* explanations, they are therefore left with descriptions. But an explanation does not have to be "ultimate" in order to provide a scientist with an intellectually satisfying and useful answer to a question beginning with the word *why*. Many eminent philosophers and scientists support the idea of explanation in science. For example, Ernest Nagel, in discussing this question, says that "there are in fact well-established uses for the words 'why' and 'explanation' such that it is entirely appropriate to designate an answer to a 'why' question as an explanation . . ." [4, p. 27]. Albert Einstein tells us that "the general theory of relativity owes its origin to the attempt to explain a fact known since Galileo's and Newton's time . . ." [1, p. 257].

The really ironic aspect of this controversy is that Samuelson himself arrives at a sound conclusion on the basis of premises and polemics which are open to serious question. His conclusion: "The fact that nothing is perfectly accurate should not be an excuse to relax our standards of scrutiny of the empirical validity that the propositions of economics do or do not possess" [5, p. 236]. This is sound advice. If we view economics as a system of knowledge, as a way of understanding the working of a complex reality, then this *understanding* requires that assumptions, theories, and consequences form intelligible patterns, both internally and with respect to the external world. It is indeed possible to predict without being able to explain (understand), and vice versa. But the best theories offer explanations *and* predictions. Economists have found that good predictions depend importantly on their accumulated understanding of the economy. Take Machlup as a case in point. His "simplified" theories and the demand by the U.S. Congress for his services as an expert in economics both derive from his broad and deep understanding of economic organization and events.

Physicists may be able to predict certain events with great accuracy without worrying about a literal understanding of reality. Some economists may aspire to this seemingly high level of sophistication. Others, who do not, can draw some comfort from the words of a physicist whose approach led to a revolution in our thought and in our world: "Some physicists, among them myself, can not believe that we must abandon, actually and forever, the idea

of direct representation of physical reality in space and time; or that we must accept the view that events in nature are analogous to a game of chance" [1, p. 261].

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Professor Samuelson on Theory and Realism: Comment

Contradictions do not exist in nature because nature does not differ from itself. Similarly there can be no real methodological difference between economists who do not differ in the methods they use in their actual scientific work. The dispute between Professors Machlup and Samuelson on the need for realism in theories can, therefore, only reflect a semantic confusion.

Machlup identifies theory with "postulated relationships." But this is not sufficient. The postulated relationship "if pigs had wings they'd fly away" does not qualify as a scientific theory until one has shown how it can be related to experience. Machlup recognizes this when he says that "we always combine the postulated relationship (which constitutes the theory) with an assumption of some change or event" [1, p. 733] to deduce consequences that can be tested empirically. If we *always* do that, it follows that *the possibility* of such a combination is an additional necessary condition that must be satisfied before a postulated relationship qualifies as a theory. The postulated relationships must be subject to the possibility of empirical testing.

The combination of a postulated relationship with a particular set of observed changes or events in the complicated and messy world of reality necessitates recourse to "impure" conditions that involve deviations from the "pure" theory of imaginary limiting cases like perfect gases or smooth surfaces or dimensionless particles or infinite elasticities. The possibility of making adequate allowances for such deviations in the observed changes or events, or in the observations testing the deduced implications, may be said to constitute the "realism" of a theory. I do not think Machlup would question the desirability and importance of "realism" in this sense.

Any set of postulated relationships can usually be combined with many

different sets of assumed changes or events, and the more there are of these, the more useful the theory. The theory is, therefore, "wider" than any particular application of it, and the wider the better. Such width makes it harder to identify the theory with any particular application of it to the real world, and may be said to constitute the "unrealism" of a theory.

"Realism" and "unrealism" are lively and attractive words, but they constitute a trap. They sound like contradictory qualities, but they mean nothing more than "applicability" and "generality"—less colorful (and less calorific) words perhaps, but guiltless of suggesting a nonexistent contradiction. "Realism" furthermore easily comes to stand not only for being *subject* to empirical testing, but also for the ability to *survive* such a test; and "unrealistic" to mean either "*not subject* to empirical testing" or "*failing* in an empirical test," (which, of course, implies that it *is* subject to empirical testing).

Samuelson's denial that a theory "is much wider than any of the consequences deduced" [2, p. 736] cannot be taken seriously unless he would claim that his factor-price equalization analysis has no applicability outside the consequences deduced about post-Marshall-Plan Europe; and his denial that theories can ever explain anything but can only describe cannot be taken seriously until he denies that his analysis even helps to explain the European economic recovery.

The logic can be saved by reading "any of the consequences deduced" as "all the consequences deduced," and defining "theory" to include all the conceivable assumed changes and events that can be combined with the postulated relationships as well as all the inferences that would follow from all the combinations, including all the adjustments that would have to be made for the "imperfections" or complications of the real world. But nobody would ever finish spelling out such a theory. Only a drastically brief abstract hint at all these relationships could ever serve as a *usable* theory. The truth of the abbreviation does imply the truth of all the implications but it is only the abbreviation that is a usable theory, and the genius of scientific advance consists precisely in the invention of such simplifying abstractions. The *realism* of the theory depends on the possibility of allowing accurately enough for the differences between the actual world and the abstract model. It does *not* mean that the abbreviated hint at the infinitely complex relationships, the model that is simple enough for human beings (or even computers) to be able to use, must consist of, or contain, an accurate description of reality. This is yet another possible meaning of "realism" and the one that Machlup so strenuously and properly objects to.

But Samuelson does not really insist that useful theories have to be "realistic" in this last sense. For him a theory is "realistic" if it passes the empirical test. This is clearly shown by Samuelson's attributing his successful prediction of European economic recovery to "the degree of realism of the model" [2, pp. 737-38], and the model he uses is an extremely abstract one, by no means intended as an accurate description of the world in which the European economic recovery took place.

The desirability of realism in this sense would not be disputed by Machlup

or by any F-twister, but Samuelson's suggestion that an F-twister would have said that a theory "is even better for its inadequacy" [2, p. 737] can be explained only by the theory that men are beguiled by words, and especially when they leave their work to indulge in methodological disputation. This general theory can be applied to the particular case of Samuelson trapped (or playfully allowing himself to be caught) by a pseudo-transitivity from "width" via "unrealism" to non-"realism" to "inapplicability" to "failing" (inadequacy) in an empirical test, so that welcomers of *width* are taken to be advocates of *error*.

"Unreality" in the sense of "width" or generality of a theory is appreciated by everybody, not only by F-twisters. Non-"realism" in the sense of the failure of a theory in an empirical test, sets F-twisters, just like everybody else, to look for a better theory. Meanwhile, until the better theory is found and established, the old theory continues in fairly good standing, made use of by everybody, not only by F-twisters, on the "wider" theory that there may have been some error in the test.

My conclusion is that there is no real difference between Samuelson and Machlup and F., and that they should all go back to their work where they are "all of them right."

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Professor Samuelson on Theory and Realism: Comment

To work at the boundary of disciplinary competence is, almost by definition, to engage in the philosophy of science. Such activity is exciting and rewarding, but it is also precarious. The risks involved are twofold, since one can miss the boundary either by not going far enough or by going too far. One who does not go far enough (i.e., one who, though actually well within the domain of disciplinary competence, believes himself to be at the boundary) either will inject philosophical puzzles into the substance of his discipline or will wrestle philosophically with substantive issues. The result in either event is bad science masquerading as philosophy. Yet one who goes too far (i.e. one who, though well within the province of metaphysics, takes himself to be straddling the disciplinary boundary) will import foreign empirical considerations into the interior of ontology. The result, bad philosophy masquerading as science. Although exceedingly difficult to survey, the boundary of a discipline must be clearly staked out. And, if both bad science and bad philosophy are to be avoided, that boundary must be dutifully observed by all parties.

The foregoing remarks are by way of apology. For whenever, as I am about to do, a noneconomist *publicly* enters an on-going hassle among leading economists like M. Friedman, P. Samuelson, and F. Machlup, the public is entitled to demand his credentials. Now if the dispute were wholly within economics, my credentials, which are philosophical, would be unacceptable. But in fact the issues which these distinguished economists are debating bear upon the nature of positive science in general and on the nature of positive economics in particular. They concern, among other things, the semantic relation of experience to theory and the place of mathematics and formal logic in the structure of empirical science. They are, in short, issues which lie at the boundary of economics, issues which happen to be the stock-in-trade of the philosopher of science.

Having given my excuse, let me now make my proposal. I propose to do two things: (1) to present a semantical analysis of scientific theories, which I hope economists will find informative; and (2) to extract from this analysis certain methodological maxims or morals which I hope economists will find useful. Or, to put it in a nutshell, I intend to relate a theoretical story that has a methodological moral.

I.

Let me begin by recalling J. N. Keynes's distinction, lately reaffirmed by Friedman, between positive science and normative science. Keynes characterized *positive science* as "a body of systematized knowledge concerning what is" [5, p. 34], and he portrayed *normative science* as "a body of systematized knowledge discussing criteria of what ought to be" [5, p. 34]. It is only the former, viz. positive science, that I wish now to discuss.

In a positive science, one can identify certain structural components. There are, first of all, the postulates. The *postulates* of a science—they are sometimes called *assumptions* or *primitive propositions*—constitute its systematic core. From them, by way of logical and mathematical implications, flow all the remaining propositions of the science which are called its *theorems* or *derived propositions*. Thus all the propositions of a science are compressed, so to speak, into its postulates, which are usually relatively simple and few in number. By another name this logical compression of the theorems into a handful of simple postulates is known as *systematization*. All of us are familiar with the postulates of at least one highly systematic empirical science, viz. Euclidean geometry. (I am of course construing Euclidean geometry, not as a formal theory of abstract objects called points, lines, planes, etc., but rather as a factual or empirical theory of mensuration.) By *Euclidean geometry* let us understand the set of theorems of Hilbert's celebrated axiomatization thereof [4]. When Euclidean geometry is so understood, it is largely fortuitous that a particular proposition is a postulate of some given formulation thereof. The reason, obviously, is that there are many, in fact infinitely many, postulate sets for Euclidean geometry, many of which bear little or no resemblance to the particular set of postulates advanced by Euclid himself. (Euclid's axioms, by the way, are formally deficient.) This logical fact, viz. that what is a postulate in one systematization of a science may be a derived the-

orem in another, might be called the *relativity of systematization* or the *relativity of postulation.* Now one of the issues of the methodological hassle mentioned above is the status of the "assumptions" of positive economics, i.e., the status of its postulates. Specifically, it is contested whether they should be "realistic" or "unrealistic." In the light of the relativity of postulation, we should expect this issue to be as relevant to the derived propositions of positive economics as to its postulates or assumptions. But allow me to defer examination of this issue for a few moments.

To derive theorems from a set of postulates, rules, known as *rules of inference,* are needed. A *theorem* is simply a sentence which is the last member of a proof, where a *proof* is defined as a finite sequence of sentences each of which is either a postulate or is inferrable from earlier sentences in the sequence by means of the rules of inference. Since theorems are sentences for which there exist proofs, it may appear somewhat paradoxical that theorems are rarely proved. Instead of a proof, what is usually proffered are considerations designed to convince a reasonable and well-informed individual that there exists a proof of the alleged theorem. Furthermore, the rules of inference of a scientific theory are almost never made very explicit. At best they are indicated by such loose phrases as "the rules of mathematics and formal logic." Now formal logic licenses the inference from a sentence A to a sentence B whenever the conditional sentence "If A, then B" is a truth of logic. It is imperative to notice that neither A nor B need themselves be logical truths. E.g., logic authorizes the inference from A, "Everyone acts so as to maximize his utility," to B, "Milton Friedman acts so as to maximize his utility," because the conditional "If everyone acts so as to maximize his utility, then Milton Friedman acts so as to maximize his utility" is a truth of elementary logic. Thus we see that

> Logic has its practical use in inference from premises which are not logical truths to conclusions which are not logical truths . . . and it is in this way that logical truth links up with extralogical concerns. Precisely the analogous account holds with regard to applications of mathematics generally; the tremendous utility of mathematical techniques in natural science turns simply on the importance of discerning mathematical truths of the form "If . . ., then . . ." whose component parts are statements of natural science [7, p. 7].

E.g., mathematics countenances the inference from Galileo's law, "$s = \frac{1}{2} gt^2$," to the conclusion "$ds/dt = gt$," since the conditional "If $s = \frac{1}{2} gt^2$, then $ds/dt = gt$" is a mathematical truth.

The use to which logic is put by scientific theories is naturally something quite apart from the logical properties of such theories, chief among which are consistency and completeness. When discussing criteria for choosing among several alternative hypotheses, each of which accounts for all known facts, Friedman asserts that

> Logical completeness and consistency are relevant but play a subsidiary role [subsidiary, i.e., to matters of simplicity and fruitfulness]; their function is to assume that the hypothesis says what it is intended to say

and does so alike for all users—they play the same role here as checks for arithmetical accuracy do in statistical computations [2, p. 10].

Here as elsewhere Friedman uses the word "hypothesis" as a synonym for "theory." Such usage is evident in the passage just cited, since completeness is an attribute of systems of propositions (hypotheses) and not of isolated propositions. Thus, when we bear in mind his idiosyncratic use of "hypothesis," Friedman seems to be saying that a theory is complete if "it says what it is intended to say." Let me clarify somewhat Friedman's notion of completeness which agrees tolerably well with the customary logical one. Let O_i be a class of sentences that one intends or wishes to be the theorems of some theory T. One might call O_i the *intended output of* T. Let the *actual output of* T, viz. the set of sentences which really are its theorems, be O_a. Now should $O_i = O_a$, T will be said to be *complete with respect to* O_i. In a system of economics, one might wish to have as theorems all true (or perhaps all highly confirmed) sentences that can be written in the notation of that theory. Nothing is more easily done if one doesn't mind having false sentences also included in the actual output of the system. [It is very easy to attain truth (just assert everything), as easy in fact as it is to avoid error (assert nothing). What is difficult is not the separate pursuits of these two goals but their joint prosecution.] But if the economic theory includes the concepts of elementary arithmetic, we know from Gödel's work [3, pp. 173-98] that it cannot be both consistent and complete with respect to the class of true sentences inscribable in the notation of the theory. The ideal of a (consistent) economic theory which embraces all the truths of economics is unattainable. Economists must perforce rest content with economic theories that are complete with respect to classes of sentences substantially short of the class of all true ones.

Although Friedman's concept of completeness approximates the usual logical one, his notion of consistency is quite unorthodox. We saw above that for Friedman a theory is consistent if what it says is "alike for all users." He thus seems to confuse consistency with *univocity* which characterizes, not theories, but single hypotheses. When a logician describes a theory as *consistent,* he means ordinarily that no sentence and its negation are both theorems of the system. This notion of consistency has obviously nothing to do with univocity, which signifies mere absence of ambiguity. Far from playing the "subsidiary role" in theory evaluation that Friedman consigns to consistency (i.e. univocity), logical consistency is an unqualifiedly necessary trait, the most basic one even, to be demanded of any satisfactory scientific theory. And one need not look very far to discover the reason why. For, if a theory is inconsistent, every sentence of it is a theorem. What Friedman can lightly dismiss as relatively unimportant in theory evaluation, then, is not consistency but univocity.

The importance of consistency is equalled only by the difficulty of establishing it. So difficult is it to establish consistency that, like legal innocence in a free society, a scientific theory is presumed consistent until proved otherwise. The burden of proof falls, not on the advocates of a theory to prove it

consistent, but on its adversaries to present evidence of inconsistency. Consider, e.g., the economic theory of demand. Does the fact that no one has demonstrated its consistency render it highly suspect of this logical felony? The answer, I believe, is "No." But how should one appraise this same theory were, on some future date, he presented with conclusive evidence that the theory is inconsistent? He would have to declare the theory quite unacceptable and either abandon it altogether or, what is more likely, modify it in such a way as to block the known ways of deriving contradictions within it.

I come now to the third and last component of a scientific theory to which I wish to direct attention, viz. its *semantical rules* or, as they are sometimes called, *rules of interpretation.* These rules confer meaning, either partial or full, on the postulates and theorems of a scientific theory. Without interpretative rules, a theory would reduce to an uninterpreted calculus which points to nothing beyond itself. It would be as empirically irrelevant as the game of chess. Friedman makes this same point somewhat cryptically as follows: "Viewed as a language, theory has no substantive content; it is a set of tautologies" [2, p. 7]. Now the prime function of interpretative rules is to *render fact germane to theory.* They stipulate, in terms of facts, partial or full truth conditions for the sentences of the theory, i.e., for all sentences that can be written in the notation of the theory. Put metaphorically, they legislate the official rates for converting theoretical paper money into factual coin. In virtue of the semantical anchorage effected by interpretative rules, the acceptance and rejection of the sentences of a theory become functions of observation and experimentation.

Though absolutely essential to a scientific theory, interpretative rules are seldom enunciated in a forthright manner. More often than not they are neglected altogether, with the result that someone who wishes to understand the theoretical assertions of a science must glean, to whatever extent he can, what the interpretative rules are by observing the practice and behavior of professionals, i.e., he is forced to learn these interpretative rules in much the same way that a carpenter's apprentice learns the rules of woodworking and construction. No doubt much of the polemics in the social sciences over the acceptability of certain theories can be traced to the failure to make these interpretative rules explicit. For, to the degree that these rules are made definite and explicit, the testing of a scientific theory becomes a matter of intersubjectively ascertainable fact.

What are the prospects in economics for eliminating the ills which stem from the failure to give theory a solid semantical anchorage in the bedrock of fact through precise and explicit semantical rules? In Friedman's opinion, not very good! According to Friedman, "One effect of the difficulty of testing substantive economic hypotheses has been to foster a retreat into purely formal or tautological analysis" [2, p. 11]. Accordingly, Friedman even foresees the contingency of economics degenerating into "disguised mathematics" [2, p. 11] and of substantive economic theory being reduced to a barren "structure of tautologies" [2, p. 11]. If Friedman's point is to be grasped, his words must not be taken too literally. For obviously there is no present danger of economics developing into a branch of mathematics in the sense that

mathematical truths (tautologies) are asserted as its sole postulates. But, if Friedman is right, there is danger, perhaps imminent danger, that the semantic linkage between theory and fact will be severed. The upshot of such a rupture would be, not a system of analytic or mathematical truths (tautologies in the broad logical sense), but rather an uninterpreted calculus. Though of interest to metamathematicians, uninterpreted calculi are of no use whatsoever to economists. And with respect to the degree to which they systematize observational sentences so as to make prediction, retrodiction, and explanation possible (the tripartite aim of positive empirical science), uninterpreted calculi and analytical mathematical systems are quite on a par. The degree in either case is zero. Perhaps the utter futility of both for purposes of prediction, retrodiction, and explanation prompted Friedman to warn of economics degenerating into "disguised mathematics" when he evidently meant to warn of it becoming an uninterpreted calculus.

II.

Bearing in mind the salient features of scientific theories underscored by the foregoing analysis, let us investigate more closely the semantical anchorage of scientific theories in order to avoid what the philosopher Gilbert Ryle has called the "Fido"-Fido fallacy [8, pp. 186 ff.]. This very fallacy, I will argue, lies at the heart of Samuelson's castigation of "unrealistic hypotheses" and causes him, in Machlup's words, "to reject *all theory*" [6, p. 733]. Consider the sentence "Fido has 4 legs." Now the services of a trained semanticist are hardly required to discover that that sentence is true if and only if Fido does indeed have 4 legs. Any English speaker who knows what the referent of the name "Fido" is knows how to verify whether or not that sentence is true. The semantical rules implicit in English usage link the truth-value of that sentence to very elementary observations in a perfectly straightforward way. Just to have a name for them, let us call *basic sentences* those sentences of English to whose truth-values experience is germane in the same direct and immediate way as it is to our paradigm "Fido has 4 legs."

Let us now consider another sentence which superficially resembles our paradigm, viz, the sentence "This volume of gas (1 cc.) contains 4.5×10^{19} molecules." Puffed up by our success in analyzing the first sentence, we might repeat our previous analysis and pronounce, with appropriate gravity, that the second sentence is true if and only if this volume of gas really does contain 4.5×10^{19} molecules. But will an English speaker, supplied with our analysis, know how to set about ascertaining whether it is true or false? Of course not, unless he is a trained scientist, for the semantical rules of English do not bind this sentence directly to experience. This sentence, therefore, does not belong to the class of basic sentences. Although its ties to experience are neither direct nor immediate, unless this sentence has some rapport with experience, unless experience is somehow germane to its truth-value, it has no factual significance at all. For surely sentences with no experimental ties whatsoever must be judged empirically meaningless. Again to have a convenient label at our disposal, let us refer to sentences which, though tied semantically to experience, do not enjoy that same direct relation thereto that character-

ized the basic sentences, as *theoretical sentences*. Our present problem, then, is to clarify the manner in which experience is germane to the acceptance (as true) or the rejection (as false) of theoretical sentences of the likes of our exemplar "This volume of gas contains 4.5×10^{19} molecules." A brief biography of the theory in which this particular theoretical sentence is embedded may perhaps shed some light on the problem.

About 1808 Dalton proposed his atomic theory of matter of which the following were some of the leading postulates:[1]

(1) Matter consists of indivisible atoms.
(2) The atoms of a given chemical element have identical properties.
(3) The atoms of different elements differ in weight and in other characteristics.
(4) Atoms are indestructible.
(5) Chemical reactions are simply rearrangements of atoms.
(6) A compound substance consists of "compound atoms" each of which contains a small but definite number of atoms.

On the basis of these and other similar postulates Dalton was able to explain a large number of well-known chemical facts. *Inter alia,* he explained the existence of distinct chemical elements, Boyle's law, Proust's law of definite proportions, and the conservation of mass in chemical reactions. All these "facts" were accounted for by Dalton's theory in the sense that they were logically and mathematically deducible from it, although some of the derivations depended on such mediating assumptions as that a gas is composed of a large number of rapidly moving but extremely small atoms. Further confirmation for Dalton's theory came indirectly from the study of the behavior of gases to explain certain aspects of which Avogadro postulated (1811) that equal volumes of different gases contain the same number of basic particles. By identifying these particles (molecules) with clusters of Daltonian atoms, Avogadro made it possible to derive, by dint of rather complicated inferences, such properties of atoms as their size, weight, and even their number. In particular, Avogadro was able to infer that one cc. of gas contains about 4.5×10^{19} molecules. Now clearly the evidence on which Avogadro accepted the sentence "One cc. of a gas contains about 4.5×10^{19} molecules" bears no direct semantical relation to it. It consists, firstly, in all the true basic sentences derivable (in many instances inductively) from the atomic-molecular theory and in the negative fact that no basic sentences so derivable were found to be false. There is, secondly, the logico-mathematical fact that the theoretical sentence in question is deducible from that theory, together with certain basic sentences accepted as true. Given the way this theoretical sentence is related to the evidence for it, it appears that to say that this sentence is "true" is tantamount to claiming that there exists such evidence for it. To regard it thus as true does not entail that one believes that there really are tiny little particles 4.5×10^{19} of which are crammed into a space no larger

[1] This account of Dalton's postulates and the atomic theory of matter is taken substantially from A. French, "Atom" [1].

than the tip of your thumb. The rigid belief that acceptance of a sentence commits one to the existence of objects which serve as referents for its denoting expressions and which have the properties designated by its general terms is Ryle's "Fido"-Fido fallacy, i.e. the fallacious inference from the sound employment of a name to an object which it denotes. Applied to basic sentences, a "Fido"-Fido principle of meaning is perhaps unobjectionable. It supplies, in fact, the rationale for our analysis that "Fido has 4 legs" is true if and only if Fido has 4 legs. Philosophical mischief begins only when the principle is extended to theoretical sentences. If we were to christen as "naïve realism" the view that the "Fido"-Fido principle applies with equal force to all sentences, the thesis here outlined which restricts that principle to basic sentences might well be called "critical unrealism." At the cost of being dubbed an unrealist, let me assert unconditionally that theoretical sentences are inherently unreal in the sense that experience is germane to their acceptance only in the indirect and devious way described above.

Friedman, I believe, holds the same opinion about theoretical sentences, but he expresses it in a somewhat misleading way. He rightly assails those who, like Samuelson, would make the "realism" of a postulate set a separate and distinct test of its acceptability, separate and distinct, that is, from its predictive, retrodictive, and explanatory adequacy. But excess of righteous indignation prompts Friedman to laud postulate sets which are "descriptively false in (their) assumptions" [2, p. 14]. Now the most significant feature of theoretical sentences like Dalton's postulates is that they are neither descriptively true nor descriptively false. Those adjectives are properly applied only to basic sentences, i.e. to sentences behind the denoting phrases of which stand bona fide referents. Indeed, if, as we shall see in a moment, Samuelson's mortal sin is to reject all theoretical sentences, Friedman's venial sin is to liken them much too closely to basic sentences. Friedman's overly Baconian model of theory formation leads him to assimilate theoretical sentences to basic sentences, for he regards theory-formation as fundamentally an abstractive operation. He conceives it much too narrowly as the practice of devising "theoretical generalizations to fit as full and comprehensive a set of related facts about the real world as it is possible to get" [2, p. 300]. Or, as he succinctly puts it, "A hypothesis is important if it 'explains' much by little, that is, if it abstracts the common and crucial elements from the mass of complex and detailed circumstances surrounding the phenomena to be explained" [2, p. 14]. But surely the end-product of such an abstractive process of generalization would be *basic sentences*. Only by injecting the misleading thesis that the basic sentences so abstracted are "descriptively false in their assumptions" can Friedman pretend to segregate them from the other basic sentences.

Thus, while we can concur with Samuelson's vehement denunciation of Friedman's "unreal hypotheses" [10, pp. 736-39], i.e., descriptively false postulates, we must reject Samuelson's reasons for denouncing them. Samuelson censures Friedman, not for likening theoretical sentences too closely to basic sentences, but for positing any difference at all between them. "A lifetime of sad experience," says Samuelson, constrains him to advance the meth-

odological maxim that he calls *Samuelson's Razor,* to wit: "All economic regularities that have no common-sense core that you can explain to your wife will soon fail" [9, p. 235]. If we take a wife to be by definition someone who understands basic sentences but finds theoretical sentences incomprehensible, we can restate Samuelson's Razor in linguistic jargon as follows: any sentence that cannot be reduced, without loss of meaning, to basic sentences should be rejected. The upshot of Samuelson's Razor is, as Machlup has clearly perceived [6, p. 733], the complete eschewal of theory, for theoretical sentences are precisely those sentences that are not adequately translatable into basic sentences. To what basic sentences, for example, could one reduce Dalton's postulate that matter is composed of indivisible atoms? But let us not be duped; not all our maxims need rest on sad experience. That would be too pessimistic an approach to positive science. There are, after all, many happy experiences from which we can also extract maxims. If, e.g., there is any methodological moral to be drawn from the century-and-a-half-long successful life of the atomic theory of matter, it would seem to be that an empirical science comes to maturity only after it effects a clean break with basic sentences, only after it boldly postulates theoretical statements that ultimately are anchored, though not submerged, in experience by means of semantic ties to basic sentences. Thus I myself would proclaim the contrary of Samuelson's Razor, viz. all economic theories that can be explained to your wife are *theoretically* worthless. Or, in linguistic terms, when theorizing, aim for postulate-sets that cannot be reduced without loss of meaning to basic sentences. And furthermore, if one classifies as "unreal" all sentences that are not basic, I would urge that, when theorizing, one be satisfied with nothing short of unreality.

III.

In closing, let me try to anticipate, and thus ward off, a possible misunderstanding. I do not mean to institute a new and separate test for the acceptability of theoretical postulates, viz. their "unreality" or theoreticalness. My maxims are meant only to remind the theoretician to stick to his business, which is the contrivance of systems of *theoretical* sentences which can be used to predict, retrodict, and explain observable phenomena. Convinced of the central role which bona fide theoretical sentences play in the evolution of an empirical science, I desire only to make the theoretician acutely aware of the premium of his theoretical greenbacks so that no methodological swindler can get him to exchange them for a pocketful of basic coins.

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Professor Samuelson on Theory and Realism: Reply[1]

When first seduced to discuss methodology, I warned that it is a field in which Gresham's Law holds in the form, "Hot air drives out cold." I should also have predicted that it is an activity where the law of diminishing returns holds in the most virulent form. And yet it is a field where, as Editor Gurley has learned the hard way, every economist feels his ideas are as good as anyone else's. Though many call themselves, few are chosen: the papers of Garb, Lerner, and Massey are but the visible peak of the iceberg of submitted comments.

The discussion has progressed, I think, away from the issue of an F-twist that considers unrealism of a theory as a virtue, to controversy over the role and nature of "theory" and over the meaning of "explanation" as against "description." A Gallup poll count of the mail would seem to show that there is a widespread will to disbelieve in my rather hard-boiled insistence upon "theory" as (strategically simplified) description of observable and refutable empirical regularities, and a widespread hankering for a more exalted Machlup role of theoretical explanation. To stand one's ground against the majority merely in order to be provocative I regard as contemptible in a scientist. But candor requires me to state that the arguments of my critics are suspiciously diverse and unrelated: since what they have in common is primarily their dissatisfaction with me, they really do not provide a common alternative credo to which I could subscribe even were their arguments tempting.

I.

Alphabetically, Garb comes first. His discussion reminded me that there are worse movements than the zero movements of a deadlock—namely, movements backward in understanding and in isolating differences of opinion. Example: his long third paragraph simply misinterprets what I wrote. My approving of Hertz's assertion that Maxwell's theory *is* his equations represented

[1] I am grateful to the Carnegie Corporation for a reflective year, and to F. Skidmore for research aid.

no backsliding on my part: instead it emphasized the view that a description (equational or otherwise) that works to describe well a wide range of observable reality is all the "explanation" we can ever get (or need desire) here on earth. Careful reading of my words will show that I only insisted that the validity of the *full* consequences of a theory implies the validity of the theory and so of its *minimal* assumptions. Had Garb added the words "full" and "minimal," the alleged disagreement with Machlup would have disappeared. Example: when Garb says "an explanation does not have to be 'ultimate,' " I think he is resaying what I said, not controverting it. *An explanation, as used legitimately in science, is a better kind of description and not something that goes ultimately beyond description.* Example: the final beautiful quotation from Einstein is an irrelevancy. It has to do with the technical issue of whether in quantum mechanics probability statements can be replaced by causality statements like those of classical mechanics. It has nothing to do with methodological differences as to the role of theory itself. So much for Garb.

II.

Lerner's paper shows that many a false word has been said in jest. He in effect says that Samuelson and Machlup both do good work, acting alike in their practical work during the six days of the week. Therefore on Sunday they can't be in genuine disagreement on methodology. By this reasoning, a thousand philosophers err in thinking that Ernst Mach and Arthur Eddington differ diametrically on the methodology of physics: since both men contributed mightily to the Gross National Product of physical knowledge, why pay attention to the detail that Eddington thought he could deduce from the laws of thought (!) the number of electrons in the universe, while Mach claimed the scientist merely discerns mnemonic devices for describing brute facts. I remind Lerner that the aphorism, "All wise men believe in the same true religion," is an atheist's joke not a pithy truism.

If Lerner had read with sympathetic care my writings, he would have seen at the beginning what it was I denied—namely, that "a theory is much wider than any [in the sense of 'any and all'] of the consequences deduced." Precisely because of the ambiguity of words like "any," I used the unambiguous logical symbols of set theory (p. 234 of my original article [8]); by attentive reading of this passage, he could have been spared the labor of "Saving my logic," particularly since I have to reject his charitable rescue. It is nonsense, and not *my* nonsense, "to include all the conceivable assumed changes and events that can be combined with the postulated relationships as well as. . . ." Moreover, Machlup and I are one in objecting to the requirement that a usefully realistic theory must be completely accurate. Who seriously thinks otherwise? Lerner regards it as Sunday nodding on my part to think that anyone could be so stupid as to believe that a theory "is even better for its inadequacy." He does not disagree that such an assertion is stupid. Having been at pains to state the matter so plainly, I am glad to learn the conclusion is *now* obvious. Lerner's final conclusion represents what the logicians call a *non sequitur:*

if in the end it turns out there is no real difference between Samuelson and Machlup, this will follow from considerations not discernible in Lerner's note.

III.

I turn now to the *pièce de résistance,* Massey's valuable paper. I have no quarrel[2] with Massey's Part I, which provides a masterly summary of the semantics of scientific analysis that will be useful to all economists and novel to most. In Part II, the impractical philosopher shames the practical economic scientist and puts his finger on the one critical issue in the present controversy—the relationships between observable reality and the various assertions made by the scientific theorist. Part II makes, I think, a definite contribution to the discussion;[3] but I must confess that, in reading it, I was overtaken with aspirations of grandeur, and came to wish that I were Massey and could rewrite Part II. For as it now stands, it could be misinterpreted by the casual economist reader to exalt "indirectly observable" empirical regularities over "directly observable" empirical regularities. Concretely, the reader might mislead himself into believing that the special theory of relativity, which is founded on directly observable empirical regularities, is intrinsically less noble or less something than the kinetic theory of gases, which is based upon allegedly nondirectly observable tiny molecules; and indeed that this kinetic theory is better than the classical theory of thermodynamics *merely because* the first and second laws of classical thermodynamics are so beautifully capable of direct empirical confirmation in every steam engine or housewife's kitchen. So let me restate the issues.

Hempel [3, p. 23] denies the following assertion: "Any term in the vocabulary of empirical science is definable by means of observation terms." A reader like Massey might be forgiven for thinking that I differ from Hempel on this point and therein lies my heresy. So let me make the following clear: when I read the grounds on which Hempel denies this doctrine (which he calls the *narrower thesis of empiricism* [Hempel 3, p. 24]), I find no reason to differ on this point with Hempel or Machlup or anyone else. For what are Hempel's difficulties with such an assertion? They are of the following type. Point (i): Scientists say an object is "magnetic" and mean by this that, if a small iron object is put near it, that object will be attracted by it. But what if

[2] Almost none. I doubt that "consistency" provides the problem for any science like economics or physics that it provides for the mathematical logician concerned, say, with the consistency of Hilbert's axioms for arithmetic. The status of the special theory of relativity or of consumer demand was the same after Gödel as before. To say that we take consistency of a scientific theory for granted because it is so devilishly hard to prove this fact is like saying we assume a woman virtuous unless proved otherwise because it is so hard to prove any woman lacking in virtue. That is surely not so: Bayesian probabilities, that most women are virtuous in our culture and that few scientists have won undying fame for themselves in finding flaws of consistency in important scientific theories, suffice to explain why we take for granted what we do.

[3] To help my understanding of these issues, Professor Massey kindly provided me with minimal references to related writings that should be of interest to economists, namely the Hempel and Nagel references of my bibliography. I benefited from this course of study, which I believe deepened my understanding of my own position.

no iron object is found near it? Is it an empirical statement that it is (still) magnetic? Point (ii): Scientists use metrical terms like "the length of this stick can be any real positive number" or "the apple had an instantaneous velocity of so much at noon and an acceleration of this definite magnitude."

Well? Narrow empiricism is supposed not to be able to handle these two difficulties, whereas a *"liberalized thesis of empiricism"* [Hempel 3, p. 31] can. O.K., then I am a liberal empiricist. The concept of limits—in which one does not have to go *to* the limit—enables us to go from integral- to real-number systems and from average velocities to instantaneous derivatives; hence, point (ii) offers no room for controversy here. And, while I am prepared to believe that point (i) has presented a problem to the philosopher trying to represent scientific procedures, I find it hard to imagine any two modern physicists or economists ever having a substantive dispute about such a matter.

I elaborated on the above because the reader of Hempel might easily be tempted to think that the demonstrated difference between narrow and liberal empiricism has something basic to do with Hempel's repeated contrast between (a) description and (b) prediction and explanation. I was amused in reading these authors through a magnifying glass to see how Freudianly guiltily they (with the exception of Braithwaite) shy away from validly distinguishing description from explanation. A *description* of an empirical *regularity* provides the basis of *prediction,* which will be as accurate or inaccurate as is the regularity being described. Because prediction is good and a case is being made for "explanation" as something beyond "mere" description, prediction is again and again coupled with explanation and gratuitously demarcated from description. In this too-brief discussion I hope to have cast doubt on Hempel's Points (i) and (ii) as a proper basis for *this* distinction.

When we move on to Nagel and to Massey, we find the alleged basic distinction in the difference between "directly observable" empirical regularities (or descriptions) and "indirectly observable" regularities. In Nagel the tone is rather tentative; in Massey, an imperialistic vocabulary is introduced in which all directly observable empirical regularities are squeezed out of "theory" completely (being called "basic sentences"); and indirectly observable regularities are alone called "theoretical sentences." That this is a perversion of usage can be demonstrated by the following examples. And if one agrees with the point these examples are designed to make, one suddenly finds Samuelson innocent of such grave charges as "rejecting *all* theory."

Consider four of the most important physical theories of all time, by any physicist's admission. (1) Galileo's analysis of a falling body; (2) Newton's theory of universal gravitation as applied to the *n*-body problem; (3) Einstein's special theory of relativity; and (4) classical thermodynamics of Carnot, Clausius, Kelvin, and pre-1900 Gibbs. Later add a fifth theory, (5) the kinetic theory of gases (and various models of statistical mechanics).

I have checked with several physicists and find they agree with me that the first four of these historic theories are expressible completely in terms of Massey's "basic sentences" alone.

Example: if S_t represents the position of a rolling ball on an inclined plane at time t, Galileo's law of gravity can be expressed in the directly observable hypothesis,

$$(S_{t+h} - S_t) - (S_t - S_{t-h}) = 0 = \Delta^2 S_t = d^2 S_t/dt^2.$$

Example: if the coordinates of the 2-body problem are depicted by the usual notation, Newton's behavioral equations become

$$m\ddot{x} = -GMmx/r^3, \qquad m\ddot{y} = -GMmy/r^3, \qquad r = (x^2 + y^2)^{1/2}$$

and can be verified directly by acceleration measurements, without indirect checking of elliptical orbits and Kepler's Law. Indeed for $n > 2$, the differential equations are not integrable in terms of elementary functions but the differential equations can be directly tested.

Example: Einstein built the axioms of special relativity on the precise direct observations of the Michelson-Morley experiment, extrapolated to every observer of light no matter what his uniform velocity with respect to any other observer. (Incidentally, Professor Royall Brandis of Illinois pointed out to me that the observations of the perihelion of Mercury provided an indirect test of the *general* theory of relativity and not, as I erroneously said, the *special* theory.)

Final example. The most high-fallutin' theoretical formulation of classical (nonstatistical) thermodynamics is certainly that of Carathéodory of 1909. If *it* is not a theory, the word theory is surely being redefined in a way strange to all usage. Yet, in terms of Massey's new definition of "theoretical sentences" ["which, though tied semantically to experience, do not enjoy the same direct relation that characterizes the basic sentences" (such as "Fido has 4 legs" or "the velocity of this ball increased proportionally with time since it was relesased")], Carathéodory turns out to be "eschewing all theory." For Carathéodory's symphony begins with the proud words, "One of the more notable results of the investigations made in thermodynamics during the last century is the realization that this science may be developed without any hypotheses which cannot be empirically verified." He goes on to squeeze out even the concept "heat," as superfluous, and builds directly on the empirical observations of Joule and Clausius. (It is true he uses ideal concepts of "insulating walls" and the like, but properly refers to "thermos bottles" that approach the realization of such concepts. My own position never rejected approximating concepts.)

Since Hempel, Nagel, Massey, Machlup, and all sensible men argue from empirical observation of what celebrated scientific theories have been like in the past, I consider it fair game to give these examples in order to reject as tragically misleading Massey's key conclusion at the end of his Part II, of which I requote only two sentences.

> If, e.g., there is any methodological moral to be drawn from the century-and-a-half-long successful life of the atomic theory of matter, it would seem to be that an empirical science comes to maturity only after it effects a clean break with basic sentences, only after it boldly postulates theoretical statements that ultimately are anchored, though not sub-

merged, in experience by means of semantic ties to basic sentences. Thus I myself would proclaim the contrary of Samuelson's Razor, viz. all economic theories that can be explained to your wife are *theoretically* worthless.

* *

Never, never make a joke. My remarks about Samuelson's Razor, and my experience that only the simple theories that can be explained so as to make sense to an intelligent outsider (one's wife) turn out to hold up in economics, was intended as a humorous *obiter dictum* as far as the controversy over the role of theory was concerned. My remark referred to theories of stock market prediction like the famous Gridiron Law: "In every year that Harvard beats Yale and there is confirmation by Penn beating Cornell, the Dow Jones averages will rise." Jobbing *backwards*, we find this fits the facts of 1920-40: but what scientist was surprised when it failed in the postwar? It represents a bad description in terms of width of empirical coverage, in contrast to the following: "When Kennedy's fiscal policies send up GNP, that's good for profits and makes me think we ought to invest in common stocks that do not already have a high price-earnings ratio and which stand to share in the general prosperity." You can call the latter an "explanation" of equity price trends, but that does not deny me the right to formulate it as a description of regular concomitance of fiscal policy variables, GNP changes, profit changes, and equity price changes.

What about case 5 of the kinetic theory of gases, which I agree differs from the first four cases and involves the indirectly observable hypotheses that Kempel, Nagel, and Massey talk about? Here we do need some less imperialistic name than Massey's "theoretical sentences." Let me henceforth refer to them as "indirectly observable" sentences or Sentences_2 in contrast with Sentences_1 = Basic Sentences. I gladly welcome both kinds of sentences and wish that the philosophers of science had probed more deeply into their nature. Thus, Kepler's 3 laws might legitimately be considered as indirect ways of verifying Newton's inverse square law of acceleration. If we call them Sentences_2, we should realize (Cf. Born, [1, p. 2]) that it can be easily demonstrated that the three Kepler Laws *together* (elliptical orbits in one plane, radius sweeping equal areas in equal time, period of revolution of any planet around the sun proportional to the 3/2 power of its distance from the moon) are necessary and sufficient conditions for the truth of Newton's 2-body acceleration laws, which are Sentences_1. So such indirectly observable sentences do differ from the following kinetic-theory ones.

Suppose I tell my wife that the air in that transparent balloon consists of small molecules averaging 600 miles per hour—about the speed of a bullet. Why should she believe me? If only macroscopic data about balloon pressures and volumes are observed, she needn't. "But," she says, "I read in a book there are 4.5×10^9 molecules in each c.c. of the air in the balloon." I reply, "Suppose there are. Without a microscope you couldn't possibly detect that fact, and separate it from the fact that there are 4.6×10^9 or 45×10^9. But

you can be sure that there are certainly not as few as 4.5 × 10 heavy molecules; for if there were, you would see the balloon pulsing not to mention lots of other things." Being a sensible woman, she would reply, "So long as there are no observable consequences to me of there being 4.5×10^9 or 45×10^9 molecules, I am completely indifferent to your assuming or not assuming the existence of that many. And how did you get that odd idea?" My reply, "By reading Greek poetry of Democritus and Lucretius. By thinking that endless paddling of water warms it up, which suggests (to me and Count Rumford) hotness must somehow be related to motion rather than being a substance. By finding that postulating a sufficiently large number of hard, small balls gives me (and Maxwell) a model whose directly observable *average* energy of collisions on the wall of the balloon agrees numerically with the directly observable pressure on the walls of the balloon. By finding that mixing balls twice as heavy with the original balls gives me *average* energy of collisions on the wall agreeing with the surprising observable fact that mixing together a balloon of light gas with one of heavy gas gives me a predictable pressure of the mixture satisfying the so-called Dalton Law of Partial Pressures. It is an agreeable fiction, if you please, that there are 4.5×10^9 molecules *or more*. And this fiction usefully prepares me for what happens when I crush the balloon. Now the balls get closer together; now I can't neglect their Newtonian attractions; and now the model predicts that halving the balloon's observable volume will *less* than double the observable pressure. And this agrees with directly observable descriptions, such as the gas in the balloon becoming liquid."

Up to this point, say 1897 when Planck first wrote up his treatise on *Thermodynamics*, one could regard the Sentence_2, "This gas has a given number of molecules," as merely a convention. Mach, Stallo, and Ostwald too stubbornly held to such a view—too stubbornly, I think, but not without excuse. Thus, Stallo said, why explain an elastic gas that you can see directly in terms of hypothetical elastic balls you can't see? One answer is, "Why not? To do so is harmless and it does help me remember by Newtonian calculation, if I should happen to forget, how observed gases behave." Mach, who insisted on the mnemonic nature of all scientific statements, should not have objected to this in principle. He should merely have said, "Don't forget that your results at the macroscopic level are quite independent of the number 4.5×10^9. And actually Planck [7, p. 26] said, "This, of course, need not be a whole number. . . ."

But now move into this century. Give my wife a microscope and show her Brownian motion. Have her look at a radium-dialed watch with an ordinary magnifying glass. Listen to a Geiger counter. Observe a Wilson Cloud chamber. Literally see a single atom with a field emission microscope trained on a tungsten point. Count molecules emitted from a radioactive substance. Observe photoelectric effects as chemical reactions on an exposed film. Now, though I do not insist on the point, she does observe and describe molecules in almost the same literal sense that anyone counts the 4 legs of a dog. Avogadro's Number now takes on a more concrete meaning: after Einstein and Perrin calculated it from Brownian movements, Mach and Ostwald properly threw in the towel. If they are to be criticized, it is in being too impatient

with Boltzmann's (at worst, harmless) calculations in the pre-Einstein era.

The philosophers of science should bring their most fashionable example of indirectly observable theoretical concepts up to date. In terms of modern techniques of measurement, atoms are no longer a-empirical concepts; neither in 1965 are genes and chromosomes in the theory of particulate genetics.

IV.

Let me summarize. There has been no successful demolition of my view that science consists of descriptions of empirical regularities; nor of my insistence that what is called an explanation in science can always be regarded as a description at a different level—usually a superior description in that it successfully fits a wide range of empirical regularities. This, in my view, is the proper basis for the Braithwaite-Ramsey distinction [in Braithwaite] between a superior description that can be given the honorific title of "explanation" and an ordinary description. My detailed analysis of the molecular theory of gases permits me now to amplify the logical symbolism of my original paper. Let the full assumptions of the molecular kinetic theory be called B. Let its full consequences be C, which can be broken down into its macroscopic consequences C_1 (pressure, volume, etc.) and into its microscopic consequences C_2 (standard deviation of Brownian movement, etc.). If the observed facts confirm both C_1 and C_2 (as after 1910, it became known that they do), no one will quarrel with the theory B. Suppose, as before 1900, the known facts confirm only C_1? What then? So long as the theory's C_2 implications are not known to contradict empirical data, the kinetic theory B is no worse (except perhaps aesthetically) than some rival theory β, whose *full* implications are also the confirmed C_1. Occam's Razor suggests, "Choose β rather than B, other things being equal." But Occam's Razor, as Einstein almost said, like elegance is for tailors. Other things may not be equal, as for example the superior ability of B to be "remembered" by virtue of its "simplicity" or deducibility from theories applicable to other areas of empirical reality (e.g. Newton's laws applied to colliding elastic particles).

A harder problem is one where C_1 of B is confirmed but C_2 of B is refuted by the facts. How big must the discrepancy of C_2 from the facts be to make us want to drop B? That always depends on the "importance" attached to discrepancy and agreement. I can imagine cases where scientists would say, "B has C_2 implications that are unmistakably refuted by the facts at the microscopic level. But who cares about such discrepancies, which do no harm and are unimportant. Because B predicts the important truth C_1, we stick with it as a theory." I cannot quarrel with this if (i) scientists are truly agreed that C_2 discrepancies are unimportant and (ii) there is no readily available truncated theory, B-, which can be distilled from B by lopping off its C_2 aspects and which can provide pretty much the same mnemonic patterning of the facts that B does. My original quarrel with the F-twist was a reminder of the importance of (i) above. This should not be controversial.

Where I must be careful not to overstate my case is in connection with the following: After a theory's implications C_1 are well confirmed by the facts, and even prior to our testing its C_2 implications, we increase our Bayes-probability belief in the likelihood that its C_2 implications will be confirmed.

This heuristic principle seems to be well founded by past experience and reflects, I suppose, a bonus in the form of an underlying simplicity of Nature which—if it had not existed—could not be legitimately invented.

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Uncertainty and the Welfare Economics of Medical Care: Comment

In their exegesis of K. J. Arrow's discussion of the welfare economics of medical care [1], Professors Lees and Rice reach the conclusion that ". . . the greater the X, given p, and the smaller the p, given X, the more likely will it be that $(V\text{-}C)$ is nonnegative" [2, p. 146], where X is the amount of a possible loss, p its probability of occurrence, V the value to the individual of complete insurance against the loss, and C the cost (in excess of actuarial cost) of the insurance. This conclusion violates the commonly held view that events with a very low probability of occurrence, as well as those with a very high probability, are not worth insuring against. In fact, this conclusion appears to be more consistent with the analysis of Lees and Rice than the one they arrive at, given the presence of indivisibilities in both sellers' and buyers' costs for placing insurance.

Buyers' costs such as ". . . the transactions cost to the individual of completing and filing application and claims forms, paying premiums, keeping records, etc." [2, p. 143] as well as many corresponding items of sellers' costs are largely indivisibilities, showing relatively small variation with either the size of a policy or the probability of loss. The Lees-Rice "cost of insurance" function in this case would take the form $k_1 + k_2pX$ or $k_1 + k_2p$ where the k's are constants. The first form of the function is illustrated in Figure

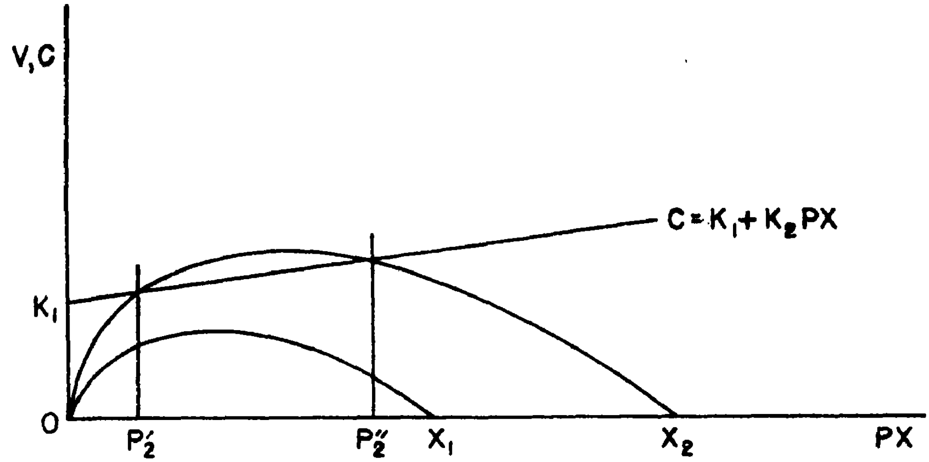


FIGURE 1

1. It is apparent that the risk with the smaller value would not be worth insuring against regardless of the probability of loss, while that with the larger value would be worth insuring against only if p_2 lies between p_2' and p_2''. Thus very small probabilities of loss as well as very large probabilities would militate against insuring against a loss in the presence of indivisibilities in the cost function, and sufficiently small losses would not be worth insuring against at any level of probability.

This analysis provides a justification of "package" insurance policies, as a loss of low value or low probability may be added to a "package" with no addition or a very small addition to k_1. Similarly, a group of small or low probability losses may justify insurance as a package, though none of them would justify it individually.

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Pattern of Income Distribution in an Underdeveloped Economy: A Case Study of India: Comment

In a recent issue of this *Review* P. D. Ojha and V. V. Bhatt (hereafter O-B) presented data on the income distribtuion of India [6] which purport to show the pattern of income distribution is less unequal in India than in some of the developed countries. If this finding were correct, it would throw serious doubt on Kuznets' generalization that income tends to be more unequally dis-

tributed in the developing economies than in those which have attained some degree of maturity [3]. Indeed it might suggest that comprehensive economic planning of the Indian type readily overcomes the tendency toward inequality of income, which in most cases seems to be associated with the development process. Unfortunately O-B tell the U. S. reader little about the derivation of their data, except to refer him to the *Reserve Bank of India Bulletin.* Examination of the available Indian statistics discloses that (1) the assumptions made by O-B in estimating the Indian income distribution lead to a significant understatement of income inequality, and (2) they ignore an important body of data which contradicts their estimates and therefore their conclusion.

The omitted evidence is a household survey of Urban Income and Saving, conducted by the National Council of Applied Economic Research of New Delhi (hereafter NCAER) in 1960 with a stratified probability sample of about 4,400 families in 30 cities and towns all over India [8].[1] Table 1 compares the O-B income distribution with that obtained by the NCAER survey. It also presents the 1959 U.S. distribution for purposes of comparison. We find that the NCAER income distribution is much more unequal than the O-B distribution. The concentration ratio for personal income in the urban sector is .40 for the O-B data, compared with .51 for the NCAER distribution. The share of the upper quintile is 49 per cent according to O-B, but 58 per cent according to the NCAER. These discrepancies are sufficiently large to lead to almost opposite conclusions: If the O-B estimates are correct, the con-

TABLE 1—DISTRIBUTION OF INCOME IN URBAN INDIA AND IN THE URBAN UNITED STATES
Shares of Ordinal Groups of Households

Households	Urban India		Urban U. S.
	O-B (1953–57)	NCAER (1959–60)	(1959)
Top 5 per cent	26%	31%	16%
Top tenth	37	43	31
Second tenth	12	15	17
Third tenth	10	10	12
Fourth tenth	8	8	11
Fifth tenth	8	7	9
Sixth tenth	6	5	7
Seventh tenth	6	4	6
Eighth tenth	6	4	4
Ninth tenth	4	3	2
Bottom tenth	3	1	1
Concentration ratio	.40	.51	.43

Sources [6, p. 714, Table 1] [8, p. 44, Table 20] and 1960 Survey of Consumer Finances.

[1] Curiously, Ojha and Bhatt refer to this survey at one point in the underlying article in the *Reserve Bank of India Bulletin* [5, p. 1352], while at the same time disregarding its results. The NCAER data also are quoted extensively in [7].

centration ratio is not larger for urban India than for the United States or Great Britain. Despite the closeness of the concentration ratios, the shapes of the O-B and the U.S. distributions are quite different, however, with the O-B data showing higher shares in the top decile and in the three bottom deciles and the U.S. distribution displaying a correspondingly greater concentration in the second to sixth deciles.[2] If the more unequal NCAER distribution is correct, the degree of income inequality for India is close to the top of the observed range, comparable to that for other countries which are at an early stage of development, such as Ceylon, Mexico (1957), and Colombia [3, p. 13].

Fortunately, it is not necessary to leave the matter at this inconclusive stage. The O-B income estimates are *not* based on a survey or census of income. Rather, their starting point is the surveys of household *expenditures* conducted by the National Sample Survey. To this O-B add taxes and their own estimates of the distribution of household savings. The NCAER survey, by contrast, collected income data directly from households. It was modeled after the Survey of Consumer Finances, conducted by the Survey Research Center, University of Michigan. The same is true of the British survey, and the Ceylon and Puerto Rico surveys with which O-B compare their Indian data. The figures for practically all the other countries referred to also are derived from household surveys and censuses which inquired directly about personal incomes. Most economic statistics, whether survey data or broad aggregates, have a substantial margin of error. However, if the same methods of estimation are used over time or between countries, meaningful comparisons can nevertheless be made. Comparisons between data derived by basically different methods are much more dangerous. The greater similarity of the methods underlying the NCAER income distribution and those for other countries (cited in [6, Tables 3-5]) of itself should dispose us favorably toward comparisons with the NCAER data.[3]

One may check the distributional characteristics of the NCAER urban income data by subtracting from the personal income figures estimated taxes and savings (the latter determined by a separate sequence of questions). The resulting distribution of consumer expenditures can then be compared with the National Sample Survey distribution of expenditures for the urban sector. These comparisons show excellent agreement: the concentration ratio for consumer expenditures implicit in the NCAER data for 1959-60 is .42; for the NSS data for 1957 it is .39. Since savings, which are more unequally distrib-

[2] The very poor are underreported in both sets of Indian data in the sense that (a) pavement dwellers (those without any home) are not sampled, and (b) the joint family tends to harbor those with very low incomes or no jobs, so that they do not constitute separate households [8, pp. 52-54].

[3] Rough checks on the NCAER income data show them to be of the approximately correct order of magnitude. For example, one may blow up the data by multiplying average income in each bracket (as estimated by the survey) by the distribution of all urban households between brackets (as estimated by the survey). This comparison suggests that at least 90 per cent of urban income, and probably a higher proportion, was covered by the survey [8, pp. 102-4]. This coverage is of the same order of magnitude as that obtained in the Surveys of Consumer Finances.

TABLE 2—SAVINGS PATTERNS IN RELATION TO INCOME FOR URBAN INDIA, 1959-60

Disposable Income	Urban India (NCAER)		
	Weighted Per Cent of Households	Saving Income Ratio	Share in Aggregate Net Saving
Rs.	(1)	(2)	(3)
Under 1000	42.5%	−20.6%	−87.9%
1000–1999	32.5	− 6.0	−44.7
2000–2999	10.7	− 1.0	− 4.3
3000–5999	10.4	4.5	37.6
6000–9999	2.4	11.4	32.9
10,000–24,999	1.3	27.8	93.5
25,000 and over	0.2	44.5	72.9
All	100%	3.3%	100%

Source: [8, p. 78, Table 43].

uted than income, are not fully covered by the NCAER data, not enough is subtracted, and the residual NCAER inequality of expenditures would be slightly overstated, even if the income estimates were entirely correct. Thus it can be said that there is no significant disagreement between the NSS data which are the starting point of the O-B estimates and the NCAER data. The observed discrepancy in income distribution arises from the O-B method of superimposing savings estimates on the expenditure distribution.

When it comes to adding savings to the expenditure distribution, O-B make an assumption that is contrary to statistical evidence. Taking the best available estimate of *aggregate* household saving, they set themselves the task of distributing this aggregate between the various expenditure groups. For this purpose the following assumption is made: "The consumption expenditure in the low-income group is assumed to represent its disposable income as whatever evidence is there indicates that persons with incomes up to Rs. 3000 per year have on an average hardly any net saving" [5, p. 1353]. It *is* correct that households with incomes below Rs. 3000 on balance contribute nothing to aggregate saving; but they do dissave on balance, as the second column of Table 2 indicates. In fact, the NCAER data suggest that dissaving by households under Rs. 3000 (who may draw on assets, get help from relatives, or turn to the proverbial moneylender) may be equivalent to a substantial fraction of net aggregate saving (Col. 3, Table 2). While the lower-income groups have substantial negative saving, the upper-income groups save very much more than the amount represented by aggregate net saving. Thus the O-B estimates err considerably in both directions. They fail to subtract negative saving from expenditures at the lower end of the income distribution and consequently overstate income there. At the same time, they substantially underestimate positive savings in the upper-income brackets and hence understate income at the upper end.

Lest the reader suspect that the substantial negative saving at low-income

TABLE 3—SAVINGS PATTERN IN RELATION TO INCOME FOR UNITED STATES AND GREAT BRITAIN

Income Units	Share in Aggregate Net Savings of Each Tenth		
	Great Britain 1951–52	United States	
		1949	1950
Top tenth	225%	105%	73%
Second tenth	20	26	20
Third tenth	−13	13	11
Fourth tenth	−13	8	10
Fifth tenth	4	1	4
Sixth tenth	3	*	− 1
Seventh tenth	− 6	− 4	1
Eighth tenth	−30	− 8	*
Ninth tenth	−50	− 6	− 2
Bottom tenth	−40	−35	−16

Sources: [4, p. 144, Table 72] and [9, p. 1067, Table 8].
* Less than half of one per cent.

levels represents an error in the data, Table 3 presents savings shares by income tenths for the United States and Great Britain for the years utilized by O-B as a basis of comparison. These data show that the phenomenon of negative saving in the lower-income brackets is not unique to India. True, these data probably exaggerate negative saving. Yet a very careful attempt by Irwin Friend and Robert Jones [1] to correct the 1950 U.S. urban savings distribution for errors and omissions (particularly the omission of currency and the underreporting of demand deposits) led to a revised savings distribution which still showed negative savings/income ratios in all brackets below $3000.[4] If O-B had set themselves the task of estimating longer-run average incomes, the assumption of zero dissaving in the bottom brackets might have been justifiable. But it is not meaningful to compare a long-run distribution of income in one country with short-run distributions in other countries.

Both 1949 and 1950 data are shown for the United States in order to illustrate the fluctuations that occur in the extent of negative saving in the lower-

[4] The quality of the NCAER savings data is only partly relevant to our argument, since the NCAER income data were obtained by direct inquiry about income (*not* by adding savings to expenditures). Still, it should be pointed out here that the NCAER savings data underestimate saving, primarily because no data were collected on changes in currency holdings, on employers contributions to provident funds, and on cooperative shares; and because changes in bank deposits were underreported. Adjustment for these items probably would have raised the share of the middle-, and possibly of the lower-, income brackets in total saving. It is, in addition, conceivable that some Indian low-income families exaggerate debt incurrence and hence negative saving; but it is not plausible that omissions and errors in the data account for all negative saving. Even a small negative savings/income ratio in the under-Rs. 2000 income brackets means large aggregate dissaving because of the concentration of about three-fourths of the families in these brackets.

income brackets between good years (1950) and recession years (1949). The difference in the amount of negative saving between Great Britain and the United States suggests that the magnitude of negative saving in the lower brackets may be influenced by the level of real income. It would seem that in the course of Indian economic development changes in the extent of negative saving would occur. Hence the O-B method of estimating *changes* in the income distribution over time, which makes no allowance for such variations, must be judged unreliable, quite apart from the level of their concentration ratio. Periodic household surveys measuring income directly would seem to be a much more promising method of assessing changes in income inequality.

The NCAER data presented relate to the urban sector only. However, the crucial assumption that saving is zero for households with incomes below Rs. 3000 was also made by O-B for the rural sector, and it is equally indefensible there. Observers of the Indian agricultural sector would undoubtedly agree that many small cultivators are unable to make ends meet, not only in years of bad harvests, but also in the longer run. Hence they borrow money and gradually mortgage or sell their land, i.e. there is negative saving at the lower end of the rural income distribution. Thus it is clear that the O-B estimates understate rural income inequality as well.

Yet, what requires explanation is primarily the relatively high income inequality in the nonagricultural sector of developing countries. Rural income inequality stems largely from historical patterns of landownership. That in a country like India the urban industrial sector, which is newer and has much greater resemblance to a Western economy, should display a comparatively high degree of income inequality is much more puzzling.[5] It would be regrettable if faulty data led economists and the Indian planners to ignore this problem. The Indian income distribution *does* confirm the Kuznets finding of greater inequality of income in the developing than in the more industrialized economies. A clear recognition of this phenomenon is a first step towards research to explain the underlying causes and, ultimately, to policies to combat the tendencies toward inequality.

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[5] Some suggestions about the underlying causes have been offered by Kravis [2] and Kuznets [3].

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Pattern of Income Distribution in an Underdeveloped Economy: A Case Study of India: Comment

In a recent article in this journal, P. D. Ojha and V. V. Bhatt attempted to find out "whether the conclusion reached by Kuznets about greater inequality in underdeveloped countries is borne out in the case of India" [5, pp. 711–12]. The authors concluded that India did not substantiate Kuznets' generalization.

The purpose of this note is to argue that the authors' analysis of India can be seriously questioned. I have tried only to indicate here that India does fit the pattern established by Kuznets.[1]

I. *Kuznets' Hypothesis*

On the basis of extensive international cross-section and time-series evidence, Simon Kuznets [2] [3] concluded that over a long period of sustained economic growth in which an underdeveloped nation matures to a developed nation, the distribution of income first widens, then stabilizes, and finally narrows. Estimates indicated that at the present level of development of countries, the inequality was narrower in developed countries than in underdeveloped countries. This "hypothesis" was tested on time-series data of (now) industrially advanced countries and also on available cross-section international evidence. The studies of Kravis [1] and Oshima [6] lend support to this general hypothesis. It is important to stress that Kuznets' hypothesis was tested not only for the size distribution, but also for the sectoral and regional distributions. Therefore when Ohja and Bhatt refer to Kuznets' conclusions, they are confining themselves to the size distribution, and *in particular* to the degree of income inequality in India vis-à-vis the developed countries.

[1] This topic is covered in Chapter 5 of my doctoral dissertation, "Economic Growth and Income Distributions in a Developing Nation," Harvard University, January, 1965. Further, a single case can neither prove nor disprove the empirical generalization of Kuznets.

II. *The Data*

The central body of data the authors utilize is the *National Sample Survey* (NSS) data. These data refer to consumer expenditures and are collected in a sampling inquiry. Because of conceptual changes, the NSS data suffer from biases, requiring some preliminary adjustments. The authors do begin on a cautious note regarding the reliability of data, but this is not evident later when they make inferences. To suggest the biases in the data,[2] I have summarized in Table 1 estimates of national income derived from NSS data.

It is evident from line 3 in Table 1 that NSS estimates are generally

TABLE 1—A CONSISTENCY TEST OF NATIONAL SAMPLE SURVEY AGGREGATES
(Rupees thousand million)

Source	Fiscal Years			
	1953–54	1954–55	1955–56	1956–57
National income at factor cost (NSS)[a]	98.0	82.0	89.7	94.4
National income at factor cost (CSO)[b]	104.8	96.1	99.8	113.1
Per cent difference	−6.9	−17.2	−11.2	−19.8
Per cent difference corrected for biases[c]	−1.1	−5.9	−0.6	−8.2

[a] Estimates using Reserve Bank of India estimates of savings (1962).
[b] *Central Statistical Organization* (1964).
[c] See text.

lower than CSO national income totals, and further the divergence is significant and increases over time, particularly from 1953–54 to 1956–57, the time span on which Ojha and Bhatt base their study. There are several reasons that may completely explain this divergence, but in order not to digress from the main purpose of this paper I note here only three of the more significant factors.[3] The NSS aggregates are lower because (a) the survey excludes certain items like the imputed rent of owner-occupied dwellings, which are included in the CSO totals; (b) large-scale surveys traditionally tend to miss out on high-income households; (c) the divergence between the NSS and CSO aggregates seems to increase sharply over 1953–54 because the prices employed in the NSS data to evaluate home-grown produce were changed from retail to ex-farm prices.

[2] Since NSS data are presented in interpenetrating samples, the sampling error of combined sample estimates may be estimated by half the absolute difference between the subsample estimates, which possesses one degree of freedom. The reliability of NSS data with respect to sampling fluctuations can be shown by use of this estimator.

[3] A more exhaustive treatment is attempted in my doctoral thesis.

The effect of the first two biases in data is to underestimate the shares of the upper ordinal groups in the size distribution of consumer expenditure, and hence in the income distribution. Since the proportion of home-grown produce in consumption tends to be larger among lower-income groups, the effect of the third factor is to overestimate the inequality in Period II (of Ojha and Bhatt) relative to Period I. Hence one first has to make sure that any trend observed in the size distribution is not entirely due to these biases. Ojha and Bhatt show no explicit recognition of these drawbacks in data.

III. *Assumptions*

Since data on income distribution that refer to total income recipients do not exist for India, measurements of income inequality have to be based on *estimated* income distributions. Ojha and Bhatt derive their estimates using the NSS consumer expenditure distribution. This is done in their earlier paper on the subject [4, pp. 1350–52]. But it is not at all clear how they constructed their distribution, for implicit in their study must be some assumption on the distribution of savings (not available for India), which they have left quite obscure. If I may guess on the basis of paragraph 17 of [4], the authors appear to have assumed that saving is zero for households earning less than Rs. 3000, and positive otherwise. Such an assumption is highly unrealistic as it ignores dissaving of low-income households, which results in understating the income inequality.

What is perhaps more obscure is their derivation of the national distribution from the rural and urban distributions. As presented in their Table 1 [5, p. 714], the share of any of the upper ordinal groups in the national size distribution is greater than the share in the rural distribution, but less than the share in the urban distribution. To put it differently, the estimates of the shares of ordinal groups in the all-India distribution do not appear to be consistent with the estimates of the rural and urban shares. To illustrate, let us consider in their Table 1 the share of the top 5 per cent in the all-India distribution. Since 24 per cent of the households (which account for 50 per cent of the income) reside in the urban sector, if the top 5 per cent in the rural sector have more personal income than any other ordinal group except the top 5 per cent in the urban sector, then the share of the top 5 per cent in the national distribution must be $0.5\times17.0+0.5\times26.0=21.5$ per cent. This is a conservative estimate for it assumes that the top 5 per cent in the rural sector earn more than the 90–95 per cent group in the urban sector. But this conservative estimate of 21.5 per cent is higher than the Ojha-Bhatt estimate of 20 per cent, which therefore suggests serious questions concerning their derivation of the all-India distribution on which many of their inferences are based.

IV. *Intersectoral Inequality and the Size Distribution*

To restate the above point, the Ojha-Bhatt estimates of the national distribution imply very narrow intersectoral inequality (which may be measured as the ratio of urban to rural per household income). To elaborate this point, let us consider the following mathematical construction:

Let f_r and f_u be the income distributions in the rural and urban sectors, respectively. Then

$$f = w_r f_r + w_u f_u \tag{1}$$

is the national distribution of income where w_i is the fraction of households residing in the ith sector.

If we measure income inequality by the coefficient of variation C, then

$$C = \frac{\sqrt{w_r C_r^2 + w_u C_u^2 \lambda^2 + w_r w_u (\lambda - 1)^2}}{w_r + w_u \lambda} \tag{2}$$

where C_i = income inequality in the ith sector and λ = intersectoral inequality. Thus, the inequality in the national distribution depends not only on intrasectoral inequalities, but also on the level of urbanization w_u and intersectoral inequality λ (with $0 \leq \lambda \leq \infty$).

To show that the Ojha-Bhatt estimates of the national distribution imply very narrow intersectoral inequality, we insert in equation (2) the values of C, C_r, C_u, and w_u ($\equiv 1 - w_r$) and solve this equation for λ. But instead of estimates of C, C_r, and C_u, Ojha and Bhatt present only estimates of the Lorenz index. We convert their estimates of the Lorenz index to derive the implied estimates of the coefficient of variation, by a purely expository device.[4] Substituting these values in equation (2), we get:

$$0.688\lambda^2 - 1.181\lambda + 0.448 = 0. \tag{3}$$

The solutions to this quadratic equation are $\lambda = 1.14$ or 0.57. Rejecting the latter solution, the former solution implies a very narrow intersectoral inequality, for actually from NSS data on consumer expenditure,[5] λ is close to 1.5. The Ojha-Bhatt estimate is, paradoxically, even higher than ours. Their estimate of intersectoral inequality is 1.52, since they estimate per household income at Rs. 1,150 and Rs. 1,751 in rural and urban sectors, respectively. Thus the Ojha-Bhatt estimates of the national distribution suffer from a serious inconsistency, which must be explained first before any inferences can be based on them. Obviously, with an implied value of 1.14, they have underestimated the inequality in the national distribution.

V. *Conclusions*

In the process of analyzing the data and the mathematical relationship between the intersectoral inequality and the size distribution, I have argued that Ojha and Bhatt have ignored the biases in data, and by virtue of their estimation procedure have underestimated the inequality. Since their con-

[4] By assuming the distribution of income to be log-normal, which (distribution) fits Indian data well. Under this assumption, if C is the coefficient of variation and L is the Lorenz index, then

$$C^2 = \exp.\left\{\sqrt{2}\Phi^{-1}\left(\frac{L+1}{2}\right)\right\}^2 - 1.$$

[5] Supplemented with Reserve Bank estimates of household saving.

clusion about inequality in India is entirely based on their estimates of the national size distribution, it is not an acceptable conclusion. A numerical proof is given in Table 2.

TABLE 2—SIZE DISTRIBUTION OF INCOME AND CONSUMER EXPENDITURE IN INDIA (Percentages)

Ordinal Group	Ojha and Bhatt[a] (1953–57)			Ours[b] (1956–57)			
	Income			Consumer Expenditure			Income[b]
	Rural	Urban	All-India	Rural	Urban	All-India	All-India
Top tenth	25	36	24	26	33	30	38
Second tenth	14	11	14	15	15	17	15
Third tenth	12	11	12	12	11	13	11
Fourth tenth	10	8	9	10	9	10	9
Fifth tenth	9	8	9	9	8	8	8
Sixth tenth	9	7	8	8	7	7	6
Seventh tenth	7	7	6	6	6	6	5
Eighth tenth	5	4	5	6	5	5	4
Ninth tenth	5	4	5	5	4	2	2
Bottom tenth	4	4	3	3	3	2	2
Lorenz ratio	0.306	0.382	0.355	0.314	0.398	0.407	0.460

[a] Ojha and Bhatt [5], disposable income ignoring household size.
[b] Income distribution based on the assumption that the top 10 per cent account for all the saving. The household size has been explicitly accounted for in the distribution. The shares of ordinal groups were estimated from cumulated proportion of households. For an algebraic expression of this method, see Swamy [7].

The inequality in the national distribution is not only a function of the inequality within sectors, *but also* of the level of urbanization w_u and intersectoral inequality. It is evident from (2) that if the intersectoral inequality is wide enough, the dispersion of income in the national distribution will be greater than the dispersion in the rural or urban sectors, i.e., $C > C_r$ and $C > C_u$.[6] Inserting in equation (2), for example, the values relevant (for the expenditure distribution) to India, i.e., $C_r = 0.69$, $C_u = 0.75$, $\lambda = 1.49$, $w_u = 0.24$, we get the coefficient of variation for the whole country as $C = 0.79$, evidently greater than 0.69 or 0.75.

If we assumed the absence of intersectoral inequality, i.e., $\lambda = 1$, the ex-

[6] It also follows that since

$$\frac{\partial C}{\partial \lambda} > 0,$$

a widening of intersectoral inequality implies, *ceteris paribus*, a widening of the national size distribution of income. With economic growth, because of the rapid growth of the urban sector, the intersectoral inequality widens in the earlier phases which contributes to the widening of the national distribution.

pression for C simplifies to $\sqrt{w_r C_r^2 + w_u C_u^2}$, from which it follows that $C_r < C < C_u$. For India, then, $C = 0.71$.

TABLE 3—COMPARISON OF CONCENTRATION RATIOS AMONG SELECTED COUNTRIES

Country	Disposable Income Concentration
India 1956–57	0.46
India (Ojha and Bhatt)	0.33
Ceylon 1952–53[a]	0.41
United Kingdom 1951–52[a]	0.29
The Netherlands 1950	0.41[b]
United States 1950[a]	0.32

Sources:
[a] Kuznets [3, Table 8, p. 28].
[b] Ojha and Bhatt [5, Table 4, p. 716] who perhaps have taken it from the U.N. source they quote. Kuznets' estimate for The Netherlands (1954) is 0.29.

Hence, it is apparent that by assuming away intersectoral inequality, the dispersion in the national distribution of income is understated, and the wider the intersectoral inequality, the greater is the understatement.

It is indeed tempting to find out where India stands in the Kuznets-spectrum of countries. This is difficult because the available estimates in India refer only to consumer expenditure, not to income, and even these, as we saw in Table 1, have to be used with care. However, I have presented in Table 2 an estimate of income distribution, emphasizing that it understates inequality, which I cannot correct since there are no data on dissavings and tax evasion.

On the basis of Tables 2 and 3, the indication is that India does substantiate Kuznets' hypothesis.

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* The author is instructor in economics at Harvard University. He is indebted to Simon Kuznets for many stimulating comments and criticisms on an earlier draft of this paper. Remaining errors are the author's.

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Pattern of Income Distribution in an Underdeveloped Economy: A Case Study of India: Reply

Since the methodological aspects of our estimates were presented in detail in our article in the *Reserve Bank of India Bulletin* [4], we did not reproduce them in our article in this *Review*. Eva Mueller and I. R. K. Sarma suggest that a "U.S. reader" is unlikely to study relevant references. Contrary to our expectation, this seems to have turned out to be true in the case of the comments on our article by Subramanian Swamy.

Swamy finds that our assumption regarding distribution of saving and our derivation of the national distribution from the rural and urban distribution are "obscure." If he reads our *Reserve Bank of India Bulletin* article [4] again, he will find that the assumption about distribution of saving is very clearly stated [4, paragraph 26], and the derivation of the national distribution by aggregating rural and urban distributions is clearly shown [4, Table III]. Some inconsistencies between the NSS and CSO data are also pointed out [4, paragraph 10]. It is not clear how Swamy has estimated national income at factor cost on the basis of the NSS data; contrary to his results, we had found that the estimated all-India consumption expenditure on the basis of the NSS data exceeded the private income as estimated by the CSO [4, paragraph 10]. *Further, our distribution estimates relate to the distribution of personal income as estimated by the CSO.*

Swamy finds that our estimates of the shares of ordinal groups in the "all-India distribution do not appear to be consistent with the estimates of the rural and urban shares." This statement again shows that he has not carefully studied our earlier article [4]. Since the all-India distribution is derived by aggregating the rural and urban distributions, the question of inconsistency *simply* does not arise. Swamy finds this inconsistency on the basis of his facts and assumptions, and not on the basis of our estimates. For example, his estimate of the share of the top 5 per cent in the national distribution is based on the assumption that 24 per cent of the households, accounting for 50 per cent of income, reside in the urban sector. Our estimates show that 20.8 per cent of the households, accounting for 28.6 per cent of income, reside in the urban sector [4, Table III]. If Swamy makes a recalculation on the basis of our estimates, he will find that his charge is ill-founded. Again, his estimate of intersectoral inequality is based on his facts and assumptions. The urban households are assumed to represent 24 per cent of total households, while our estimate for the share of urban households in the total is 20.8 per cent. Further, as pointed out earlier, since we have derived the national distribution by aggregating the rural and urban distributions, an estimate of intersectoral inequality is based on facts, while his estimate is based on his assumption

that the distribution of income is log-normal. Swamy's mathematical constructions and estimates, thus, are irrelevant.

Turning to the comments made by Mueller and Sarma, they make an astounding statement: income distribution estimates "for practically all the other countries referred to also are derived from households surveys and censuses which inquired *directly* [italics ours] about personal incomes." We can do no better than quote Kuznets, "Unlike the information on industrial structure, the evidence on the distribution of income by size is more in the nature of synthetic estimates, in which the ingenuity of the investigator overcomes gaps in the deficiencies of the available data" [2, p. 12]. The income distribution estimates for the United Kingdom and the other West European countries are all derived almost exclusively from tax returns [5, Chs. 2 and 6].

We did not use the NCAER survey results [3] precisely because we were rather skeptical about their reliability. The NCAER survey, further, relates to income distribution in the urban sector, and that too for 1959-60 only, while we were interested in the distribution of income among various income and functional groups on an all-India basis from 1953-54 onwards. The survey covers only 85 per cent of the urban income on the basis of the NCAER study [3, p. 36]; however, on the basis of our estimates of urban income, the survey ignores about 30 per cent of urban income.[1]

Apart from these reasons, the NCAER income distribution estimates are irrelevant for our purposes for two additional reasons:

1. The NCAER definition of the urban sector is inconsistent with the Census definition; the NCAER survey covers population residing in towns with a population of 10,000 or more, while the Census definition covers towns with a population of 5,000 and more [3, pp. 7 and 10].

2. Even with regard to the urban sector, as defined by the NCAER, the survey ignores about 2-3 million nonhousehold population [3, p. 101], comprising population living in hotels, hostels, boarding houses, hospitals, prisons, religious and charitable institutions, beggars, pavement dwellers, boarders, renters, domestic servants, etc. [3, p. 12].

Our estimates refer to the distribution of income *as estimated by CSO*

[1] It is difficult to understand how Mueller and Sarma can claim that the NCAER survey covered more than 90 per cent of urban income. The NCAER Study [3] gives two figures for urban income: survey estimate for income and estimate for income after adjustments [3, p. 36]. It is the survey estimate which is used for the purpose of income distribution, and this estimate is lower by 15-20 per cent than the other adjusted estimate. Our independent estimate for urban income is higher by more than 30 per cent than the survey estimate.

The principal reason given for the accuracy of the adjusted estimate is that the tax liability estimated on that basis is close to the tax payable by the urban sector, as obtained from the income tax data [3, pp. 102-3]. In fact, this proves that the adjusted estimate is incorrect; since there is considerable tax evasion, the income tax liability estimated on the basis of a correct estimate of urban income should be much larger than the actual income tax paid or payable. This shows that even the adjusted estimate understates urban income. For the extent of tax evasion, see P. D. Ojha and V. V. Bhatt, "Some Aspects of Income Distribution in India," *Bulletin of the Oxford University Institute of Economics and Statistics*, Vol. 26, No. 3, 1964.

among households *estimated on the basis of the Population Censuses. Our* coverage, both of incomes and households, is complete.

It is true that our assumptions about the distribution of saving affects our estimates of income distribution. But then we have no firm and reliable data with regard to the distribution of saving. The NCAER survey data relate to only 20 per cent of net saving [3, p. 78], ". . . a larger portion of saving omitted from the distributional analysis is attributable to the middle rather than the top and the bottom income households" [3, p. 79]. It is quite likely therefore that the saving of the top 5 per cent of the households would not be affected even if we had got accurate data relating to the distribution. If we assume that the bottom 40 per cent of households dissave and that this dissaving is offset by the saving of the next 50-55 per cent of households, the result may well be that the concentration ratio would be *lower* than what is estimated by us. Mueller and Sarma's statement that our assumption relating to the distribution of saving has led to a significant understatement of income inequality remains to be proved.

The really fruitful comment on our study is that made by Alan Heston [1]. He does not object to our assumption regarding distribution of saving for deriving the *state of income distribution* during a period of time. He, however, raises a valid objection against this assumption when an attempt is made to estimate *changes in income distribution* from Period I to Period II.

Even on a priori grounds, one could expect, during a certain phase of its development, a lower degree of inequality in income distribution in an underdeveloped country than that in a developed country. When an underdeveloped country starts its industrialization program by a fairly radical abolition of the big feudal and landed interests, the tendency toward greater inequality as a result of urbanization and industrialization would be more than offset for some time by the reduction in inequality as a result of agrarian reform. In India the big landlord class and the princely class were already abolished by 1953, while the top urban-industrial class in terms of income was relatively not as large as the other two before 1953. One could therefore expect a lower degree of inequality in income distribution in India than in the developed countries. Of course, as our estimates show, there is a tendency toward increasing inequality in the urban sector, and if this continues, it may turn out that by 1970 the concentration ratio in India would be higher than that in the developed countries. In any case, even according to our estimates, the concentration ratio in the Indian urban sector is higher than that in the developed countries.

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BOOK REVIEWS

General Economics; Methodology

The Intelligence of Democracy—Decision Making Through Mutual Adjustment. By CHARLES E. LINDBLOM. New York: The Free Press, 1965. Pp. viii, 352. $7.50.

This book deals with a very important aspect of governmental decision-making, and has been written by a distinguished economist who had already made notable contributions to our understanding of social decision-making processes. His present book contains many interesting observations. All the same, in the reviewer's opinion, it is a disappointing book because its main conclusions are unnecessarily vague and unspecific, largely owing to the fact that the author makes rather little use of precisely defined theoretical concepts or of clearly specified analytical models. For instance, even though there is a brief discussion of the technical concept of Pareto optimality, most of the analysis is conducted in terms of such ill-defined common sense notions as "coordination," "rationality" (used in a nontechnical sense), "consistency," etc. (In particular, "coordination" as used in the book sometimes corresponds to the concept of an equilibrium point in a game-theoretical sense, implying that the different decision-makers' strategies are mutually best replies to one another; while at other times it corresponds to Pareto optimality. Since equilibrium points in general are definitely not Pareto optimal, this distinction should not be blurred over by denoting these two very different concepts by the same term.)

The author distinguishes between two main methods of coordinating the policies of different decision-making agencies (governmental and nongovernmental). One is coordination by a central decision-maker, presumably guided by some concept of public interest or social welfare. The other is coordination through spontaneous mutual adjustment among different government agencies as well as different social groups and organizations, each of them representing different—largely sectional—social interests. This latter type of coordination Lindblom calls "partisan mutual adjustment." (Nonpartisan mutual adjustment among different agencies, if all of them were guided by the same concept of public interest, would be classified by him as a form of central coordination.)

Many critics of the U.S. governmental system have argued that the great multiplicity of largely independent government agencies with ill-defined and often overlapping jurisdictions leads to major inefficiencies in governmental decision-making. Even defenders of the system have often admitted that this proliferation of independent agencies will in fact result in certain inefficiencies, but have argued that these are a price worth paying for avoid-

ing undue concentration of political power in the hands of the central government.

In contrast to this widely accepted view, Lindblom argues that in a complex society effective central coordination of policy decisions is simply impossible because the central decision-maker could never obtain the enormous amount of information he would need for this purpose, and because he could not make an effective use of it even if he could somehow get access to all this information. Hence we cannot possibly avoid dividing decision-making power among a number of government agencies (as well as a number of private organizations). On the other hand, partisan mutual adjustment among all these diverse decision-making units will tend to achieve a high degree of policy coordination even in the absence of any central direction.

In the reviewer's opinion, Lindblom has convincingly shown that under *some* conditions this mutual adjustment may achieve satisfactory coordination (in the sense of near-Pareto-optimality). But of course the really interesting question is under *what* conditions this is likely to happen and under what conditions centralized coordination is both feasible and necessary. This question is hardly discussed in the book at all—and in effect it cannot be meaningfully discussed at all without using more precisely defined analytical models than those used in the book.

For example, Lindblom puts considerable emphasis on the costs and difficulties of central coordination, but hardly ever mentions the costs and difficulties of coordination among different government agencies. Nor does he make it clear that there are great differences between different decision problems as to the extent to which they can be decomposed into reasonably independent subproblems which then can be entrusted to the care of different agencies without much need for detailed coordination. In many cases no such decomposition is possible. For instance, many of us feel that disastrous results tend to follow when, under a weak president, different government agencies—such as the State Department, the Central Intelligence Agency, the various armed services, etc.—all pursue different and mutually contradictory foreign policies without effective central coordination by the White House. Again, many of us are not very happy with the results of entrusting city planning to a number of independent municipal governments without any central regional coordination.

Finally, Lindblom's arguments about the impossibility of central coordination are far from convincing. Admittedly, a central decision-maker cannot possibly coordinate the policies of different government agencies in any great detail—but this does not mean that he cannot, or should not, coordinate them in their general outlines. The commander-in-chief of a large army clearly cannot personally make the tactical decisions for all army units under his command. But this does not mean that he cannot, or should not, personally approve of all major strategy decisions. Once more it depends on the particular policy problem to what extent a central decision-maker can effectively control the basic strategy choices without necessarily being overwhelmed by the unmanageable tactical detail. Moreover, the limits of possible effective central

coordination are bound to shift over time. As technology and social institutions grow in complexity, central coordination of government decisions becomes more and more difficult. But this fact is often more than offset by advances in the techniques and resources available for central coordination. For instance, the advent of operations research, cost-benefit (and cost-effectiveness) analysis, high-speed computers, etc., have made central coordination both practicable and profitable in areas where this was not the case in the past.

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Analysis for Military Decisions. Edited by E. S. QUADE. Chicago: Rand McNally & Co., 1964. Pp. viii, 382. $10.00.

This volume makes available in revised and updated form a set of lectures on military systems analysis first given by RAND Corporation staff members in 1955 and 1959. It is geared to an audience of military decision-makers, with emphasis on principles rather than techniques, but there is much to interest economists concerned both with theory and applications.

The volume's principal distinguishing feature is the impressive collective wisdom it compresses between two covers, rather than its originality, since most of the ideas offered have appeared in earlier articles and books by the 13 lecturers and their colleagues. The problems tackled—choosing efficient weapon systems and related tactics—are about as difficult as any that absorb the energies of physical and social scientists. Modern weapon systems are extremely complex; technological uncertainties abound; the reactions of rivals are hard to predict; and the intangible, multidimensional objectives of weapons programs are not readily collapsed into the nice ordinal or cardinal functions we like to maximize. RAND's analytic approach in the face of these obstacles, illustrated especially well through three fascinating case studies, will surely serve as a model for evaluating major new government programs in the space, foreign aid, and domestic welfare fields as well as in the military sector.

Several themes recur in the case studies and in lectures on over-all analytic philosophy, model-building, the selection of criteria, cost analysis, enemy reactions, gaming methods, research and development strategy, technological scaling laws, and the use of computers. One is the frequent superiority of rough, common-sense quantitative analysis over high-powered optimization techniques. As the editor observes in his introduction, honors in early military operations research work went to people who improved or found new uses for sophisticated mathematical methods, while in current weapon systems analysis honors go to those who are able simply to find out what the problem is. A second theme is the need for iteration and feedback in redefining objectives and devising new, dominant solutions. Also emphasized is the role of "sensitivity analysis"—estimating how conclusions may be affected if uncertain parameters take on adverse values.

Since the authors stress the selection of appropriate decision criteria, it is

worth mentioning that they don't always agree on what is proper under comparable conditions. In Chapter 5 Roland McKean criticizes maximization of the effectiveness/cost ratio, plugging instead for maximization of gains less cost when the two are commensurable. Three chapters later E. S. Quade appears to opt diagrammatically (if we allow for a slight drafting imprecision) for maximizing the effectiveness/cost ratio. And in Chapter 10, Thomas Schelling suggests that a nation should weigh its own costs against the costs its action will induce a rival nation to incur. The logic of this last (quite popular) criterion escapes me. Choosing actions because they cost rivals more than they cost us seems rational only if the amount of resources rivals can allocate to defense is fixed—an assumption unsupported by recent history—or if a "game of economic ruin" arms race is being waged. And if it is true that affluent Communists are less likely to be aggressive Communists, the advisability of the latter strategy is highly doubtful.

My principal critical reaction relates more to the compendium's philosophical underpinnings than to its specific content. Several contributors acknowledge that formal game theory is seldom directly useful in analyzing the tough questions of weapon systems choice. However, with the pronounced exception of Schelling, a visitor from far distant shores, the RAND line on rival reactions retains the heart of zero-sum game theory: the minimax assumption. To be sure, many of the explicit illustrations are of the tactical variety, where zero-sum conflict is most closely approximated. Albert Wohlstetter also pays lip service to the notion that in strategic games we and our rivals have some common interests. On the whole, however, there is too little concern for the fact that strategic deterrence, decisions to develop new weapons technologies, and arms races are typically Prisoner's Dilemma games, in which minimax is a notoriously bad strategy. The deeply pessimistic *Weltanschauung* of this work is characterized most vividly by Wohlstetter's assertion that the Soviets' "fondest desires" include launching a successful surprise nuclear attack against the United States. No one can prove that such pessimism is wholly unwarranted, but the expected value of the world's survival game almost certainly suffers if we continue pursuing a minimax approach to the problems of deterrence, arms control, and disarmament. All might benefit if military systems analysts spent less time worrying how we can make the best of the worst our rivals may do, and more time inventing ways of cooperating to avoid new self-defeating steps in the perfection and spread of mass-destruction weapons.

One further point in the same vein: I find unnecessarily antagonistic the book's repeated references to the Soviet Union as "the enemy." Can't the Air Force's famed Office of Nomenclature, Special Division for Acronyms, concoct a term more neutral emotionally?

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Economic Processes—The Composition of Economic Activity. By Melvin A. Eggers with A. Dale Tussing. New York: Holt, Rinehart and Winston, Inc. 1965. Pp. xi, 435. $6.00.

Economic Processes—The Level of Economic Activity. By MELVIN A. EGGERS WITH A. DALE TUSSING. New York: Holt, Rinehart and Winston, Inc., 1965. Pp. xi, 403. $6.00.

These two volumes are a new text for use in the introductory course. Its publication is justified by (a) the presentation of elementary economics without any graphs (yes, *graphs!*), (b) the simultaneous development of the real and financial aspects of macroanalysis, and (c) the separation of the introductory text into two separate volumes, one for the macro theory (*The Level of . . .*) and the other for everything else (*The Composition of . . .*). These three features are closely—if not intentionally—related, since the micro section is very poor, due in part (but only in part) to the absence of graphs, while the macro section gives a clear and interesting integration of real and financial analysis on Gurley-Shaw-Tobin lines, so that the instructor's freedom to use one section without having to use the other turns out to be more than a publisher's gimmick.

In the macro volume, the theory of income determination is presented both in real and financial terms with an attempt to integrate individual and aggregate behavior. The emphasis is definitely on finance. The point of departure is the idea that only people produce goods and services and people are compensated either with the product itself (barter) or with equivalent claims against that product (financial instruments). Income, then, has to equal output. A firm—a "production group"—is an entity that can't own anything itself, since it isn't a person. The firm can only hold assets (real or financial) when it concurrently issues a liability (a claim)—which can be net worth—to a person. People and products are central; firms can act only by proxy.

This departure from the usual firm with its living, breathing, and ubiquitous entrepreneur yields a coherent description of financial flows (claims) as the counterpart of real product flows and, as a minor bonus, a nice rationalization of double-entry accounting.

A firm, as output is produced, will issue claims to people who produced the output. Product and claim both disappear from the accounts when people trade the claim for the product. On the real side, saving (people's failure to take all the product they've made) is clearly the same thing as investment (the firm's retention of that much of the product). And on the financial side, saving (people's acceptance of a claim instead of product) is clearly the same thing as borrowing (firms' issues of new claims to people so the firms can keep part of the product).

Rather than using the usual equality of intended savings and intended investment to describe the equilibrium level of aggregate income, this approach stresses Tobin's equality of intended savings and intended deficits. "An excess of desired savings over actual creation of financial instruments . . . has a general contractional [*sic*] effect on the economy." Unless some sector is willing to borrow (issue net new financial claims) in sufficient quantity to absorb all that people want to save at a full-employment level of output, there will be a recession. Demand-induced inflation is the result of trying to borrow more than people are willing to save at full employment.

This development of the financial side of real transactions is refreshing, and its use in an introductory text gives a welcome alternative to the orthodox exposition of Keynsian macroanalysis. The real side is not neglected—the separate components of aggregate demand are analyzed for stability, the multiplier is well explained, built-in stabilizers and discretionary fiscal policy are discussed—but real variables are definitely subsidiary to the flows of financial claims. Anyone interested in a brief summary statement of the model would be advised to turn directly to Chapter 14, "Macroeconomic Relationships in Perspective."

A major strength of this approach, of course, lies simply in its integration of finance. Stocks, bonds, and other nonmonetary claims need no longer be relegated to institutional or historical sections of the course, but can be (in fact, must be) brought into the analytical stuff of the subject. And the stabilization and allocation functions of the government work neatly into the analysis—government is not just an intrusive manipulator of aggregate demand for full employment, but it is the source of collective consumption which may be paid for by taxes (reducing people's private claims to the product) or by deficits (offsetting full-employment saving). Too, this primarily financial approach makes important identities—savings and investment, deficits and surpluses—seem self-evident. It becomes quite obvious, for instance, that borrowing makes saving possible just as much as the other way around. But it is in the treatment of money and finance that the real payoff of this approach comes, since it is possible to reach a level of sophistication in dealing with money supply and demand and integrating these with finance that far surpasses that of any elementary text I know of.

But there are costs inherent in introducing macroeconomics through finance. The most obvious of these might appear to be simple inefficiency—fully one half of this (combined) text is taken up with the exposition of macro theory; it takes five chapters (about a quarter of this volume) before national income accounts can be even discussed. Another quarter of the book —those five chapters that follow immediately after national income accounts —deals with finance, financial intermediaries, commercial banks as financial intermediaries, and the determination of the money supply through monetary policy. It is a detailed discussion that removes much of the mystery that too often surrounds the creation of money in elementary texts, but it takes up a lot of space and time. A student does not learn of the multiplier until more than half of the macro section is behind him.

Perhaps it is unfair to characterize this unusual amount of space devoted to finance as an "inefficiency," since it does make the book a different product—the student who comes through a course structured around this book will have a far more comprehensive understanding of finance and financial institutions than he would from any more orthodox course. But there is a real cost if the detail of and emphasis on finance cause the student to lose track of how the real variables change. Or, a more pertinent danger, students of this book may become so immersed in its unique approach that they fail to realize that the "deficient aggregate demand" they'll read about in other courses is the same thing as the "excess of desired savings over actual creation of financial

instruments" they encounter here. Finally, the approach-through-finance requires a great deal of accounting, and there may be students who find graphs a far more congenial expositional device than T-accounts.

In sum, the macro volume presents a coherent and quite successful development of the theory through simultaneous discussion of real and financial variables. There are little irritants—the use of non-words like "expansional" and "contractional"; a tautological discussion of a factor's "contribution" to the product (which makes a $100 mugging "contribute" $100 worth of product); the restriction of the subject of economics to *quid pro quo* exchanges; the inconsistent use of an awkward private vocabulary. These are relatively minor in the macro volume, but on the order of an epidemic in the micro volume.

I hardly know what to say about the micro volume, *The Composition. . . .* Even detailed discussion could only appear intemperate because this is a bad book. It is full of inaccuracies, inconsistencies, and curious vocabulary besides being confused and erroneous in its exposition. Its widespread adoption could set the country's level of economic literacy back ten years. While there might be satisfaction of sorts in supporting this judgment with detailed references, both limited space and charity suggest that nothing more be said about the micro portion of this text.

My reservations about the macro volume center on the usual question of any text that uses a new approach to an old subject—does it "teach well?" In this, the authors have had more experience than I, but I would be most confident in adopting this volume for the macro part of an introductory course in a graduate school of business where the emphasis on finance would serve an immediate end. It might work well, too, as the core of an intermediate course in which the macro theory was to be merged with the usual financial and banking course. But in recommending it as a text for the macro part of the introductory course—for which it is intended—I would be more timid. As indicated above, I would have some fear that the financial vocabulary and detail might obscure the understanding of real variables. And since all of the applications of the theory—trade, development, public goods—are in the micro rather than the macro volume, the instructor would have to devise a good deal of outside reading or let his introductory macro course become an abstract text-and-theory course—an alternative many instructors would resist. Finally, the absence of supplementary material—a workbook or instructors' manual—would create problems in some introductory courses.

Considering the two volumes together, the macro volume is a strong and interesting book that gives the instructor a real alternative in developing the theory of income determination. But, despite its strength, it cannot offset the weaknesses of the micro volume enough to recommend adoption of both of them as a single text.

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Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

Investment Demand and U.S. Economic Growth. By Bert G. Hickman. Washington: The Brookings Institution, 1965. Pp. viii, 264. $6.00.

In the future, when I want to show a graduate student how to report quantitative research, I shall have him read Bert Hickman's recent investment study. Written with a minimum of professional doubletalk, it outlines clearly a model of investment behavior, presents and discusses empirical tests of the model, and then probes the implications of the findings for the growth of investment demand in the United States.

The model is basically a capital stock adjustment mechanism, or flexible accelerator. This is modified somewhat by a complicated expression for the user-cost of capital and by a trend term which permits the capital coefficient to change over time if this seems consistent with the data.

The investment data cover those subdivisions of investment which are covered by the Office of Business Economics in its regular quarterly survey. Hickman has constructed corresponding capital stock estimates and uses their yearly growth rates over the postwar period as his dependent variables. As independent variables he uses current and lagged output-capital ratios, the time trends, and the user-cost estimates.

In general, the results show that the flexible accelerator gives a good explanation of capital stock growth, provided the trend terms are included. The trends pick up negative coefficients, indicating a decline in desired capital-output ratios. The user-cost variables are largely insignificant.

Hickman explains the apparent decline in desired capital-output ratios by reference to an ingenious transformation of Solow's "embodiment" model. This amounts to the replacement of Solow's vintage-weighting by a depreciation scheme and a trend term for rising productivity of the nominal capital stock.

In pursuing the implications of his findings, Hickman attaches considerable importance to this rising capital productivity. It implies that at constant full-employment growth the investment share in real GNP will decline secularly, and that a return of the U.S. economy to normal levels of unemployment will leave us with an investment share considerably lower than we had in the late 'forties and early 'fifties. Without a substantial shift in the price of capital goods relative to the price of the rest of GNP, this could be a source of the kind of secular stagnation Keynes envisioned unless we are willing to have continuous federal deficits.

This conclusion is, I think, largely the outgrowth of an incompletely specified model. Implicitly, Hickman's model incorporates a perfect capital market with no reluctance on the part of business for raising outside funds. That is, it fails to incorporate the main ideas of Duesenberry's *Business Cycles and Economic Growth*. This is unfortunate. In some of my own work on data similar to Hickman's, I have found the trend terms are insignificant

after variables measuring the cost of funds are added to the flexible accelerator.

This is, of course, a matter which must be argued out. What cannot be disputed, however, is that Hickman has written a splendidly stimulating and informative book. He is a very model of a modern model builder.

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Manufacturing Production Functions in the United States, 1957: An Interindustry and Interstate Comparison of Productivity. By GEORGE H. HILDEBRAND AND TA-CHUNG LIU. Ithaca: New York State School of Industrial and Labor Relations, Cornell University, 1965. Pp. xi, 224. $6.00.

The principal purpose of this book is to provide estimates of production function coefficients for 15 two-digit manufacturing industries in 1957. The estimates are obtained using cross-sectional data from the Survey of Manufactures. It is a tribute to the expositional skill of George Hildebrand and Ta-Chung Liu that the theoretical and empirical difficulties of this highly technical subject have not prevented them from writing a remarkably readable book. They are painstakingly careful at model specification, judicious in their use of econometric procedures, and aware of the limitations of their results. It is a good book and a useful addition to an already voluminous production function literature.

The book contains four chapters, the first of which is an excellent summary of the main empirical findings. The most important findings are: (1) technological change was of quantitative importance in explaining increases in output for most of the 15 industries; (2) most industries exhibited increasing returns to measured inputs, and the scale economies are quantitatively important; (3) the factor-income-shares approach to measuring production function coefficients seriously underestimates these coefficients; and (4) in 1957 all 15 industries were off their profit-maximizing labor demand schedules in the sense that they had too much production labor. The second chapter contains a brief review of the literature with particular emphasis on the CES production function, and the third develops the basic econometric model. The final chapter presents the parameter estimates for as many as eight alternative specifications of the basic model for each industry, employing both two-stage and "naïve" least-square procedures. This chapter also contains an analysis of the 15 industries as a whole, a section on labor and capital productivities by states, and an excellent summary cautioning the reader on several of the critical assumptions. The book contains three appendices, one of which contains most of the data they have used.

The simultaneous equation model is formulated with a Cobb-Douglas-type production function and separate production (L_p) and nonproduction (L_n) worker demand equations. Labor demand is assumed to adjust with a lag, and a lagged labor input variable (L_{-1}), measured in man-hours, is introduced. Capital inputs (K_{-1}) measured by book value of assets, are lagged

one period and treated as exogenous. The production worker input variable is corrected for labor "quality" (q) on the basis of years of school completed by persons over 25 years of age in each state in 1960. The authors assume that "technical change" is capital "embodied," and it is introduced in the capital exponent by a variable measuring net to gross assests (log R), a measure of capital "vintage." These features are summarized by the production function:

$$(1) \quad \log V = \log A + b \log q \log L_p + c \log L_n + e \log R_{-1} \log K_{-1},$$

where V = value added, A = constant; and two labor demand equations of the form

$$(2) \qquad \log L = \alpha_0 + \alpha_1 \log L_{-1} + \alpha_2 \log V + \alpha_3 \log W,$$

where W = the wage rate. Each variable is defined for a "representative" establishment and cross-section data by state are used to estimate the relationships for each 15 two-digit industries.

Both "naïve" and two-stage least-squares procedures are used in estimating several alternative versions of the basic model which differ in these respects: (1) capital inputs are defined alternatively as gross book value of plant and equipment and as a similar variable corrected for rented capital; (2) the labor "quality" correction is sometimes omitted; (3) production and nonproduction labor inputs are sometimes combined; and (4) (log R) is sometimes omitted. Estimates for each version of the basic model are presented and each industry is discussed separately. Choices among estimates are made and discussed on the basis of "plausibility." Because of the imprecise and often arbitrary nature of this procedure, the presentation of both successful and unsuccessful estimates is especially helpful to the reader and adds to the usefulness of the work.

The variable ($\log R \times \log K_{-1}$) is always highly significant but generally not statistically superior to the variable $\log K_{-1}$ by itself. Even if we accept the suggestion that R indexes capital "vintage" rather than accounting practices, the sense in which the results support the capital "embodied" hypothesis is unclear. The use of (log R) as a separate variable would have permitted a more convincing test. As presented, it is difficult to determine how well the (log R) part of the variable ($\log R \times \log K_{-1}$) performs.

Although the authors are aware of the crudeness of their "quality" correction for labor, the corrected variable gives more plausible results in seven of the fifteen industries than the uncorrected. This suggests that a more detailed investigation of variation in labor quality using more refined corrections will be a fruitful undertaking. In particular, data in the 1960 Population Census (occupational breakdowns by industry, by state, and by sex) permit one to devise a "quality" correction which is industry-specific in each state, obviating the use of a variable which is only state-specific and which probably contains large errors when applied to a specific industry in that state.

Hildebrand and Liu assume imperfect competition, and the elasticity of establishment product demand appears in some of the estimated labor demand coefficients. Unfortunately the elasticity is not identifiable, nor are indepen-

dent estimates available. Nevertheless, the magnitude of the elasticity is central to their conclusion that manufacturing establishments had a surplus of production workers in 1957. The extent of overemployment vis-à-vis the estimated profit-maximizing quantity varied from an astounding 64 per cent in food and kindred products to a not-inconsequential 9 per cent in rubber. To obtain these estimates the authors use the identity relating the marginal revenue product per dollar of wage cost (MRP), the value marginal product per dollar of wage cost (MPP), and establishment output demand elasticity (η). An estimate is obtained from the production function of MPP and a plausible value of the demand elasticity is assumed, yielding the marginal revenue product per dollar of wages. From this figure the implied employment adjustment necessary to set the MRP equal to unity is deduced. The authors' estimates of labor surplus are sensitive to small errors in the estimates of MPP. Moreover, the authors' findings are dependent critically on their definition of (η). Using an argument which implies constant market shares, η is interpreted as the elasticity of market demand facing the *entire* industry. However, had η been interpreted as a weighted average of the within-industry cross elasticities and the entire product market demand elasticity, much larger absolute values of η would have been plausible and, as a result, the authors would have obtained a much smaller estimate of labor surplus. Finally, because of the central role given to the surplus of labor conclusion, it is unfortunate that the authors do not provide a convincing explanation or identification of the adjustment "cost" which lead to the amount of overemployment suggested. For many industries, unionism cannot explain this finding.

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Economic History; Economic Development; National Economies

Explorations in Enterprise. Edited by HUGH G. J. AITKEN. Cambridge: Harvard University Press, 1965. Pp. x, 420. $9.75.

In 1944 the Committee on Research in Economic History reported to the Social Science Research Council that "the role of entrepreneurship in the emergent American economy" was one of the two major areas, the other being the role of the government, in which research should be stimulated in the postwar period. The chairman of that Committee, Arthur H. Cole, later was instrumental in establishing the Harvard University Research Center in Entrepreneurial History, which was in operation from 1948 to 1958. Hugh G. J. Aitken, one of the early associates of the Center, presents in this volume a selection of 18 articles illustrating the nature and the scope of the Center's research output. The book contains also a general introductory chapter by the editor, dealing mainly with the intellectual history of the Center, and shorter introductory notes to each of the four parts into which the book is divided. Aside from the first part, which contains five articles on theoretical approaches to entrepreneurial history, the book's division into parts labeled

"Entrepreneurship in the Community," "Entrepreneur as an Individual," and "Historical Varieties" is rather arbitrary.

The sample of studies reprinted here has the major virtue of bringing out clearly what the scholars associated with the Center achieved and what they failed to do. Their main achievement was to stimulate and carry out valuable research in an area which until then had received very little attention. What they never achieved was to work out a basic theory of entrepreneurship that could be tested historically. Without such a theory, genuine advance beyond the present stage will be difficult, and the information provided by further individual studies will necessarily remain of limited value. Lack of an adequate theory to evaluate the significance of entrepreneurship in relation to other factors producing economic change can easily lead to historical misinterpretation. An illustration of this danger of erroneous inferences is contained in the article on French businessmen, by David S. Landes, included in this collection. In 1951 Landes concluded that the attitude of French entrepreneurs was a serious inhibitive influence on economic development. The unprecedented growth of the French economy since then raises grave doubts about his assessment of the entrepreneurial factor, and his recent attempt (in an article not included in this collection) to rescue the substance of the earlier evaluation is not altogether successful.

Whether it is possible, or even meaningful, to devise a theory to assess accurately not only the productivity but also the supply function of the entrepreneurial factor remains to be seen. The editor's comments and the articles dealing with approaches to the historical study of entrepreneurship indicate quite plainly that even the fundamental questions of how and what one actually studies when one studies entrepreneurship in an empirical context were by no means settled at the Center. Entrepreneurship, the article by W. T. Easterbrook rightly reminds us, is a theoretical concept. Schumpeter's contribution stresses that entrepreneurship relates to a function, not to a physical person and much less to a class of persons. For Schumpeter this function is restricted to innovation, but Cole uses the inclusive definition of "the utilization by one productive factor of the other productive factors for the creation of economic goods" (p. 33). It is difficult to see how one would distinguish between ordinary labor and entrepreneurship in his definition. On the other hand, both Cole and Schumpeter are thinking only of capitalist entrepreneurship. The attempt to reduce the theoretical to an historical creature, to paraphrase Easterbrook, proved the entrepreneur to be a very elusive individual. The recourse to the concept of role—another theoretical concept but this time borrowed from sociological theory—adopted by T. C. Cochran does not go very far in resolving the basic problem confronting the economic historian in this area.

The most promising approach to the determinants of entrepreneurship is that developed by Leland H. Jenks, who concentrates on the social psychological aspects of the entrepreneurial personality and its relation to the cultural environment. Since there is general agreement that entrepreneurship is a form of behavior, highly productive behavior, it is meaningful to explore both the

psychological aspects of this behavior and its interaction with the social historical context. The disciplines involved can suggest behavioral hypotheses that the economic historian may be able to test through comparative studies. In this manner the historian may some day be able to give the economist a better answer to the vexing question of whether development generates entrepreneurship or vice versa.

It is obviously impossible in a review to make detailed comments on all of the individual contributions in a collection of articles. The collection itself is well balanced in that it illustrates the different kinds of emphasis in the work that has been done in entrepreneurial history; some of it focuses on the social historical context of entrepreneurship, some on particular individuals or specific groups of individuals, and some on specific innovations. My own, perhaps biased, judgment is that the articles which yield the greatest insight into process of economic change are those which are comparative or lend themselves to comparative study. This applies especially to Cochran's comparison of business behavior in Latin America and in the United States, but also to the articles by Landes on France, Pelzel on Japan, and Rosovsky on serf entrepreneurship in Russia. The worst kind of work which passes for entrepreneurial history is the antiquarian treatment of individual businessmen. The studies of individuals included in this collection are fortunately not guilty of that fatal disease. They do illustrate, in fact, how individual instances may be used effectively to illustrate points of more general significance. All in all, this collection of studies makes profitable reading for anyone interested in economic history or development. In addition it presents an interesting account of an intellectual venture and its fruits.

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Le Scandale du développement. By J. AUSTRUY. Collection "Bilans de la Science Economique." Paris: Marcel Rivière et Companie, 1965. Pp. 535. F30.

Despite persuasive attacks by theorists and repeated disappointments experienced by developing countries, there is still considerable consensus on the analytical tools and policy instruments appropriate to economic development. J. Austruy sets out to attack this "conventional wisdom." What distinguishes his attempt from many others is the absence of an ideological commitment either to the market or to central planning and collective ownership. The result is a book-length essay, which this reader finds somewhat disappointing on balance despite a number of stimulating points.

What is the scandal of development? Is it the ethical problem of large and nondiminishing inequality of incomes between haves and have-nots? Is it the political and administrative "mess" so often said to surround the assistance given (or not given) by advanced countries to underdeveloped ones? Or is it intellectual fraud perpetrated by theorists and practitioners of development in claiming the insight needed to analyze and advise? All of these pose problems in the author's view, but he sees the true scandal of development as a more

fundamental one, namely the upheaval which development entails for the traditional society. Development is not a uniform progression along some well-marked road from below to above, from darkness to light, from penury to affluence. Rather, it is a fundamental transformation which makes possible the changed ends and means necessary for a new ordering of the society, an ordering whose main operational feature is the ability to sustain economic growth. The reasons for holding this view, and its implications, are set out on two levels, that of analytical concepts and techniques, and that of policy goals and choices. In both cases the villain is partial analysis, in particular the tendency to isolate the economic dimensions of the problem and to find a separate solution for this subsystem.

On the level of analysis, Austruy rejects both homothetic growth and the successive equilibria of stages as adequate bases for analyzing the development process. Nor is the removal of barriers to the achievement of an efficient static allocation of resources a fruitful approach. In support of this attack on static and partial development economics, the author reviews in critical detail many of the familiar tools of analysis, such as the Harrod-Domar model, the use of incremental capital-output ratios and other investment criteria, international comparisons of GNP per capita, etc. But the arguments are familiar, if carefully spelled out, and are in fact taken with abundant reference from the standard French and English writings in the field. More important, there is no attempt to provide even a clue to more-adequate tools.

Development policy should not be an attempt to emulate the historical experience of the West, or to achieve any simple goal such as free operation of perfect markets or a higher saving ratio. Austruy feels that the underdeveloped society is a stable one, and that development requires action which is neither automatic nor rational in terms of existing values and structures, but can only be justified *ex post*. A "power" or a leader is required to impel the society to act in ways which are rational only in terms of a value system and social organization not yet achieved. Here Austruy makes an interesting dynamic argument for development as a collective good even though he leaves unanswered, and indeed unasked, the question of how to decide which potential leadership will commit the society to development, thus justifying interference with the preferences of producers and consumers in the as-yet-undeveloped economy.

Though it may be difficult to make operational, the idea of a political "leading sector" is a stimulating one, and there are other good ideas in the book. Thus it is argued that affluent societies should recognize the growing scarcity to them of a cultural (and natural) heritage not shaped by the industrialized environment, and should therefore help developing countries transform in nonviolent ways in order to preserve more of this heritage. Less original is the emphasis on the constructive, not to say crucial, nature of disequilibria in understanding and managing development. In treating this theme, Austruy owes an acknowledged debt to Hirschman's approach, and one less clearly recognized to the Schumpeterian view of growth. The focus on the role of disequilibria in the transition from the stability of backwardness to that of

sustained growth permits some telling criticism of apologists for the major economic systems, as well as an elegant demonstration of the intellectual superiority of the Marxian over the Rostovian dynamic in terms of the logic of transition between stages.

The Scandal of Development, which purports to be a provocative stand, turns out to be a restatement, and a rather elegant one at that, of some themes as familiar to development economists as they are awkward to deal with: that everything depends on everything else, that simple concepts do violence to the complex nature of reality, that development involves social as well as purely economic change, and that it will progress more rapidly and smoothly by enlisting active forces in the society than by relying on imported institutions and alien behavior. But if economists and policy-makers have erred in ignoring these strictures, it is due as much to the difficulties of coping with interdependence and of analyzing and affecting noneconomic aspects of social change as to any predilection for the neatness and simplicity of partial equilibrium static models or to naïve ethnocentrism. Austruy provides no help to those who, like him, are dissatisfied with the conventional wisdom, but, unlike him, are compelled to measure and plan and make choices. However, it is easy to forget even known limitations of familiar techniques and habits of thought. Austruy's book will serve as a salutary methodological reminder to development economists that their tools fail to grasp the total social phenomenon that is development even in its economic dimensions.

The book contains abstracts of some 150 significant books and articles in the development literature which can provide non-French economists with a useful survey of the growing French literature on economic development.

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Dezvoltarea economica a Rominiei, 1944-1964. Bucharest: Editura Academiei Republicii Populare Romine, 1964. Pp. xvi, 787.

This volume is a monumental work attesting fully to the remarkable achievements of the Rumanian economy since World War II, and in particular since the inauguration of the present six-year plan (1960-65). The history of the two decades, 1944-64, is analyzed in 20 chapters contributed by 84 experts (most of them professors), all of whom give expression to Rumania's pride in the country's economic progress under postwar socialism (but not necessarily as a result of cooperation with the USSR).

After an introduction characterizing the prewar economic system, the book takes up postwar reconstruction, then turns to the policies adopted to implement socialism through nationalization and central planning, and finally devotes three chapters to international economic relations. The dynamic metamorphosis of the Rumanian economy is explained on the basis of measures taken to mobilize the nation's plentiful resources. Numerous tables and charts trace impressive increases in industrial investment and production—the main-

stay of the country's spectacular economic growth (13 to 14 per cent per annum).[1]

All statistics include the year 1963 and reveal the drastic changes which the country has experienced. On the basis of 1950 = 100, the 1963 Rumanian index for investment expenditures equalled 654, producers' goods 621, construction 549, foreign trade 425, national income 330, per capita national income 286, while, for example, consumer-goods output lagged relatively behind at 374, real wages at 209, and agricultural production at 174. These and other data (here omitted) leave no doubt about the priorities which the nation's autarchic planners have established. Governmental appropriations for investment, amounting to 65 per cent of the national budget, are also a symptom of the country's relentless drive for industrialization. (Between 1950 and 1963, expenditures to "finance the national economy" rose gradually from 10 billion lei to 50 billion lei.)

These capital expenditures have been supported by social policies, especially in the area of education and technical training, so that between 1955 and 1963 the number of students enrolled in technical schools increased 4.5 times, and of those taking day courses, 5.5 times. Metallurgy and engineering more than any other industry have benefited from this development. Thus when the 1963 statistics are compared with those for the prewar years, the conclusion is inescapable that Rumania is no longer as backward as it used to be, although it is still an underdeveloped nation.

Foreign trade, whose expansion the government has fostered vigorously, shows an increase of 104 per cent between 1958 and 1963. (Exports and imports rose by 95 per cent and 110 per cent, respectively). The Soviet bloc continues to account for 70 per cent of the country's external commerce, although its growth lags behind the tendencies shown by the afore-mentioned figures; between 1959 and 1963 trade with the Soviet orbit gained by only 62 per cent. An entire chapter (19) deals with the scope and forms of socialist trade and cooperation. However, the qualitative judgments frequently passed in our press about the future of intra-Soviet bloc integration are absent in this volume.

Rumania's economic independence, which the West has found promising also from the political point of view, is evidenced in the rates at which the country's exports to and imports from some West European countries have progressed. Thus between 1958 and 1963 imports from England increased seven times, from Italy five times, from West Germany three times—to furnish the most important and most impressive illustrations. Nevertheless, combined exchanges (exports as well as imports) with these three Western countries represent only about 15 per cent of total Rumanian foreign trade turnover.

The entire book meets the contemporary need for detailed information about the independent course which Rumanian leadership—still solidly Stali-

[1] Cf. Karel Holbik, "An Economic 'Miracle' through Investment Priorities?—The Case of Rumania," *Weltwirtschaftliches Archiv*, 1965, *94* (2), pp. 307-36.

nist—has pursued in loosening the country's economic ties with the Soviet bloc by expanding and liberalizing trade relations with the free world. While formally adhering to Marxism, the Rumanians have not permitted the Marxist dogma and the various ideological myths of the Communist movement to hamper their drive for economic modernization. Their determination to implement the latest technology has caused them to seek continously new contacts with Western nations. This policy has already reaped substantial benefits, thereby imparting an unforgettable lesson to their East European comrades.

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Regional Development and Planning—A Reader. Edited by JOHN FRIEDMANN AND WILLIAM ALONSO. Cambridge: The MIT Press, 1964. Pp. xvii, 722. $9.75.

The selection of contributions that are essential to the understanding of a field, represent the best produced so far, and provide the reader with an integrated view of the subject is never a small undertaking. It is difficult even in an established, well-defined area, such as price theory, public finance, or monetary theory; but to prepare a Reader for a new, ill-defined field which covers many disciplines and whose boundaries need to be established is a truly challenging assignment. Regional development and planning has attracted much interest in recent years, but so far it is not characterized by a large number of profound writings, a textbook that organizes the field, or even an agreement that it does, in fact, constitute a well-defined field of inquiry and, if so, what its nature is.

William Alonso and John Friedmann have written a general introductory section plus introductory notes to each of the volume's four parts. Nowhere can this reviewer find an explicit statement as to what the editors consider answers to the following questions: What is the field of regional development and planning? Why is it a separate field? What are its precise boundaries and unifying principles? Instead, the authors inform us that the volume is designed to provide a foundation for realistic thinking about the regional problem. They divide the volume into four major parts—space and planning, location and spatial organization, theory of regional development, and national policy for regional development. The first part contains a thoughtful paper by Francois Perroux which develops a number of theoretical concepts about economic space. It also has an incisive paper by Lloyd Rodwin on "Choosing Regions for Development." In this paper Rodwin surveys contributions, mainly of economists, designed to establish criteria for selecting the location of new economic developments. He also surveys some key empirical studies and concludes that economic growth and comparative advantage theories suggest the desirability of concerted efforts designed to develop a few "growing points" in preference to spatially more evenly distributed development. The

third article, by editor Friedmann, is primarily a promotional piece on "Regional Planning as a Field of Study." It does not appear to accomplish what it sets out to do.

It is hard to see why these three papers have been selected and placed together to help us better understand the relation between space and planning. Also, it is difficult to see the difference between the concern of these three papers and the next seven that make up Part II, "Location and Spatial Organization." This part includes a fine paper by editor William Alonso which surveys location theory, a paper by August Loesch, "The Nature of Economic Regions," which by now is a classic, and papers by geographers Berry, Ullman, and Morrill.

Part III is entitled "Theory of Regional Development" and has three sections: "Resources and Migration," "The Role of the City," and "Problems of the Rural Periphery." The power of the theory developed in this section varies greatly, since some of its 14 papers are mainly descriptions and historical reviews. The introductory note to Part III gives little insight why such a threefold division is useful for either the evolving of a regional development theory or an organization of the existing literature. However, the papers under "Resources and Migration" are among the best reproduced in this volume.

Perloff and Wingo in their paper, "Natural Resource Endowment and Economic Growth," effectively develop the concept that resource endowment is rooted in the determinants of final demand, i.e., consumer preferences, income distribution, foreign trade, etc., no less than in the current organization and technology of production. This permits the placement of resource endowment into a dynamic framework where, along with its changing composition over time, changes in the comparative advantage among regions supplying material inputs and services for the national economy will accrue.

There is also the four-part dialogue between Douglass North and Charles Tiebout. This is an attempt to relate location theory to regional economic growth, with special emphasis on the question of how we can best explain U.S. economic growth. North argues that Europe by and large went through three stages of development—subsistence, the gradual widening of market area into service with improved transport and development stratum, the basic agricultural stratum, and the gradual shift from extensive to intensive farming. U.S. development, however, depended much on export markets, which in his opinion were of crucial importance in shaping its regional economies. Finally, there is a very thought-provoking paper by Bernard Okun and Richard Richardson examining the effects of migration between regions of different development and income levels.

The final Part, "National Policy for Regional Development," is made up of 11 papers organized into three sections—"Organization for Regional Planning," "Objectives and Evaluation," and "Regional Development Strategies." It includes an excellent paper by Paul Ylvisaker which tends to develop a philosophical basis for allocating powers among various governments. Charles Leven, in an able paper, considers goals for regional development; and Edgar

Hoover and Benjamin Chinitz discuss how regional accounts can contribute to the study of regions.

This volume resembles a potpourri. There is hardly any structure that holds the pieces together and little discipline has been used to prevent the field of regional development and planning from encompassing virtually all aspects of development and planning. This fact will not help remove "a lack of professional dignity" associated with regional development and planning which, according to one of the editors, is very prevalent in the minds of scholars today. A second drawback is the volume's view that regional development planning mainly is and should be national. I think this stems in part from the editors' misconception which is expressed in one of their introductory notes. They look upon planning regions as areas where socioeconomic functions are carried out. While this is an important consideration, they do not pay enough attention to the governmental unit which has the power to plan, develop, and operate regional programs. Thus there should be more emphasis on planning by subnational regional administrative units, and on the interplay between national and regional planning for the solution of specific problems.

Finally the volume is particularly short on planning and development tools. If, as in this reviewer's opinion, one of our main interests in regional development and planning is to improve our understanding of subnational growth, its monitoring, its evaluation and guidance, forging tools for analysis and policy for regional planning are of crucial importance. And so are the design and construction of regional information systems.

There exist very few books that deal with regional development and planning. This Reader brings into one volume many articles of high quality and this should prove useful to those concerned with these problems.

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Latin America—Today's Economic and Social Revolution. By JOHN P. POWELSON. New York: McGraw-Hill Book Co., 1964. Pp. xi, 303. $9.00.

In recent years there has appeared a number of studies from academic, journalistic, and governmental observers interpreting Latin American revolutionary fervor. Students of Latin American-U.S. relations are thus aware of many divergences of economic opinion and policy between the two areas. John Powelson's contribution to the field is twofold: he offers an explanation for these differences; and he appraises the relative merits of each position. Writing from a broad background of experience in Latin America, he displays a capacity for asking hard questions on a number of topics, some of the chicken-and-egg variety, and scores some critical points on both U.S. and Latin American economic thinking and policy.

The topics Powelson examines include agrarian reform, "monopoly" (the market mechanism, foreign investment), commodity agreements (with four case studies), inflation, economic integration, foreign aid, and national eco-

nomic planning. On these issues, Powelson presents the U.S. and Latin American positions along with the economic reasoning (presented in a nontechnical manner), the philosophic values, and historical experiences buttressing them. He then takes a stand on the basis of his analysis. In this reviewer's estimation, Powelson's book is important and worthy of a wide audience both north and south of the Rio Grande. It explains much and contains many fresh insights. It should be consulted by students of Latin American economic development whether academic or otherwise, and by Latin American intellectuals seeking a mirror of their own world and a window on ours.

Powelson's approach is flexible and experimentalist. He is willing to go a considerable distance down the road with Latin American views. Thus, for example, after weighing Raul Prebisch's argument that productivity benefits accrue to the economically advanced nations, Powelson observes that "North American economists should not discount the Latin American position as much as they have in the past" (p. 75). With regard to commodity agreements, his view is that they can be of assistance in easing the adjustment to long-run adverse terms of trade and in moderating the cyclical impact in Latin America.

Regarding inflation, there are instances, Powelson argues, in which a controlled inflation of from 4 to 6 per cent a year can contribute to capital formation. People have to be duped by the money illusion, and money incomes should be rising faster than prices. In general, while he is dubious about the beneficial effects of inflation, he recognizes that the shark of political chaos can swim in the wake of monetary austerity.

It is perhaps on the issue of protection that Powelson moves farthest from the orthodox economic position. He argues that to follow the precept of comparative advantage is not necessarily the road to economic development; protection may be necessary. Briefly, Powerson stresses man-made factors as determinants of comparative advantage rather than natural resources. The present Latin American industrial structure is not conducive to developing the requisite skills and external economies. Regional preferential schemes (LAFTA) may bring greater capital formation with returns to scale and external economies which negate any short-run cost increase.

Powelson offers creative policy suggestions. He mentions several ways, for example, in which our agricultural surpluses might be used to foster land reform. Powelson also sees the need for a new type of international agency with an available skill pool that would assist governments in seeking out and making detailed feasibility studies of specific projects and also train governments how to do these things. The Latin American predilection for macroplanning, he claims, is misplaced.

Powelson poses this dilemma: why do economists, striving for objectivity, tend to divide on policy recommendations along lines that reflect their geographic origin? He implicitly poses two hypotheses: economists unconsciously rationalize the interests of their own nation into "immutable laws"; and different economic laws prevail from time to place, yet the economist has a difficult time in seeing those that relate to an environment not his own. In

the large, his worthy volume indicates that both hypotheses are partly correct.

Powelson sees the historical policy split between U.S. and Latin American economic policy recommendations as narrowing. The breach, he argues, has existed largely because of the variations in the historical and cultural experience of the two areas. His discussion of these topics is quite good. Since the depression of the 'thirties and consequent domestic social changes, Powelson asserts, the U.S. view is changing toward a more collective approach to problems more familiar to the Latin American mind.

Powelson has done a good job of defining the many pressures shaping U.S. and Latin American foreign policy views. His book raises deeper questions which can perhaps only be answered as time unfolds. Can existing conservative elites really reform Latin American social and economic structures? Can Latin American intellectuals stop regarding the United States as the devil with the will and means to "dominate" Latin America, and instead regard us more as the angel, both well-intentioned and offering proper policy guidance? Knowing the causes for our policy, can they afford to be any less suspicious of the effects that might ensue from implementation? Can the United States really be sincere in insisting on meaningful land, tax, and social reforms, and not mere palliatives, when to do so may be to open Pandora's box?

It is Powelson's contention that "The Alliance for Progress is far more than 'mere changes in strategy to counter a growing revolution in Latin America' " (p. 31), and that the success of the Alliance may well hang on our sincerity in espousing agrarian reform. One wonders however, after recent events in the Dominican Republic and the number of military strong men who have risen to the top in Latin American nations, whether Latin America will accept Powelson's benign view of the Alliance.

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National Growth and Economic Change in the Upper Midwest. By JAMES M. HENDERSON AND ANNE O. KRUEGER. Minneapolis: University of Minnesota Press, 1965. Pp. xiii, 231. $7.50.

The Upper Midwest Economic Study (UMES) has published some 28 study papers, urban reports, and technical papers. This volume draws upon these papers and is the final general report of the study.

The UMES covers the Ninth Federal Reserve District and includes Montana, North Dakota, South Dakota, Minnesota, the Upper Peninsula of Michigan, and 26 counties in north and west Wisconsin. In 1960 the area contained some 6.3 million people. Relative to the nation, a sizable number of these people live in nonurban locations. As is probably familiar, the region is closely tied to a natural-resource base. Like many other natural-resource-based economies, the transition to another economic base has given rise to a number of problems. A major purpose of the study is to understand these problems.

The general framework of the study consists of three components: (1) on the basis of the 1950-60 data and other data, project various income and employment levels; (2) analyze the various industry groups in the region; and (3) suggest policy implications without endorsing any specific proposal. Naturally, the three components are interrelated, but for purposes of review they can be treated separately.

The basic model might best be described as a commodity-flow approach. For each of the six states and part-state regions, 38 industry sectors are identified. Twenty-five are goods-producing and the remaining 13 are service-type activities including government. Each of the six Upper Midwest regions is identified along with eight regions of the rest of the United States, Canada, and foreign nations. Next, sales of, say, grain products in North Dakota to the Pacific Coast states depend upon what amounts to a disaggregated consumption function relating total purchases of grain products to Pacific Coast income. Then a trade coefficient determines what proportion of Pacific Coast grain products will be purchased from North Dakota. Finally, the North Dakota income that is generated per dollar of grain-products sales is estimated. This is done for all industries in the six Upper Midwest regions.

As can be deduced, this type of model assumes that a region's growth depends both on the growth of the regional markets it serves and the growth of demand for the particular commodity. Thus, unlike a simple export-base model, the UMES model disaggregates the export market into regional components. Empirical implementation requires, among other things, regional consumption functions, determination of trade flows, regional income per dollar of sales, and estimates of income for the various regions. Given parameters for the first three, National Planning Association regional income projections and other estimates for 1975 for non-Upper Midwest regions form the basis for a projection of income and employment by industry groups in each of the Upper Midwest areas.

In addition to projections of industry activity in each Upper Midwest region, various industries are analyzed in separate chapters. These include: agriculture, forest, water, and recreation resources, manufactures, transportation, services, and government along with a chapter on human resources. Intermingled with the discussion of particular industries are various policy implications. Much of this discussion is too particular for comment here.

The study group did not spend all of their effort "implementing a model." The discussion of the agricultural sector, for example, points up the trends and problems quite clearly without any particular need to refer to the agricultural projections emanating from the model. This is true for other sectors as well. Thus one may wonder why the study bothered to develop and implement a model.

One answer may simply be that it is needed for projections. If it predicts, fine. Yet projections of nonagricultural employment via the model vis-à-vis a simple extrapolation show North Dakota and South Dakota gaining more in the case of the extrapolation than the model projection, and Montana, Minnesota, and Upper Michigan gaining less in the extrapolation than the model

[p. 27]. In the last chapter data on actual performance since 1960, the cut-off period, show North Dakota and South Dakota with more employment than the model projects and Montana, Minnesota, and Upper Michigan with less. (No data are available for the north and west Wisconsin region.) Thus, for the three years since 1960 the extrapolation seems to be the better forecaster, granting 1975 is still a long way off and things may change.

Of course, forecast or projection accuracy is not the only reason for constructing a model. Supposedly, one wants to know why things are changing, especially if something is to be done about unfavorable changes. Thus, if the model helps explain the underlying structural changes, it serves a useful purpose. How well does the model perform in this regard?

Essentially the UMES model is an export-base model with exports disaggregated by region. Activities sales are either local or export. What kind of local sales, to a consumption sector, to business investment, or to interindustry, is not known. Also exports are not identified by separate sectors, e.g., there are no sales to the federal government. Thus, how much of the change is associated with changes in these sectors is not revealed. Evidently this type of information was traded off as a research cost in favor of the disaggregated regional breakdowns. The advantages of disaggregating regional markets has a certain amount of appeal. Yet one does wonder if the export sales of a firm such as Minnesota Mining and Manufacturing will depend upon regional changes or national changes. To the extent the firm serves a national market, differential regional growth will not affect its exports. On the other hand, some industries such as tourism may indeed be tied to regional markets. In fact, one might suppose that service activities are the most likely to be regionally oriented as regards their exports. Yet, unlike the goods-producing sectors, the model handles the 13 service sectors only on a net purchase and sale basis. If an Upper Midwest state's sales of a service exceed its purchases, it is presumed to be a net exporter of that service. In implementing the model, if for some reason the state's income increases, then the model automatically increases these net exports. No doubt this kind of formulation is necessitated by the difficulties of estimating service trade coefficients. Nevertheless, it is a severe assumption.

However, these comments should not be misinterpreted. There is far more to the over-all framework of analysis than this particular model. Real income, productivity changes, and population shifts have been analyzed in a sophisticated manner. The consequences of these changes are clearly spelled out for the Upper Midwest. Further, it is refreshing to see in a regional study phrases such as "employment in this industry will continue to decline"; "increased expenditures on schools are needed if . . ."; "out-migration appears to be the best solution"; and "the project may benefit the state, but its over-all value to the nation is doubtful." While these are not direct quotes, they could well be. By not mincing any words the authors have performed a useful public service.

For the economist interested in the more technical aspects, the various study and technical papers may be more rewarding. Yet, this volume will still

be of interest for it draws together materials from all of the individual studies.

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Statistical Methods; Econometrics; Social Accounting

Measuring the Nation's Wealth, Studies in Income and Wealth. National Bureau of Economic Research, Vol. 29. New York: Columbia University Press, distributor, 1964. Pp. 835. $6.00.

This is a report of the Wealth Inventory Planning Study established by George Washington University under the directorship of Professor John Kendrick. Approximately 150 experts explored the problems and possibilities of a meaningful national inventory of wealth and attempted to develop guidelines for the collection of requisite data and preparation of finished estimates. The study was submitted to the Executive Committee of the Conference on Research in Income and Wealth of the National Bureau.

The advisory committee to the study group stresses that firmly based wealth estimates would enhance our understanding of capital formation and economic growth, and thus future production possibilities and the demand for capital goods. Data would be obtained on the value, amounts, and ages of the stock of capital goods so that one could better understand how capital is allocated among industries from the standpoints of productivity and rates of return. The committee does not recommend a one-time census of wealth. It feels that the collection of wealth data should be tied into existing federal programs so that the scope might be expanded in the future. The director states that the moving spirit behind the project is Professor Raymond Goldsmith, who has stressed the need for better basic data and has advocated eventual regular official estimates of sectoral and national balance sheets. The director established working groups of experts from universities, industries, and government agencies for each of 14 sectors of the economy. These groups were asked to recommend uses of wealth estimates, review existing wealth data and methods of estimate, and to make recommendations for strengthening and expanding the basic data. The sector reports and background papers were used by the staff in submitting its summary report.

The detailed listings of problem areas to be surmounted in taking a wealth inventory are staggering. There are serious statistical gaps. The last census of construction was in 1939. No comprehensive data exist for real estate or for machinery and equipment. Very few sources are useful for tangible wealth estimates for state and local governments. A thorough survey of household tangible wealth has never been taken. No balance-sheet data exist for sole proprietorships. There is a lack of information for many types of nonprofit organizations.

The problems of pricing and depreciation are mentioned consistently throughout the various reports. Large areas of capital goods remain unpriced.

There is the necessity of obtaining basic data on the acquisition cost by type of asset to overcome the book-value problem. It is suggested that it would be useful to collect physical-unit data for those important asset classes which are composed of homogeneous units. Annual price indexes are badly needed for nonfarm land. Much more information is required concerning lengths of life of depreciable assets and the typical pattern of depreciation. Surely the price index, depreciation, and service life problems of consumer durables are difficult.

Many suggestions are made for initiating studies and strengthening censuses. It is recommended that wealth data be collected for each industry on an establishment basis wherever feasible. The suggestion is made to ask sole proprietors to file a balance sheet with their tax return in an inventory year. Private owners of land and other natural resources should be asked to estimate their current market values on land and natural resources. Financial assets of very wealthy persons may be difficult to determine in a consumer survey. One poll of art galleries indicated one-third of them would be willing to provide data on costs and values of their collections.

The definition of wealth chosen by the Wealth Study groups is broadly defined as all resources which contribute to the production of goods and services that men want. Perhaps unfortunately, only nonhuman wealth is treated in the book. Human wealth is not considered for "practical reasons." The legal distinction between men and property is presented. The lack of a market to provide the valuation of human capital is cited as a deterrent. It is interesting to study the census of wealth in 1860 in the United States in which each person was asked the value of his real estate and personal estate. In Mississippi, the 437,000 slaves were evaluated at approximately one-half of the $750,000,000 total wealth valuation for the entire state. If the 354,000 whites had been considered to have had a value equivalent to slaves, human wealth would have been about two-thirds of total wealth. Today, the concept of owning oneself may be substituted for the repugnance of slavery. It is not without reason that wages and salaries are over 70 per cent of personal income. If one of the stated objectives of a wealth study is to make clear the relation between national income and national wealth, human capital must be considered.

It is difficult to understand how carpets, cups and saucers, cows, and the book value of plant and equipment of institutions of higher learning are as important as the services of humans. If the wealth-size distribution of firms and establishments is to aid in studies of efficiency, account could be made of the distributions of age, education, and individual incomes of the persons employed in each of the establishments. The problems of yearly pricing in the form of income and the problem of depreciation perhaps are not insurmountable in endeavoring to allocate our most precious resource.

The Wealth Study Committee does not deny the central importance of human capital and welcomes studies which may be integrated with its proposals. It has set its goal as one of enhancing the understanding of capital formation based on wealth estimates. The main purpose of the staff report has

been to provide a consistent conceptual framework and general statistical guideline for the subsequent detailed work. This it has achieved.

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Economic Systems; Planning and Reform; Cooperation

Yugoslavia—The Theory and Practice of Development Planning. By GEORGE MACESICH. Charlottesville: The University Press of Virginia, 1964. Pp. ix, 227. $5.50.

The major effort in Professor Macesich's book is to defend a system of flexible prices and to refute rigid central planning as a device to achieve rapid economic development. Only a minor part of the book is devoted to the theory of planning. Chapter 4 deals with planning in general, and Chapter 5 with the specific Yugoslav approach, but neither of these chapters provides important new insights. Chapter 4 relies heavily on Lange's prewar blueprint of market socialism and Halm's textbook on economic systems; Chapter 5 on Waterston's slim volume on planning in Yugoslavia. The remaining chapters contain mainly a description of Yugoslavia's political and economic institutions, statistics, and sketchy information on the *modus operandi* of the Yugoslav system.

One of the main objectives of the book is to demonstrate that the performance of the Yugoslav economy improved remarkably "once the Soviet model's planning apparatus had been dropped." The author points out that in the period from 1947-52—"when the economy was in a straight jacket of the Soviet type"—Yugoslavia's national income in constant 1956 prices grew at an average annual rate of 1 per cent, whereas under "decentralization" the corresponding rate in the 1952-60 period was 10.6 per cent. There is a widespread agreement among economists in the West and, more recently, also among many Communist economists in Eastern Europe that the Yugoslav combination of substantial socialization with considerable reliance on market forces has been fairly successful, mainly because it avoids some of the misallocations and bottlenecks which occur under central planning through material balances. But there are many possible criteria of economic performance, and even if economic growth is to be considered as the most important indicator of success of the Yugoslav market socialism, its statistical evidence is more complex than the above-cited comparison of growth rates suggests.

The 1 per cent growth figure under highly centralized planning was obtained by averaging wide swings of annual rates: + 12, + 11, − 10, + 11, and − 15 per cent. Macesich himself concedes that the great decrease in real income in 1950 and 1952 coincided with severe droughts at a time when agriculture was weighted heavily in national output. Furthermore, it was probably in 1950 that Yugoslavia began to feel the full impact of the economic blockade by the Cominform countries. Therefore, the 1 per cent growth rate is by no means a meaningful measure of Yugoslavia's perfor-

mance under the old system of central planning. Nor is the 10.6 per cent rate a reliable indicator of growth under the new indirect planning.

The author recognizes that "economic, statistical, and other factors blur the significance of the economy's performance." He notes that the Yugoslav definition of income does not include the output of a part of services, and that the method of computation involves a bias; unfortunately, here as well as elsewhere, he does not attempt to estimate the scope of the bias of the official statistics. In any case, substantive economic problems are more to the point than questions of statistical measurement. Macesich emphasizes inflation as one of the serious drawbacks of the Yugoslav economy. He estimates that "the general level of prices" increased by almost 60 per cent from 1952 to 1960. Although this estimate is not properly documented, and although it is contradicted by the official (probably downward-biased) index of wholesale prices, there is no doubt about the severe inflationary pressures in Yugoslavia. In fact, since 1960, inflation has been gathering speed. Some other economic ills of Yugoslavia are mentioned less emphatically later in the book. Officially registered unemployment went up from 67,000 in 1955 to 236,000 in 1962; Macesich does not estimate the rate of unemployment, but according to Livingston's article in the *Monthly Labor Review* of July, 1964, 6.8 per cent of the wage earners in Yugoslavia were unemployed in 1963. This is a higher unemployment rate than in both West European market economies and East European centrally planned ones. The Yugoslav balance of payments has been deeply in deficit in spite of the substantial foreign economic aid received.

Obviously, the persistence of these unfavorable trends threatens the maintenance of the very high rate of growth. In fact, in the late 'fifties and early 'sixties Yugoslavia's annual increase in real income had already fallen off to 5 per cent or less. Macesich does not mention this deceleration of growth. Also, he does not call attention to the fact that while the growth rate went down, the net investment rate jumped up. His puzzling statement, "As a result of the rapid growth of national income the proportion of net investment to total national income tended to fall" (p. 131), is based on data prior to 1959. Furthermore, his "proportion of net investment to national income"—20.4 per cent in 1957-58 (in constant 1956 prices)—relates *fixed* investment to national income under the Yugoslav definition. When Yugoslavia's national accounts are recomputed in accordance with the usual Western definitions, as has been done in the 1963 OECD survey of Yugoslavia, the net investment rate is almost 30 per cent in 1957-58 and over 34 per cent in 1961 (in current prices).

As a result of this very high investment rate, accompanied by decelerating increases in real income, the marginal capital-output ratio must have risen sharply. There are, of course, many possible reasons for increasing marginal capital-output ratio. Insofar as one of them might be increasing inefficiency in capital allocation, as Macesich implicitly suggests when discussing the underutilization of industrial capacity (p. 137), this would throw additional doubt on the ability of the Yugoslav system to sustain the past growth rate.

One would expect a study in development planning to pay more attention to the capital coefficient than Macesich's did.

This is a rather diverse book. A part of it deals with matters which are related neither to Yugoslavia nor to development planning. For instance, several pages are devoted to the relation $Y = VM$ (where V is supposed to be a function of equity and bond yields, real income, price chances, etc.) and to monetary velocity in Canada. But when it comes to Yugoslavia, Macesich simply divides national income in *constant* 1956 prices by total money supply with the result that "velocity" measured in this way appears to have fallen to an almost incredibly low level of 1.83 in the inflationary year of 1961. Some of the occasional theoretical pronouncements are difficult to follow, if not incorrect, and some of the explanations of the Yugoslav planning methods raise as many questions as they answer (for instance, profitability of investment projects is supposed to be one of the criteria applied by the Investment Bank for granting credit, but Macesich does not explain how "profitability" is determined in view of the very low and very discriminatory interest rates). The author's economic philosophy and most of his policy recommendations seem to bear the imprint of the Chicago School. This is exemplified by his numerous quotes from Professor Friedman's works as well as by his unreserved advocacy of free international trade under freely fluctuating exchange rates.

Readers who are looking for a translation of the Yugoslav system into a rigorous economic model or for a genuine theory of growth and development will be disappointed by Macesich's study. His book will be of interest to readers seeking general information about Yugoslavia's socioeconomic system. It contains a few interesting statistics which are not readily available in other Western sources (for example, Table 31 provides useful data on employment, fixed capital, and net product in Yugoslavia's industry by size of the enterprise). But some of the statistics and some of the author's definitions should be used with caution; for instance, "social product" under the Yugoslav definition is not really "similar to gross national product" as the author claims on page 16; it excludes a part of services, and in recent years it has been approximately one-tenth below Yugoslavia's GNP under the usual Western definition.

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Business Fluctuations

Evidences of Long Swings in Aggregate Construction Since the Civil War. By MOSES ABRAMOVITZ. New York: National Bureau of Economic Research, Occasional Paper 90, 1964. Pp. 240. $4.00.

This carefully delimited monograph is intended to take its place among a sizable phalanx of predecessors and successors originating primarily from the National Bureau of Economic Research and treating of one or another aspect of what are nowadays known as Kuznets cycles or long swings in the rate of

U.S. economic growth. The title is an accurate reflection of the spirit as well as the substance of the main body of the paper, which consists of a notably careful but strictly empirical appraisal of the statistical evidence concerning the existence of long swings in construction activity since the Civil War. Fortunately for the reader who likes to see theory used at least as an interpretive guide in empirical research, however, Moses Abramovitz allows his theoretical imagination freer rein in the first and last chapters, in which the findings about construction are placed in the broader perspective of the long swings in aggregate economic activity.

After a brief discussion of the variety of statistical indicators of construction activity and the size of construction in the economy, Abramovitz tackles his central problem in Chapter 4. This is to identify the long swings in aggregative construction and its components from a study of 37 (substantially overlapping) annual time series. The basic need is to distinguish the long swings from the shorter fluctuations associated with business cycles. This is accomplished by smoothing the data to eliminate the influence of business cycles and then using the unsmoothed observations to identify "trend-like movement(s) lasting distinctly longer than an ordinary business-cycle phase, but only if this impression was confirmed by a like movement in the smoothed data."

Once having identified and dated the long swings, Abramovitz prepares a series of measures in the Burns and Mitchell tradition which are designed to test whether the long swings do indeed differ significantly from the shorter business-cycle fluctuations in construction activity. These tests include a comparison of the average durations and amplitudes of the two kinds of fluctuation; a comparison of the durations and amplitudes of the "specific" (short) cycles in construction occurring during the long upswings with those occurring during the long downswings; measures of conformity to bring out the extent to which the various branches of construction regularly participate in the long swings of aggregate construction; and finally, an indirect test based on the growth rates of the construction labor force between successive Census dates. All these tests confirm the existence of the long swings in construction activity.

The existence of a long cycle in residential building or total urban building was established many years ago by Riggleman and Long, and as Abramovitz points out, his new chronology is quite similar to those previously presented in the literature. Moreover, Isard and Silberling long ago stressed the association between railroad (and other transportation) construction and the building cycle. Industrial, commercial, and public utility construction have customarily been neglected in long-cycle studies, however, and the very existence of a long cycle in aggregate construction was questioned by Colean and Newcomb.

What Abramovitz has done is to assemble and assess all the available evidence bearing on the long cycles or swings in aggregate construction in an effort to resolve the basic factual issues. In this he has been as successful as weaknesses in the data on construction activity before World War I permit

for a cautious investigator: "In sum, since the Civil War, there has been a succession of long swings in aggregate construction activity. These consisted of upsurges followed either by protracted declines or pronounced retardations, in which all the major sectors of the industry participated. The widespread participation of the major sectors is a finding important in its own right and also as confirmation of the long-swing behavior of aggregate construction." Notice the qualification concerning the "downswings": on several occasions the absolute declines in the available indexes are sufficiently mild so that they conceivably indicate no more than prolonged retardation in the growth rate of actual construction activity.

It is fair to say that the pioneering investigators were interested in the building cycle primarily because of its apparent connection with the "periodic" appearance of abnormally severe business-cycle depressions. More recently, and in good part due to Abramovitz' efforts in two papers published in 1959 and 1961, attention has shifted to the relationship between the building cycle and the long swings in the rate of economic growth first studied by Kuznets and Burns in the 1930's. This relationship is discussed impressionistically in the concluding chapter of the work under review, but it is to the previously mentioned papers that the reader must refer if he is to become fully acquainted with Abramovitz' ideas on this important subject. Suffice it to state here that he rejects the concept of an independent cobweb cycle in construction, stressing instead that "The very existence of long swings in aggregate construction means that they are part of a wider phenomenon. An explanation must, therefore, envisage an interaction between construction activity and the economy at large in which long swings appear in many facets of economic and demographic change; and these feed back, each in its own fashion, to impose something of a common pattern on the otherwise divergent geographical and sectoral branches of construction." Without attempting such an explanation, Abramovitz in his several papers has nonetheless provided basic insights into the principal ingredients of a complete model of the long swings.

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Money, Credit, and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

Federal Credit Agencies—A Series of Research Studies Prepared for the Commission on Money and Credit. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963. Pp. 491. $7.50.

Federal Credit Agencies, prepared for the Commission on Money and Credit, contains seven research studies on as many direct lending or loan insurance programs. George F. Break writes on "Federal Loan Insurance for Housing" (FHA and VA); Jack M. Guttentag on "The Federal National Mortgage Association" (FNMA); Ernest Block on "The Federal Home Loan Bank System"; D. Gale Johnson on "The Credit Programs Supervised by the Farm Credit Administration" (The Federal Land Bank Board, the Inter-

mediate Credit Banks and the Banks for Cooperatives); Dale E. Hathaway on "The Federal Credit Program for Individual Farm Development" (Farmers Home Administration [FHA] and related programs); George S. Tolley on "The Rural Electrification Administration" (REA); and Jack McCroskey on "Federal Credit Programs for Small Business" (Small Business Administration [SBA] and the relatively new Small Business Investment Companies).

The studies have a nearly uniform structure, dealing with the history of the program in question, with its direct and secondary effects (for example, the effect of home loan insurance on construction activity, on mortgage borrowing costs, etc.), with program objectives (whether they were achieved) and with the question whether the program was justified ("appropriate") in the first place. Finally, there are policy proposals in each of the studies.

It is a useful book, carrying the story of government credit programs up to 1958 or 1959. Along with the special appendix on federal credit programs that appears annually in the President's budget message, it could be used to give factual and polemical substance to that section of college and university courses on banking and financial institutions that deals with the now very large and varied credit activities of the federal government.

It is interesting to compare the policy proposals found in these research studies with those of the Commission. Professor Break's skepticism about the utility of fixed interest rate ceilings (statutory or administrative) on FHA and VA underwritten home loans is matched by the Commission's conclusion that the ceilings should be abandoned. So far, our government has shown little interest in following this (to me) constructive suggestion.

Professor Guttentag's treatment of FNMA, which strikes me as a very professional job, provides an example of the opposite type. He is all for Fanny May, as we know that agency, "leading" the market by "a more aggressive price policy." The Commission, on the other hand, advised that mortgages should be bought and sold at market prices, with no attempt to "control" mortgage prices.

In my opinion, both Guttentag and the Commission are open to criticism. Guttentag fails to give Fanny May credit for a price policy in 1960 aimed at nudging the mortgage rates of private lenders off dead center and starting them down. This is the kind of policy he clearly prefers; and it is precisely the policy that the agency followed in that period. The Commission, on the other hand, is open to the criticism that it underestimates the stickiness of the adjustment mechanism in money and capital markets and underestimates the good that can be done now and then by a nudge or by a push. Of course, the agency can't be too aggressive in such a policy or it will end up with all the mortgages!

This writer was a member of the board of directors of Fanny May during the period in question and can attest to the "stickiness" of interest rates and of bond and mortgage prices, particularly the latter. My own theory of this is that the lenders were so persuaded recession was impossible that they hesitated to bid higher prices for mortgages for fear they would move them to a level which, when capital demands revived, they would be "stuck with" for

too long a period. It is an interesting case in which a little less optimism would have been a good thing. In any case, mortgage prices were not moving as they should have, despite an easing credit situation, and it was Fanny May's deliberate policy to nudge them up, that is, to nudge the long-term interest rate down. It was done in several moves and happily, long-term mortgage yields (and costs) moved down without the agency becoming the sole mortgage lender in the United States. All the same, the process took rather longer than was good for the economy.

For the most part the essays are admirably objective, which is not an easy attitude for a writer on federal credit programs to assume. Gale Johnson's paper on the Farmers Home Administration is outstanding in this respect.

All in all, the work is well done, and the several authors are to be complimented on providing teachers of finance with informative and provocative papers on a subject of growing interest and importance.

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The German Inflation, 1918-1923. By KARSTEN LAURSEN AND JØRGEN PEDERSEN. Amsterdam: North-Holland Publishing Company, 1964. Pp. 138, plus charts. $4.20.

Probably the most widely accepted view of the great German inflation is that it resulted from a vast increase in the quantity of money printed to finance government deficits, and that it had disastrous effects. This short monograph challenges the orthodox views of both the causes and the effects of the inflation.

The authors examine the standard quantity-theory explanation by comparing the increase in the floating debt and in the currency stock with the price increases during a number of subperiods. They find no close connection between the former two variables and the price level; nor do they find any evidence that the rise in the floating debt and in currency were the initiating factors. They therefore reject the standard explanation and prefer the explanation offered by K. Helfferich, J. H. Williams, J. W. Angell, and others which was that the balance-of-payments deficit caused by reparations payments induced a rise in the exchange rate which raised import prices and hence the general price level.

The book presents a model which stresses the impoverishment of Germany after the war when productivity had fallen, the capital stock was inadequate, and the exchange rate had turned against Germany. This exchange-rate movement was due to the high propensity to import resulting from the previous wartime conditions as well as from the low export capacity and the need to make reparations payments. Given those conditions, German wages should have fallen relative to those in other countries. But labor protected its real wages by forcing up money wages as import prices rose. The result could only be continuous inflation, which the authorities financed by increasing the money stock via the deficit. Given these conditions, inflation could only be

prevented by either restricting credit, which would have created large-scale unemployment, or by controlling wages, which was impossible.

Unfortunately, the authors do not really test their explanation, but apply it only in a rather descriptive manner centered on an analysis of relative wages in Germany and other countries. Perhaps feeling that they had destroyed the prevalent quantity-theory explanation, they saw no need for testing their explanation against any other. But what they discredited is only a crude, old-fashioned quantity theory, and they seem to be unfamiliar with Phillip Cagan's excellent study ("The Dynamics of Hyper-Inflation," in M. Friedman (ed.) *Studies in the Quantity Theory of Money*) in which Cagan made velocity a function of expected price changes and got a very good fit for the period September 1920 to July 1923. Karsten Laursen and Jørgen Pedersen give no evidence which would make one think that their theory gives a better fit. Moreover, their use of currency data instead of currency-plus-deposits data for the money stock and their failure to take account of changes in output seem questionable points. Finally, they give no adequate evidence for thinking that the increase in the money stock was induced by wage movements rather than by the deficit.

The second part of the monograph deals with the effects of the inflation on production and income distribution. On the basis of available quite sketchy data, the authors argue that inflation did not reduce output until the hyperinflation of 1922-23, while by contrast the deflationary policies followed in other countries reduced output substantially. They reject the common view that the inflation led to wasteful investment by pointing out that the evidence for this view is extremely crude.

More surprising is the authors' treatment of the distributional effects. They argue that these were not very bad, though price level stability plus full employment would have been preferable. Since they claim that the inflation was a cost-push one they reject the wage-lag hypothesis. Admittedly, the salaried groups, particularly the higher-salaried employees, lost during the inflation, but given the revolutionary mood in Germany and other conditions at the time, the salaried group would have lost relative to other groups in any case. The loss suffered by *rentiers* is de-emphasized by the authors, who state that *rentiers* do not form a homogeneous social group, and hence that individuals who lost in their role as *rentiers* were often wage or salary earners. Their loss was mitigated by their greater opportunities for becoming entrepreneurs. According to the authors, "it must be assumed" that bonds were largely repaid early in the inflation and the funds were invested in business. Similarly, pensions and insurance estates could be repurchased and the funds invested in business. They conclude that, given the lost war, etc., the distribution of income "in its main features" under inflation was "no less equalitarian than it would have been under any possible alternatives; . . . the distribution of income would probably have been less equitable under any alternative policy within the horizon of the period."

The authors give little evidence for many of these propositions. While skepticism about the wage-lag hypothesis is currently widespread, the authors

appear to underemphasize the redistribution between creditors and debtors. Even the comparatively mild U.S. post-World-War-II inflation had substantial redistributional effects in this way, and hence it is likely that a hyperinflation would have much greater effects than the authors claim. In other words, to what extent were debts really prepaid and pensions and insurance estates repurchased? Casual statements about what "must be assumed" are inadequate.

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Money and the Economy. By JOHN J. KLEIN. New York: Harcourt, Brace and World, Inc., 1965. Pp. xii, 436. $7.50.

Money, Banking, and Central Banking. By RICHARD H. TIMBERLAKE, JR. New York: Harper & Row, Publishers, Inc., 1965. Pp. xvi, 352. $6.95.

Money and Banking is probably the most "popular" upper-division course in economics. Consequently a large number of texts for this course is already available. This "large number" exaggerates the number of available alternatives, since many texts are very similar in scope and content. Professor Klein's book is a variation of the standard text; Professor Timberlake has added to the number of available alternatives. Both texts obey precedent in that they review monetary institutions before they explain the logic of the interrelation between the money supply and aggregate economic activity. Both authors describe, in about 30 to 40 pages, the balance of payments, the concept of international equilibrium, alternative remedies for imbalance, and the problem of the adequacy of international liquidity. This reviewer would appreciate a concise theoretical frame of reference at the beginning of each text and wonders whether, in books of this length, the space taken by a very brief coverage of international phenomena might not be better used in giving depth to the analysis of domestic problems.

The virtues of Klein's book—explicitly designed for a one-semester course—are its lucid style; its thorough coverage of private financial institutions and the Federal Reserve System; a brief but comprehensive chapter on the history of banking up to the 1930's; and a very thorough theoretical section which includes a succinct description of the major monetary theories of the past (from Bodin to the Cambridge School). However, while this book is worthy of consideration, some shortcomings must be mentioned. The reader, having noted the eminence of the author, starts with confidence. This confidence is shattered on page 2 by the statement that "checks are money." In his development of private financial institutions the author uses "T" accounts to demonstrate the creation of deposits: unfortunately, he does not clearly point out the need for funds to come into the banking *system* from outside before multiple-deposit creation can take place (pp. 76, 93).

Part II covers the Federal Reserve System, its organization, purposes, functions, monetary arsenal, and historical performance. The monetary tools are examined individually in historical perspective. The assessment of the

System's performance is kind-hearted: "Fifth, unemployment in this period [1958-59] was usually in excess of 5.5 per cent. . . . Hence, our restrictive policies of this period were inappropriate in the light of the employment situation" (pp. 230-31). Unfortunately, this part fails to give adequate coverage to the problem of inflation in the postwar years: in particular, the cost-push and structural theories are omitted, and no explicit mention is made of the price and employment data which generated the theories. The monetary role of the Treasury and debt-management are treated briefly in this part.

Part III gives a thorough treatment of national income. The model relies upon the derivation of IS and LM curves. The determinants of the forces affecting the IS and LM schedules are analyzed separately in some detail so that the IS/LM graph may be used to demonstrate the effects on national income of shifts in the schedules induced by autonomous changes and policy measures. One weakness of this part is that, in the sequential discussion of factors affecting a schedule, the author fails to make explicit the relative importance of the determinants.

The text ends with four short chapters: one on alternative prescriptions for monetary policy; one on alternative fiscal policies, and two on international considerations. Each of these topics merits more detailed coverage than space permits Klein to give. Problems inherent in measuring price level changes are completely omitted—understandably so in a one-semester text.

Timberlake has attempted to add to the number of real alternatives available for a money and banking course. His product is differentiated by its relative de-emphasis of national income analysis and fiscal policy, its use of "historical analysis in explaining monetary and banking development" (p. xiv), and by its brevity which is achieved "when economies of style are deliberately and painstakingly employed" (p. xiv). Unfortunately, the brevity is achieved only at a cost of clarity or of coverage, as exemplified in the too-cursory treatment of deposit creation in Part II and of the theoretical treatment in Part IV. The historical development of the commercial banking system and the evolution of the Federal Reserve System (Parts II and III) are excellent, although monetary policy and events from 1950 through 1958 in the final chapter of Part III are shortchanged with a mere 12 pages, including sections treating the Accord and the "Bills Only" doctrine. Post-1958 events are not covered except for a reference in a discussion of the problem of the current gold outflow.

National income theory is indeed de-emphasized. Thirteen pages cover both "the simple analysis of national income determination" (including the investment multiplier) and "the role of money in national income analysis." However, five pages are devoted to the real balance effect. The section on "the role of money in national income analysis" is unsatisfactory: the flow of saving is shown graphically as being elastic to the rate of interest (p. 234); the transaction demand for money is nowhere stated to be influenced by income and is not explicitly considered in the section; and the omission of IS/LM analysis (or an equivalent) means that the interaction of real and monetary variables is nowhere covered in a general equilibrium sense. The balance of

the part on "Money and National Income" comprises an uncritical summary of the Friedman-Meiselman study of the relative stability of the investment-multiplier and income velocity[1] and of a discussion of the problems and implications of alternative measures of the money stock.

Part V covers briefly the international monetary mechanism. Part VI is an interesting idea in a text. It is designed to acquaint the student with four current issues: the current gold outflow and the possible reduction of the gold stock below the requirement of the Federal Reserve (a problem which has been eliminated for the present by recent legislation); pressure-group inflation; 100 per cent reserves; and the case for reforming the political structure of the Federal Reserve System. The discussion of pressure-group inflation is mainly concerned with casting doubt on the possibility of the phenomenon. The concept of cost-push or sellers' inflation based on the simultaneous existence of price level increases and unsatisfactory levels of employment in the mid-fifties and the idea of a Phillips' curve-type choice between employment and price stability are deemed to result from "an improper use of statistics" (p. 329) on the grounds that the evidence in support of the hypothesis is based on statistical series of insufficient accuracy. Strangely enough, the structural hypothesis of the 1955-57 inflation is nowhere mentioned.

It is an author's prerogative to allocate a predetermined amount of space among competing topics. Timberlake's allocation is very different from that desired in a text by this reviewer. However, notwithstanding allocative differences, the book is of too-inconsistent a quality to be recommended for consideration.

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[1] Milton Friedman and David Meiselman, "The Relative Stability of Monetary Velocity and the Investment Multiplier in the United States," in the Commission on Money and Credit, *Stabilization Policies* (Englewood Cliffs, N.J., 1963), pp. 165-268.

The Money Market and Monetary Management. By G. WALTER WOODWORTH. New York: Harper & Row, 1965. Pp. 496. $8.50.

G. Walter Woodworth's emphasis on the organization of money markets distinguishes his book from the traditional money and banking texts. Following two introductory chapters, the balance of the first part (about a third of the book) is devoted to institutional descriptions and brief analyses of the markets for federal funds, U.S. Government securities, commercial paper, bankers' acceptances, federal agency securities and bankers' certificates, brokers' loans, and other short-term obligations. The second part of the book deals with governmental monetary and fiscal management. Included in this section is a historical and critical description of monetary, fiscal, and debt management from 1914 to 1963. (This occupies a fourth of the book.)

Woodworth says that his purpose in writing this book was twofold. First, he felt that it would fill a void in available teaching materials. Second, it

would provide the necessary integration between money markets and monetary-fiscal management. With respect to the first purpose, the book should prove useful as an elementary source book for descriptive information on short-term monetary obligations. The book is well written, and the chapters on the money-market instruments contain suitable, and fairly complete, references to source material. Woodworth is correct in asserting that the student of economics and business finance ought to have knowledge of the money markets and their instruments, and his book provides good, concise descriptions.

A shortcoming of the book is that the promise of an integration of money markets with monetary and fiscal management is not really attempted. The author could have taken either of two approaches. First, he could have written from the point of view of the prospective financial executive, who needs knowledge of the money-market instruments and of monetary, fiscal, and debt policy to make profitable decisions. The first part of the book fulfills part of this need, but the impact of the government's policy decisions on the money markets is not considered. Nor are techniques of decision-making in financial markets described, with one exception. On the other hand, the difficulty of such an undertaking is indicated in this one instance, where Woodworth described the "practical uses of yield curves." It is true that the yield curve exists and can be drawn for any point in time, but the investor cannot assume that an upward-sloping curve, for example, will persist over time so that the investor can " 'ride the yield curve' downward and reap the rewards of appreciation by selling at appropriate times" (p. 195). I should mention, however, that this lapse is unusual for Woodworth. Most of his book is carefully written, with few unwarranted or unexplained assumptions.

The second approach one might use is the descriptive material on the money markets as a basis for understanding the way monetary and fiscal policy has worked and ought to work. For example, the effect (if any) that different monetary obligations and money-market institutions have on open-market operations could be brought out. On the other hand, if there is no effect, the usefulness of the first section of the book is open to question.

In other respects, Woodworth's book is similar to the many texts in the field. It is nicely done and is better or no worse than many of its competitors. Unfortunately, like so many other textbooks, it appears to have been written as if much of the professional literature in its field had never been published. This is most distressing since in recent years there has been much very important theoretical and empirical work in monetary economics. The task of any textbook ought to be to report and summarize the findings of researchers that otherwise are unavailable to students, either because of limited library facilities or their technical difficulty. For example, Tobin's questioning of the usual definitions of money, Gurley and Shaw's contributions on the relation of financial intermediaries to monetary questions, Friedman's theoretical and empirical studies on the relationship between the money supply and business fluctuations, Meltzer and Brunner's empirical studies on the demand and supply of money, and the many other important contributions to our knowledge must be brought to the attention of undergraduate students. The textbook ought to exist for just this purpose.

To summarize, Woodworth's description of the money markets is a useful addition to the materials available for instruction. His explanations of the workings of monetary, fiscal, and debt policy and of U.S. monetary history are well written and suitably free from misleading generalizations. But, he has failed to integrate the first and second parts of his book and, more regrettably, he joins most of his textbook-writing colleagues in overlooking the important research that has been published in recent years.

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Public Finance; Fiscal Policy

Public Debt and Future Generations. Edited by JAMES M. FERGUSON. Chapel Hill: The University of North Carolina Press, 1964. Pp. 234. $6.75.

During the last five years there has developed a new debate over an old question: Can debt finance lead to a burden transfer from the present to future generations; and if so, by what mechanism?

This volume assembles all of the articles which appeared in the major journals during 1958-63 on this question, as well as relevant excerpts from various books published during this period. Including contributions by Lerner, Buchanan, Meade, Bowen-Davis-Kopf, Vickrey, Modigliani, Shoup, and others, the collection makes lively, if confusing, reading. While the hunt has been great fun, the snark (if it exists) remains an elusive quarry.

To begin with, it cannot be overemphasized that this discussion relates to debt burden in an economy of the classical type, where full employment in the private sector is maintained automatically and no stabilization policy is required. In such a setting, a dollar of loan finance is as deflationary as a dollar of tax finance, both reducing private expenditure by the full amount. The choice between the two means of finance is not made as a matter of controlling aggregate demand. Rather, the question is whether the choice between loan and tax finance may be used to distribute the burden of public expenditures between present and future generations.

The contributors to the debate are agreed that the resource withdrawal from private use must occur at the time when the public outlay is made. This must be the case whether tax or loan finance is used. If burden is defined as "release of resources from the private sector," it is evident that no burden transfer can occur. Some contributors to the debate accept this definition, but most do not. Quite properly, they wish to define the burden on any one generation as the effect on that generation's lifetime consumption or (as I prefer) its potential lifetime consumption.

One way in which this burden may differ under tax and loan finance is through what Shoup has referred to as the Ricardo-Pigou mechanism. Suppose that taxation results largely (or, to simplify, entirely) in reduced private consumption, while borrowing results largely (or, to simplify, entirely) in reduced private capital formation. If generation I taxes itself when a given public purchase is made, its consumption is reduced, and the burden is shouldered

by generation I. If loan finance is used instead, consumption remains initially unimpaired, but private capital formation is reduced. As a result, generation II will inherit a lesser capital endowment, thus reducing its potential (or actual) consumption. The burden is transferred to generation II. To be sure, generation II also inherits the government bonds, but with them it assumes the tax liabilities needed to finance interest thereon. Unless there is a "debt illusion," this constitutes no net gain and cannot offset the lower level of real wealth that is passed on.

This mechanism of burden transfer depends on the condition that loan finance falls more heavily on saving, while taxes fall more heavily on consumption. It does not require overlapping generations. It remains the most clear-cut and perhaps the most important type of burden transfer in the full-employment setting, but it is not the only one. The focus of the recent debate is precisely on demonstrating that there may be other mechanisms as well. A common characteristic of these mechanisms is that the comparison is not between tax and loan finance proper, but between initial tax finance and initial loan finance with subsequent debt retirement. This mechanism will therefore be referred to as the postponed-taxation effect.

One of the earliest contributions to this approach was offered in Buchanan's *Public Principles of Public Debt,* passages from which are reproduced in this volume. While it must be credited with stimulating much of the discussion, Buchanan's position remains difficult to follow, as evidenced by the rather diverse interpretations given it by the various authors. The essential point seems to be that lending is a voluntary exchange and hence cannot involve a burden. A burden results only when taxes are paid, which requires a compulsory surrender of wealth. Hence burden is incurred only when taxes are paid to retire the debt. As long as the debt is not retired, burden is postponed. This is the case whether the released private resources incidental to the initial lending were from consumption or from capital formation. Effects on the future level of GNP, as indeed the entire Ricardo-Pigou effect, are not considered relevant to the point.

A subsequent thesis by Bowen-Davis-Kopf sets out to demonstrate that burden transfer can occur even in an all-consumption economy, without inheritance, where each generation consumes its entire income over its life span. Suppose generation I finances a public outlay by bond issue. As it purchases the bonds, its consumption is reduced. Later on, generation II arrives on the scene, overlapping generation I. At this later period, generation II is taxed, and the bonds held by generation I are redeemed. As a result, generation I increases its consumption, while II reduces its. The burden is thus transferred from I to II. Generation I's lifetime consumption is kept intact, if partly postponed, while generation II suffers the full reduction in consumption. A burden transfer through the postponed-taxation effect has occurred, even though the Ricardo-Pigou effect has not been operative.

The present writer has presented a somewhat similar model of generation overlap, but again the argument is stated in unnecessarily restrictive terms, thereby diverting attention from the basic issues involved. This is the prop-

osition that loan finance postpones the final settlement of an expenditure burden, simply because it postpones the final reduction in net worth which is implicit in taxation. This indeed was precisely the point advanced by Keynes in his *How to Pay for the War,* reference to which might well have been included in the volume.

The fact of the matter is that burden transfer through the postponed-taxation effect can occur in an all-consumption or a capital-formation economy; whether lifetime consumption is assumed equal to lifetime income or not; with or without inheritance; and whether or not the saving-consumption impact of tax and loan finance differs. The only condition which must be met for the mechanism to work is that there should be generation overlap.

While the Ricardo-Pigou effect and the postponed-taxation effect involve distinct mechanisms of burden transfer, they are not mutually exclusive, but may coincide. The conditions required for the Ricardo-Pigou effect (inheritance and differential savings impact of tax and loan finance) are compatible with that needed for the delayed-taxation effect (overlap of generations). Thus if both sets of conditions are met and only part of the initial loan is retired, generation II will be burdened partly via the postponed-taxation effect (which is independent of the initial saving-consumption response) and partly by the Ricardo-Pigou effect (which depends on this response).

Having learned that burden transfer may occur by either of these mechanisms, what are we to conclude about the politician's dictum that debt finance is bad because it "mortgages" future generations?

Firstly, he must be told that this entire debate, as previously noted, refers to a hypothetical economy where full employment is maintained automatically by the private sector. The Ricardo-Pigou mechanism breaks down in an economy where the tax-loan (or tax-loan-monetary) mix is determined by considerations of stabilization policy; and the postponed-taxation effect, in this more realistic setting, can be invoked only at the cost of unemployment or inflation. As far as central government is concerned, the recent debate therefore seems of little importance for a country such as the United States even though it is significant for low-income countries or for local finance.

Secondly, even in a classical setting, it cannot be concluded that burden transfer is necessarily bad. Indeed, the principle of intergeneration equity demands that public expenditures on durables which involve the transfer of intergeneration benefits should be matched by a corresponding burden transfer. This, of course, is the element of validity behind dual budget systems involving a current and a capital budget.

Nevertheless, the fact remains that burden transfer is possible, and the contribution of the recent discussion has been to show that the Ricardo-Pigou effect is not the only mechanism by which it can be accomplished. The editor is to be commended for having put together this interesting volume, which should receive widespread use by students of public finance.

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International Economics

The Dollar and the International Monetary System. By ALVIN H. HANSEN. New York: McGraw-Hill Book Company, 1965. Pp. xxii, 290. $7.95.

In the torrent of literature on the international-payments and monetary system which pours from the world's printing presses, this is an outstanding item, not alone for the distinction of its author, or his stamina in producing books on the world economy at long intervals (*Economic Stabilization in an Unbalanced World,* 1932, and *America's Role in the World Economy,* 1948), but for his contribution to the discussion.

Professor Hansen retains his independent cast of thought in international as in domestic matters. Though he enters the field seldom, he brings to it a fresh viewpoint and an integrity of critical judgment which will not allow him to follow in the fashionable channels. Not that he is unfamiliar with the literature. The book attests to an impressive examination of books, pamphlets, speeches, hearings, unpublished memoranda and includes a survey of the literature from Aliber to Zolotas, with such up-to-date features as critiques of the Hart-Kaldor-Tinbergen plan on commodities, Bernstein on the supplementary reserve unit to be set up at the I.M.F., and a previously unpublished Lerner plan for lowering the price of gold.

This is impressive. More so, however, are Hansen's own analysis and prescription. He does not believe that the world suffers from a lack of liquidity; the problem is one of lack of trust in the fiduciary element of international reserves. This he would correct first with gold guarantees. He would then adjust matters back to the position before the European central banks began to nibble the dollar-exchange standard to death by slow conversions of dollars into gold. This would be through Posthuma-like agreed ratios of gold to total reserves. Then, and then only if needed, would it be possible to create an International Reserve System, based on the Group of 10, which would have the function of creating reserves to order when called for.

Meanwhile, Hansen's analysis would correct misconceptions in Europe and the United States. He would persuade the Europeans that much of their present difficulty in borrowing long and lending short rose from their inadequate capital markets (pp. 122n., 156), and Americans that the Department of Commerce definition of equilibrium was an arbitrary and misleading one, under which the United States would probably be making the appropriate contribution to the world payments system by running a "deficit" of $1.5 billions a year (p. 208). He is not so bold as to embark on a positive program of attempting to demonetize gold, but he raises the question more than once (pp. 49n., 118, 211) whether it is rational for central banks which could hold earning assets, say at 3 per cent a year, to hold gold for 10 years and lose 35 per cent of its value. He is not impressed with Triffin's demonstration, made each year since 1958, that the world is on the brink of a monetary crisis, and assembled a chapter of rather potted history to show that the analogy between 1931 and the years from 1958 to 1965 is overdrawn. He refuses to tilt at the academic windmill of flexible exchange rates.

It is possible to take exception to certain aspects of the book. Why do we need both Myrdal and Harris to introduce Hansen to us, especially a Myrdal who goes off in pursuit of his hobbyhorse, aid to underdeveloped countries, which in Hansen's view is a separate issue? The last chapter on "History" and the Appendix on "Financial Problems of Underdeveloped Countries" have the earmarks of an attempt to add more material to get the book into a better size-price range for commercial purposes. More substantially, he does not explore the institutional difficulties with guarantees, and the question how to limit them so as to prevent their simple extension to all monetary claims on a country, foreign and domestic, which would make exchange-rate adjustment virtually impossible. And there are times when he thinks we need deficits to expand reserves, and others when reserves can be expanded by long-term borrowing (p. 55).

These are mild strictures. The main point is that Professor Hansen has turned once more to the international scene, and we are all the beneficiaries of his powerful capacity to penetrate to the root of the matter.

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The European Community and American Trade—A Study in Atlantic Economics and Policy. By RANDALL HINSHAW. New York: Frederick A. Praeger, 1964. Pp. xv, 188, $1.95.

Soon three years will have passed since President de Gaulle's historic press conference placed a roadblock in the path of movement toward the Kennedy Grand Design. A timely and incisive evaluation of the events leading to De Gaulle's decision and a thoughtful post-mortem are contained in this book by Randall Hinshaw.

Viewing events primarily from the perspective of U.S. trade policy and prospects, Hinshaw has written a study which should appeal both to the economist and the general reader. Within this well-balanced book are included the following: (1) a concise background summary of post-1945 European economic integration; (2) an evaluation of the problems involved in British membership in the Common Market together with a cogent discussion of the forces promoting Great Britain's application; (3) a presentation of the case for economic integration; (4) a comparative study of U.S., E.E.C., and E.F.T.A. tariff positions, with a forecast of America's trade prospects in light of European tariff developments; (5) a study of the effects European integration will have on the less developed countries; and (6) some suggestions for future U.S. foreign trade policy.

Of special interest to the economist is Chapter 5, "The Changing Pattern of Atlantic Tariffs." Here, five studies which attempt to measure the average tariff rates of the Common Market, the European Free Trade Area, and the United States are compared and evaluated. This chapter would make excellent assigned reading for the undergraduate because of the careful treatment given to the pitfalls implicit in such measurements, especially the problem of

weighting. Of benefit to the economist are Hinshaw's tables showing current tariff rates for the three regions on a commodity group basis, an individual commodity basis, and a frequency distribution of average duties of categories of manufactured goods.

A chapter called "Britain and the Common Market" contains, in my opinion, one of the best summaries of the problems of British entry into the E.E.C. available today. Here, examined in clear fashion, are British agricultural policy, the role of Commonwealth Preference, and the compromises which had been reached in the negotiations prior to their termination in January 1963.

A major strong point throughout the book is the clarity with which the author treats the complexities of trade creation and diversion, variable import levies, income elasticity of demand, and the economic implications of free trade.

In his concluding chapter Hinshaw maintains that a vigorous move toward free trade is of the utmost importance to the U.S. economy. In fact, the author judges this goal to be so important that he would advocate our abandonment of the principle of equal treatment if such a move is not supported by certain other major trading countries. His readiness to sacrifice MFN may seem rash to some, but nonetheless his alternatives to equal treatment warrant consideration.

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International Trade and Finance. By EDWARD MARCUS AND MILDRED RENDL MARCUS. New York: Pitman Publishing Corp., 1965. Pp. vii, 616. $8.75.

This is a textbook for a beginning course in international economics, and according to the authors it is "written with both liberal-arts and business-administration students in mind." The book is divided into four parts: "The Theory of International Trade," "International Financial Relations," "Trade Policy," and "Growth and Development."

The first section, "International Trade Theory," is the weakest part of the book. The heart of this section is contained in Chapter 3, where the authors present the theory of absolute advantage and comparative advantage, as well as international demand and supply relationships for a particular commodity and the concept of reciprocal demand. Numerical examples are used extensively, along with rather lengthy verbal explanations which make the reading somewhat tedious at times.

In the discussion of reciprocal demand the authors, using numerical examples, derive reciprocal demand curves that are based solely on the assumption of diminishing marginal productivity and ignore completely the community indifference curve, both of which are used in the derivation of the reciprocal demand curve as set forth by Meade. While this simplifies the exposition and makes the use of a numerical example much easier, it does leave

much to be desired. In the example used by the authors, England, after trade, produces 30 additional yards of cotton cloth and 10 fewer yards of linen. The 30 yards of cotton cloth are exchanged for 20 yards of linen. This means that England has the same amount of cotton cloth after trade and 10 additional yards of linen, or that England has used all of her gains from trade to purchase linen and that no additional cotton cloth was desired. Presenting reciprocal demand in this manner is really nothing more than comparative advantage with increasing costs, and we are still left without the influence of demand.

Also in Chapter 3 the authors discuss price discrimination in international trade when elements of monopoly are present, but fail to mention that the reason it may be profitable for a monopolist to discriminate is because of different elasticities of demand in the various markets. As a result this section is unnecessarily cumbersome. In addition, in Figures III and IV the domestic and foreign marginal revenue curves, as well as the combined marginal revenue curve, are all incorrectly drawn, given the demand curves depicted.

Part II, "International Financial Relations," which in terms of number of pages accounts for half of the book, is perhaps the strongest part. Particularly good is the chapter on the foreign exchange market and the discussion of the balance-of-payments statement as an analytical tool in Chapter 8. Unfortunately, certain weaknesses appear in this section as well, such as the statement on page 172, "The interest rate is thus determined by the level of investment and the demand for money." The supply of money apparently has no effect on interest rates. Also misleading is the statement that "The world foreign trade multiplier must thus always be zero." This is true only under very special circumstances. If part or all of the increased expenditures on imports are associated with decreased saving, then the world foreign trade multiplier is not zero and even in the special case the concept is not nonsense, as the authors state, for it tells us something about what happens in the individual countries.

The section on trade policy is quite short, and included here is a discussion of the tariffs as well as U.S. trade policy since World War II. The Trade Expansion Act of 1962 is described very briefly, and the provisions for aiding domestic industries and workers that are harmed by tariff cuts receive a one-sentence mention.

In relation to tariff reductions the authors discuss some of the internal problems resulting from tariff cuts and conclude with the statement "Imposing a tariff may be unwise, but removing it could be even worse." While the problems associated with tariff reductions are real and serious, there is considerable doubt as to the validity of the statement, particularly in an age of retraining programs and financial assistance to industries hurt by tariff cuts.

The last section, entitled "Growth and Development," deals with present-day problems in international economics such as: the problem of international liquidity, regional trade organizations, economic development, and international economic instability. In the final chapter, the authors make a plea for additional theoretical and empirical work in the field. At the same time they

fail to mention the work that has and is being done in such areas as: international liquidity, price and income elasticities, the relationship between interest rates and capital movements, etc., thus giving the student the impression that the entire field is one large completely unexplored territory.

In summary, the book is uneven in quality, the theory section is weak, the section on international finance is generally well done, with the remainder of the book somewhat in-between. As a result, the book would probably not lend itself too well to most beginning courses in international economics. On the other hand, for those who like to concentrate on the financial aspects of international economics, this text would serve well, and it is in this area that the authors have best succeeded in their goal of writing "with both liberal-arts and business-administration students in mind."

RICHARD REIMER

The College of Wooster

International Economics. By WALTER KRAUSE. Boston: Houghton Mifflin Co., 1965. Pp. x, 672. $8.75.

In the Preface, Professor Krause says his book is designed to serve the interest of a wide range of readers concerned with the workings of today's international economy. It is the author's plan to present a rather difficult range of theoretical and policy material in which a familiarity with the usual content of an introductory economics is the only prerequisite. In this reviewer's opinion, he succeeds in the selected assignment by producing a volume demonstrating high-quality workmanship throughout. Whether the reader is a tyro first encountering the complexities of the field or a seasoned professional desiring an overview of current thinking on the subject, perusing the book will be a rewarding intellectual experience. The logical sequence of discussion is nicely balanced with controversial issues examined in a candid and provocative fashion. In fact, it offers an outstanding example of how an experienced author can integrate economic theory with policy imperatives so that useful alternatives for action are effected—the result is (as it should be) policy-oriented counsel for serious-minded policy-makers, professionals and laymen.

The first third of the book, encompassing 13 of the 30 chapters, is appropriately entitled "The Fundamentals of International Economics" and systematically elaborates in three major sections the standard topics—e.g., basis for trade, international payments, quotas, state trading, etc. In addition to covering the basic factual and theoretical aspects of the field, helpful supplementary case studies are skillfully used to explain the operation of import quotas, exchange controls, and commodity agreements.

The bulk of "classical" trade theory evolved during a century characterized by relative stability in the international economy. The prevailing economic order, in part a result of *Pax Britannica* and the monetary gold standard, created an environment in which the alleged advantages of specialization and unrestricted global trade were unequivocally prescribed. Today, in contrast, the real world lacks unity of purpose. As Krause observes, "Nationalistic

measures are initiated by individual countries. And the world is divided into different groups of countries—with each group having a different outlook" (p. 256). Part II, "The International Scene: A World Disunited," carries out this theme in three chapters that describe the present socioeconomic and political situation which confronts the United States in Western Europe, the Underdeveloped Countries, and the Communist Bloc—each with differing aspirations and potential for international conflicts of interest. With a frame of reference involving basic data, economic theory, and geopolitical reality thus established, Krause then launches into the area where he is at his best—i.e., policy-oriented discussion that is both structurally sound and politically feasible.

Part III, "The Goal of International Cohesion: Problems and Policies," consists of 13 chapters divided into 7 sections—e.g., International Institutions, Trade and Investment, Development and Aid, Regionalism, etc. The post-War II roles of the International Monetary Fund, the World Bank, and other supporting financial institutions are examined in an illuminating commentary that highlights both valid criticism of their activities and acknowledged achievements. Next, U.S. commercial policies are evaluated in an historical context which concentrates on the Reciprocal Trade Agreements Program (1934-62) and the Trade Expansion Act of 1962. The next chapter deals with possible vehicles for foreign investment, relevant historical magnitudes, distribution of capital flow, investment promotion efforts, and debtor-creditor relationships in terms of alleged benefits or disadvantages to the respective parties.

The four chapters on economic development and foreign aid are exceptionally well done. The section begins with an elaboration of alleged obstacles to development, proceeds through planning requirements to a lucid exposition of "The Prebisch Proposals" and the associated argument for restructuring global tariffs, while simultaneously promoting an international "retransfer" of income. Foreign Aid (i.e., our Mutual Security Program operation) is then appraised in terms of history, rationale, basic organization, magnitude, and distribution. In sum, Krause believes that "after a decade [and more?] of operations, the program could be adjudged a success in terms of halting the spread of communism. In terms of the promotion of development, however, the program appeared to measure up less well" (p. 565). Given the dual mandate of halting communism now and/or promoting development in the "poor" countries, the former with its priority political-military overtones has been our choice for emphasis to date. Also, the foreign aid program has been excessively fragmented and lacking in coordination if genuine results are desired. Interagency rivalry and abdication of responsibility is common when inherently conflicting goals are apparent—e.g., promotion of U.S. exports, containment of communism, provision of development assistance, etc. Krause believes (the Department of State, notwithstanding) that the most practical way to insure necessary coordination is to have an "autonomous agency" or ideally a Department of Development (p. 568). In view of his AID experience, this reviewer would subscribe to such a coordination arrangement with

unmistakable responsibility, but frankly doubts that reality in Washington would permit the implied "independence"—even though we, as taxpayers, might see substantially more economic development in some aid-receiving countries.

Following a stimulating discussion of various facets of the current U.S. balance-of-payments problem, international monetary reform proposals, and the implications of the major regional integration movements, an instructive chapter is devoted to the present course of U.S. foreign economic policy and the basic alternatives we face. In Part IV, "The Frontiers of International Economics," the author suggests important gaps in the literature that warrant increased attention by professionals. Several Ph.D. dissertation topics are suggested in the areas of comparative advantage, underemployment in export-dependent economies, state trading, and policy problems related to conflicts of national interest. Interestingly, one implicit "message" of this book seems to be that the policy action one recommends depends upon what he assumes to be the most valid underlying body of theory—clearly not all is known and a tremendous knowledge frontier in international economics remains to be explored.

Any commentary dealing with policy-oriented subject matter will necessarily invite controversy—this book will doubtless generate disagreement as prejudices are re-examined. It will be incumbent upon critics, however, to argue with comparable sophistication. Among other things, one could quibble about the author's reluctance to take a firm stand on certain issues after indicating the major pro and con arguments. Also, his occasional propensity to talk about the positions held by "some people" without giving specific citations will trouble some—particularly nonspecialist—readers. Except for the omission of a listing of tables and figures in the Contents, the publisher has produced an exceedingly attractive volume. These minor criticisms should in no way detract from the book's over-all merit. Its literary quality, substantive content, and appealing format should combine to make what will undoubtedly become a popular text. Each topic is followed with a selected bibliography that will permit the teacher (or general reader) to extend or modify the analysis as desired.

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Industrial Organization; Government and Business; Industry Studies

Transport Investment and Economic Development. Edited by Gary Fromm. Washington, D.C.: The Brookings Institution, 1965. Pp. 314. $6.75.

Transport investment has probably been the single most important international development instrument during the postwar period. By June 1963 the major international and U.S. donor agencies had pumped $7.2 billion into various rail, road, air, water, and pipeline transport projects around the world. In addition, the developing countries themselves have made major

efforts: allocation to the transport sector may range from 18 (Pakistan) to 47 per cent (Nigeria) of total public expenditures. There was once a naïve belief in the invariably beneficial effects of transport improvements, nourished by overoptimistic, analytically unsound field studies. Further, as Albert O. Hirschman has pointed out, transport ventures are notoriously difficult to prove wrong, either before they are started or during operation. But it is now fairly well established that although numerous projects have been outstanding successes, others have been obvious and costly failures.

Given the magnitude of transport resource allocations, it is clearly of the utmost importance to improve professional advice and sharpen decision-making. This is not at all easy, for theoretical and practical knowledge is still rather scanty. Commendably, the U.S. Agency for International Development some time ago sponsored a major research program at The Brookings Institution on the role of transport in the development process. Following Wilfred Owen's *Strategy for Mobility,* this second publication to result from the program makes valuable contributions on some of the key analytical issues. Further Brookings studies are under way.

The volume consists of 12 seminar papers given at Harvard University. A selected bibliography with some 500 items, compiled by Katherine D. Warden, is appended. The book as a whole displays some of the usual disadvantages of multiple authorship, but Fromm provides a good framework and important discussion links in an introductory chapter, in another entitled "Design of the Transport Sector," and in a third one on financing of transport investments, written jointly with A. Robert Sadove of the World Bank. Fromm purposes to accommodate transport analysis within a six-stage iterative planning process. Establishment of national objectives would come first, then the design of long-range (thirty to forty years) and short-range (four to five years) regional and sectoral plans encompassing the entire economy, next benefit-cost evaluation of individual projects, followed by financial studies, removal of inconsistencies, and finally submission of well-documented alternative plans to the political decision-makers. This sort of sophisticated, comprehensive planning is, of course, devilishly difficult to carry out in practice, but Fromm succeeds in outlining the more promising areas for future methodological advances.

Quite refreshing are the observations of Hans Heymann, of RAND, on the objectives of transportation. He points out that the selection of a national transportation system implies choice among various conflicting and incommensurable objectives. "Such a choice," he states, "cannot be approached as a straightforward economic efficiency problem; it constitutes a high-level policy decision." Pursuit of the admittedly elegant theoretical optimum is futile, "utility maps exist only in the textbooks," and as soon as noneconomic objectives are introduced, then "the economist's prescriptions based on market values break down." But the economist can nevertheless contribute to good decisions by offering "feasible transportation alternatives together with a prognosis of some of their foreseeable social implications." Heymann justifiably chides transport economists and planners, both in the market-oriented West and in the centrally planned East, for "less than inspired" past performance.

Two interesting essays by Richard B. Heflebower, of Northwestern University, and Wilfred Owen, who directs the Brookings research program, deal with the technical and cost characteristics of transport modes. Many alternative choices exist, not only among the different media (road vs. rail, etc.) and subsidiary design variations (various road-pavement types, pipeline diameters, and so on), but there are also less conventional substitutions, such as electrification for coal transport and telecommunications for passenger transport. One may add that field analysts have so far rarely taken full advantage of these technological possibilities, nor are decision-makers perhaps fully aware of them. There is no reason why the transport economist should not be able to comprehend them, since his whole professional emphasis is on comparisons among meaningful alternatives. But his necessary counterpart, the fully fledged, broadly oriented transportation and communications engineer, is a rare bird indeed. If this field is to make any advances at all, he will have to be turned out by the universities of the future, rather than the usual dyed-in-the-wool highway or railway specialists.

Louis Lefeber, of MIT, tackles the important problem of "Economic Development and Regional Growth." He offers some models which are computable for many sectors and with nonlinear production functions and then yield patterns of efficient price relationships, the violation of which may retard the growth of national income and employment. Inevitably, there are a number of restrictive assumptions, but his exposition provides many interesting insights. Mitchell Harwitz, of the New York State University at Buffalo, addresses himself to the related theme of deliberate interregional income equalization policies. He develops static general equilibrium models for many regions, and for one as well as for many commodities, which could be employed to evaluate programs of regional economic development. While there are substantial data-acquisition requirements, he suggests that these are not so burdensome as to invalidate his approach to development-planning.

The practitioner will benefit from reading the chapter "Economic Evaluation of Transport Projects" by Hans A. Adler, transportation economist with the World Bank. He points to the sad truth that some transport investments stimulate economic development, and some simply do not. Consequently, each project must be investigated individually and no helpful generalizations appear possible for the time being. He sketches the evaluation process, from benefit and cost measurement right to the calculation of the internal rate of return and other success criteria. Adler seems rather sanguine about employing "monetary terms as the only practical denominator" and observes that "they can be made substantially more useful by the use of 'shadow prices' to reflect real economic costs and benefits more closely." Not everyone, including some of the other authors, would fully share his optimism, which is perhaps conditioned by the Bank's general orientation. The stimulating Sadove and Fromm essay shows that the conclusions of economic and financial analyses can conflict. Investment priorities should not be based solely on one or the other. Instead, "a combined approach should be utilized." James R. Nelson, of Amherst College, sets himself the difficult, if not impossible, task of reconciling classical marginal-cost-pricing theory with the dynamic, multi-

dimensional problems which contemporary development programs pose. Finally, two well-written, empirically oriented essays, "Transportation in Soviet Development" by Holland Hunter, of Haverford College, and "The 'Railroad Decision' in Chile" by Robert T. Brown, of the University of Chile and the Brookings Institution, provide much grist for the methodological mill and good food for thought.

It is hard to sum up these diverse contributions. The volume as a whole certainly succeeds in expanding intellectual horizons in this field. That men with great practical experience, as well as scholars, found time to record their thoughts is admirable, for under the great pressure of work their insights are unfortunately often lost. At the same time, the essays do reveal significant gaps in knowledge and certain incongruities in existing theory and practice of development project appraisal. This is not a criticism of the authors. They perform a most valuable service by reminding us of the many puzzling problems that still have to be solved. In my view, these exist largely because we do not yet have a general theory of social and economic development which stands up to the exacting requirements of the world we live in. In Heymann's words, there is no getting away from "the painful fact that a large, complex society invariably embraces a multiplicity of conflicting objectives." As we progress towards a satisfactory development theory, the important transport sector's place in the general scheme of things can also be more clearly established.

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Development of a Population of Industrial Firms—The Structure of Manufacturing Industries in Norway 1930-1948. By FROYSTEIN WEDERVANG. Bergen, Norway: Universitetsforlaget, 1965. Pp. 275. N. kr. 42.00.

This volume reports a detailed statistical study of Norwegian manufacturing plants and firms for the years 1930 and 1948, with some additional comparisons for 1933 and 1937 to examine the effects of recession and recovery. The data were obtained from special tabulations by the Norwegian Central Bureau of Statistics and cover 13 industries, 502 sample products, and five principal product groupings. Although the data were processed in 1950-51, this volume was not prepared for more than a decade; thus, it is neither a current report on Norwegian industry nor an explicit extension of recent contributions to the analysis of industry and enterprise structure. The author has, however, reviewed this later literature, and he draws relevant parallels and contrasts with his own results from time to time.

The characteristics of plants and firms selected for this analysis are labor (persons engaged) and capital (insured value plus uninsured constructions) inputs, value added, and product composition. The author describes the size distributions of plants and firms revealed by the data, and examines the evidence of scale economies, integration, concentration, entry-exit, differential

growth rates, etc. Both changes over time and change-generating forces are discussed, but there is no systematic attempt to explain the former in terms of the latter. Indeed, each section of the report is almost a separate essay, beginning with a general presentation of the topic, followed by a detailed analysis of the data, and sometimes concluding with a review of relevant findings from other studies.

In the course of the analysis, a number of interesting results are obtained, most of them indicating the complexity of the subject, as revealed by numerous cross-classifications. The familiar skew distributions of plant and firm sizes are observed throughout the data, but they do not appear to correspond precisely to either the Pareto or log-normal models. Evidence of scale economies, although found in the average data for size classes, is sharply qualified by (1) the higher capital/labor ratios of the larger establishments and (2) the wide dispersion of capital and labor productivity within each industry and size class. In general, the concentration of production in the largest firms is found to have decreased over the period, particularly in those industries and products experiencing considerable expansion in aggregate output. The entry of new firms is shown to provide an important offset to the tendency of existing large firms to become relatively larger over time. The author concludes that variations in product structure among firms provide an important explanation for the existence and stability of the dispersion of firm size, growth rate, and productivity; in effect, the large and small firms are doing different things, perhaps with equal efficiency and success.

The most original contribution of the volume is the analysis of entry and exit in Chapter 10. The author presents a kind of Phillips-curve for the business population, with birth and death rates of firms stated as functions of the (exogenous) rate of industry growth, among other variables. Unfortunately, he does not elaborate the full set of functional relations involved nor subject his partial model to empirical test.

In spite of the limited interest attached to the data as such, this volume commands attention because of the author's analytical skill and eye for the relevant details and implications. As a result of the author's choice of "an 'open search,' which usually implies one or several preliminary hypotheses, . . . [rather] than testing a single theory," the over-all impression left by the work is one of fragmentation and *ad hoc* analysis. However, the literature does not, in fact, offer a "single theory" in which the many variables considered in this study are operationally related. The absence of a comprehensive schema—relating industry and product composition; plant, firm, and market size; capital and labor productivity; and rates of change over time—from this volume constitutes a challenge to the profession, and the suggestions and tabulations presented here should provide at least some preliminary working materials.

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Land Economics; Agricultural Economics; Economic Geography; Housing

Economics and Public Policy in Water Resource Development. Edited by STEPHEN C. SMITH AND EMERY N. CASTLE. Ames: Iowa State University Press, 1964. Pp. viii, 463. $4.95.

Stephen Smith and Emery Castle have assembled 23 articles dealing with public water policy, which is "the resolution of conflicts over water resources use" (p. 2). Contributors are economists, engineers, political scientists, attorneys, and administrators because "No one academic discipline has within it constructs which can handle all of the policy issues" (p. 2). Six of the articles were written on request to round out the conflicts, the remainder appeared mainly in the late 1950's.

Upon scanning the table of contents, two questions came to mind. Why were these particular articles selected? And why were the articles arranged and ranked in the order presented? We learn that the Committee on the Economics of Water Resources Development was in large measure responsible for the papers and their selection. Inquiry disclosed that the Committee, otherwise unidentified, is a standing committee of the Western Agricultural Economics Research Council whose members come from the Western Land Grant Colleges and Universities.

The Committee has published 13 annual proceedings. Knowing the Committee's background explains in part why (a) the time perspective of the volume, (b) the book is essentially an anthology of Committee publications, and (c) the articles are concerned mainly with the problems of distributing or controlling surface fresh water running downhill, largely in the agricultural West, and more particularly in California.

The second question pertains to the internal organization of the materials. The editors arranged the articles around three ideas—economic evaluation, financing, and organization—and presented them as chapters in five parts. These ideas are not discrete or exhaustive; other organizing ideas might have been more appropriate.

Part I, "Economic Evaluation Concepts," is essentially a review of benefit-cost refinements, which M.M. Kelso tells us in "Economic Analysis in the Allocation of the Federal Budget to Resource Development" commenced in earnest with the federal government's publication of the "Green Book" in 1950. In just 15 years benefit-cost analysis has spread far afield. Kenneth Boulding, in an original essay "The Economist and the Engineer: Economic Dynamics of Water Resource Development," explores the economist's and engineer's conflicts in thinking, offers engineers more than cost-benefit as an analytical tool, suggests that water is underpriced and an excellent commodity for tax, quotes two of his own dry water verses, and lightens the reading with such barbed wit as "Los Angeles is going to run out of air long before it is going to run out of water" and "In Los Angeles water is not a commodity but a religion" (p. 88). A rejoinder was expected from the article following, written by

Engineering Professor R. K. Linsley, "Engineering and Economics in Project Planning." It turned out to be a short treatise in which the author discusses some uncertainties, chiefly secular climatic trends, faced by engineers in scaling the size of water projects.

Part II, "Economic Evaluation, Quantification," presents four articles demonstrating the use of cost-benefit analysis in irrigation and flood control. This section is probably of least interest to economists. Too much of the emphasis is on "small solutions to small problems."

Part III, "Financial Responsibility," contains three articles, none of which was written especially for this volume. The emphasis of all three is essentially the same: the sharing of project costs among political or administrative units involved in the wholesaling of a particular water supply. Much of the discussion pertains to California and its costly jurisdictional warfare among numerous water agencies, each attempting to obtain the most water for its consumers at the lowest market price and at the expense of all taxpayers.

Economists will likely find the four reprinted articles in Part IV, "Economics and Water Law," of most interest. Here we find the crux of the water policy problem: the historical evolution and rationalization of conflicting property rights, especially between rural and urban users, to surface and underground fresh waters. Two trends stand out: (1) the trend in organizational control of water from the early private riparian rights and *laissez faire* appropriation of surplus water to the ascendant supremacy of federal over both private and local government water rights and uses; and (2) the trend in judicial decisions giving increasing support to public rights over private rights, although prior users continue to receive judicial protection.

The five papers in Part V, "Organization," of which two are original, could easily have been rearranged and integrated with Part IV. The most pragmatic and yet profound essay is the concluding one by Attorneys John G. Laylin and Brice M. Clagett, "The Allocation of Water on International Streams." The authors recite their experiences in helping or attempting to settle international water disputes. Their well-documented and lengthy footnotes constitute an excellent and annotated bibliography on the historical development, starting with an early Buddhist legend, of international law.

Political scientist Hubert Marshall presents his "Rational Choice in Water Resources Planning" in two parts: how decisions ought to be made (economic rationalization) and how they are actually made (political) (pp. 402-4). Professional economic analysis lacks incisiveness because a water-organization economist is expected to be, above all, a loyal organization man. The number of competing single-purpose water organizations is legion. Marshall's solutions are twofold: (1) strengthen the central political party organizations so that local interests are subordinated to national interests; and (2) counterbalance the organizational loyalty with professional loyalty (pp. 416-17). Is this all that is necessary to give an economist greater voice in the decision-making process? Boulding is not as hopeful because an economist is but one of several professionals concerned with water as an environmental or transporting substance.

The built-in limitations of this volume precluded the examination of two highly relevant and fitting concerns for which space could have been profitably substituted. The first pertains to the costs of providing usable water from alternative sources of supply. Reportedly, 97 per cent of all water in the world is contained in the oceans, and of the fresh water, 75 per cent is contained in polar ice and glaciers (*Water Resources*, A Report to the Committee on Natural Resources, National Academy of Sciences, National Research Council, Publication 1000-B, 1962, p. 4). The second major deficiency is the lack of a more thorough treatment of water pricing, especially at the consumer or user level. Only one paper was devoted to public water pricing, and it paid too little attention to the marginal productivity of water as a demand and price determinant. Most economists, not organizationally muted, agree that water is generally underpriced, which leads to waste, to misallocation, and to the overbuilding of water projects. Interested economists might consider joining Professor Boulding's anvil chorus. He proposes that "If he [the economist] cries loudly and long enough, . . . somebody may listen to him out of sheer irritation" (p. 92).

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The Economics of Housing. By LIONEL NEEDLEMAN. London: Staples Press, 1965. Pp. 232. 42s.

Though this book is keyed to the British scene, it presents a solid grounding in the economics of housing which has almost equal application in the U.S. housing market. While it treats British housing policy only incidentally, it does provide an understanding, well illustrated with fact, of the economics of housing which is essential to a critical assessment of governmental intervention. The economics is not at a sophisticated level, but is a straightforward explanation of economic relationships within the classical market model under strong institutional restraints. The author says that "the book assumes no previous knowledge in the reader of economics or any other specific discipline."

The discussion begins with a consideration of housing needs and methods of measuring future housing needs both quantitatively and qualitatively. Then, after a review of general demand analysis, techniques and problems in the estimating of housing demand are presented. Two chapters are devoted to the supply of housing, particularly to the organization of the housebuilding industry and to proposals for raising the productivity of the British housebuilding industry. Chapter 6 compares the financial aspects of home ownership and tenancy, the methods of financing private housing, and the financing of publicly owned housing in Britain. A chapter on the local housing market considers the market function of price determination and the characteristics of market instability that lead to housing cycles.

The last three chapters of the book describe certain pervasive housing problems and the forms of governmental intervention which have been

applied to their solutions: the matter of housing of low-income groups and the subsidy devices used; the chronic market imbalance which produces overcrowding and underoccupation and the obstacles to more effective utilization of the standing stock of accommodations; and the origins of slums, the economic obstacles to private clearance and replacement, and the scope of governmental action in this field.

In spite of historic and institutional differences, the British and U.S. housing markets are sufficiently similar so that this book can be recommended to scholars and government officials concerned with our housing problems. Some of the similarities and contrasts are interesting. The proportion of home ownership in Britain is substantially lower than in the United States, and the share of the rental housing which is provided by government is very much higher. Rent control is still in effect over a large share of the housing stock there and, in general, governmental controls in the housing market are more extensive. The home-building industries in the two countries are organized in much the same form, and both are accused of the same inefficiencies. The pattern of private mortgage lending is similar, with savings and loan associations prominent here and dominant there. An interesting contrast is the use of the variable interest mortgage in Britain, with rates on existing mortgages adjusted to conform to changes in the general level of interest rates.

In so short a book, it is not to be expected that the author present a comprehensive treatment of the subject. The aspects of housing economics which were selected for discussion are problem-oriented, useful in understanding the origins of the problems and in evaluating attempts at solution. The discussion is effectively implemented with comprehensive British housing statistics.

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Labor Economics

Labor Productivity. Edited by JOHN T. DUNLOP AND VASILII P. DIATCHENKO. New York: McGraw-Hill Book Company, 1964. Pp. xxv, 409. $7.95.

This volume consists of 30 papers presented at the Conference on Labor Productivity which met in September, 1961, at Lake Como under the auspices of the International Economic Association. The papers are almost equally distributed under the four major headings: (1) concepts and measurement of productivity, (2) international comparisons of productivity, (3) wages and productivity, and (4) technical, managerial, and organizational factors affecting productivity. The volume begins with a short report by the Program Committee, setting forth a number of methodological points on which the participants apparently agreed, and an agenda for future discussion.

With regard to concept and measures, the representative of the U.S. Department of Labor, Leon Greenberg, confined himself to discussion of the conventional "output per man-hour" measures, noting that the Department is trying to expand the denominator to include "nonproduction" worker man-

hours as well as the hours of production and of related workers which have declined in relation to the total. In passing, he stressed the difficulties of measuring capital inputs, and raised the conceptual issue of "whether capital stock or capital used should be used for the denominator" (p. 53).

Participants from the Eastern countries generally had no doubt but what capital used up in the production process should be included as inputs in the form of man-hours of the embodied, or "stored-up," labor consumed in the gross production process. The Eastern economists would also, for comparison with gross output measures, include the current and embodied man-hours represented by intermediate product consumption, through use of input-output matrices.

Solomon Fabricant, in the most sophisticated of the papers on measurement, pointed out (p. 24): "It is the value of the services of capital that constitutes capital input. This value includes more than capital consumption, and is not necessarily related to capital consumption in any close way." The National Bureau of Economic Research has, of course, prepared capital as well as labor input estimates and has related total tangible factor inputs to net output as a means of showing net economies achieved through time in usage of these inputs.

Regardless of their views on capital-input measurement, all participants agreed it is useful to compute a system of various "labor productivity" measures to show the changes in specific types of labor input per unit of output through time. Such changes result, of course, from interfactor substitutions as well as from technological and organizational changes.

The participants emphasized the difficulties of international productivity comparisons, given the differences in scope and definition of both outputs and inputs, particularly on an industry basis, and the problems in adjusting for quality differentials. For example, Soviet economists estimate labor productivity in the Soviet Union at anywhere from 40 to 60 per cent of that in the United States, on average, while estimates by U.S. economists generally yield a considerably lower ratio. Gertrude Schroeder of the U.S. Census Bureau, after adjusting 1956 data for comparability as well as she could (given inadequate technical descriptions in the Soviet literature), concluded: "Average output per production worker in the 25 branches in the USSR is shown to be 28 percent of United States output using Soviet weights and 31 percent using United States weights; the unweighted average is 32 percent" (p. 119).

Other interesting international comparisons are presented in Part II. Angus Maddison compares levels and movements of labor productivity in Western Europe, relating these to relative rates of investment (defined rather too narrowly); A. Nove compares productivity in the United Kingdom and Soviet Union; and Erik Ruist and Aubrey Silverston make intercountry comparison of productivity in the steel and automobile industries, respectively.

Yet, as Joel Dean points out, labor productivity measures are of limited usefulness in guiding management decisions. Not only should all physical input-output ratios be considered, but also relative prices which influence least-cost combinations and prospective profitability. "Productivity analysis can provide a rough indication of a company's international competitive posi-

tion and can be a springboard for more specific analysis of where to buy or manufacture products. In the final analysis, however, it is not labor productivity but rather capital productivity (profitability) which is the management guide to greater economic efficiency. The concept of physical labor productivity is essentially technocratic rather than economic" (p. 184).

The discussion of wages and productivity revealed the importance of relative levels and trends of wage rates in both Western and Eastern countries as a means of motivating and rewarding work and allocating labor. One learns from E. I. Kaputkin that individual piecework is declining in relative importance in the Soviet Union, while technological developments promote collective piece-rates and bonuses based on cost-savings. He states that between 1950 and 1959 output per man-hour in the Soviet Union rose 86 per cent, while real wages rose 62 per cent (which implies that the labor share of national income fell, in contrast to a rising share in the United States!). He wrote that the USSR plans for a 4 to 4½ per cent a year increase in labor productivity 1960-80 (which seems optimistic in view of performance thus far).

The final section contains a rather general discussion of the relationship to productivity advance of factors such as the quantity and quality of capital, including the trend towards automation, energy consumption, worker training and education, organizational structures, and management techniques. Of particular interest is the essay by Charles A. Myers, who contrasts "management by control" with "management by objectives." He quotes Douglas McGregor regarding the latter method (p. 346): "The essential task of management is to arrange organizational conditions and methods of operation so that people can achieve their own goals best by directing their own efforts towards organizational objectives. This is a process primarily of creating opportunities, releasing potential, removing obstacles, encouraging growth, providing guidance." One obtains the impression from the papers that this philosophy has been carried further in Western than in Communist countries.

More generally, while the representatives of the Communist countries were keenly aware of the importance of increasing productive efficiency, a Western reviewer of the papers cannot escape the impression that they view the problem in engineering terms, and are less concerned with "economic efficiency." As Fabricant pointed out (p. 16), "While productivity is simply the ratio of output to input, neither output nor input is a simple physical quantity independent of what men consider valuable." One wonders to what degree predominantly nonmarket economies allocate resources in conformity with men's wants. The question "productivity for what?" was not discussed, but there can be little doubt that the degree of economic efficiency of a society is one of the important factors affecting its rate of productivity advance.

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Proizvoditel'most' truda v SSSR i glavnykh kapitalisticheskikh stranakh. (Labor Productivity in the USSR and Major Capitalist Countries.) By ADOLF I. KATS. Moscow: Izdatel'stvo Ekonomika, 1964. Pp. 247. 84k.

Surpassing the U.S. level of industrial output has been a recurrent policy goal of the Soviet leadership in the postwar period, though confidence in such an attainment has been muted of late. The key to success in this aim is to overcome the U.S. superiority in labor productivity, as well as that of the major Western European economies. Heretofore, official Soviet estimates of the ratio of Soviet to U.S. industrial labor productivity has been presented in highly aggregative terms without methodological explanation, except for selective comparisons by the author in an earlier publication. In this volume the senior economist of the Labor Scientific Research Institute of the State Committee on Labor and Wages presents the first detailed comparison of Soviet and U.S. labor productivity in selective sectors, accompanied by appropriate methodological explanation.

The essay initially defines, in tortuous fashion so as to preserve Marxist purity, the meaning of final product. To the traditional Marxist definition of national income Adolf Kats adds depreciation charges on capital used for commodity production. His definition differs from the conventional Western concept of gross national product by the exclusion of most services. This definition in hand, Kats then compares final product per employee in the U.S. and Soviet economies. He then derives estimates of comparative labor productivity in 27 industrial sectors in terms of standard physical output units. His aggregate industrial comparison is obtained by weighting the separate sector ratios by employment and wage bill values. He subsequently compares industrial output trends in the two economies and offers some perceptive hypotheses for the lower level of productivity in Soviet industry. In his final chapter he introduces some limited industrial and final product productivity comparisons between the USSR and the United Kingdom, the Federal Republic of Germany, and France.

While Kats provides considerably more methodological explanation than previous Soviet comparisons of U.S. and Soviet productivity and rather exhaustively documents his sources and calculations, his biases in method and selective interpretation of his calculations are obvious to the reader with an elementary knowledge of statistical method and economic institutions in both countries. In his comparison of Soviet and U.S. final product, both in the aggregate and per employee, the omission of most services reduces U.S. output by twice the proportion of the USSR equivalent. In valuing the two economies' outputs Kats uses only the dollar price comparisons, rejecting the ruble valuation because ruble prices "understate the value of capital goods production in the USSR." Western scholars of the Soviet economy have long recognized this deficiency. Anyone familiar with international comparisons would quickly recognize that such a procedure would overstate the ratio of the Soviet to U.S. output. Kats's ratio of 60 per cent for 1959 compares with one

of around 45 per cent for 1960 computed by this reviewer in a contribution to the Joint Economic Committee of Congress. This alternative ratio was obtained from a geometric mean of dollar and ruble valuations and estimated each country's GNP by conventional Western procedures.

The comparison of Soviet and U.S. industrial labor productivity is based on a weighted sample of 27 sectors covering some two-fifths of total output. Only one machinery sector, machine tools, is included, along with 26 raw material and consumer nondurables sectors. The sample is structured in favor of the USSR by including the machinery sector in which the Soviet output ratio is highest relative to that of the United States and material and consumer-goods sectors represented by homogeneous unit values. No adjustment is made for higher quality of U.S. consumer goods or the more sophisticated composition of raw materials output. The latest pair of years in his time series matches 1958 U.S. output with 1959 Soviet production. The secular underestimate of U.S. output arising from selection of a year at a cyclical trough reverses the trends in the ratios to the detriment of the United States for 12 sectors, as compared with trends through 1957. Kats's aggregate ratio of Soviet and U.S. industrial production per employee for 1959 is 53 per cent, compared with approximately a third computed by Gertrude Schroeder for 1960 in a contribution prepared for the Joint Economic Committee.

The author perceptively analyzes factors which explain the disparity of Soviet and U.S. productivity levels. Through higher degrees of product specialization U.S. industry benefits more from economies of scale. Soviet investment has been far less balanced with concentration on direct production operations and neglect of essential supporting activities, such as materials-handling, repairs, heating, and lighting. The official policy of allocating investment resources so as to maximize the rate of industrial growth irrespective of efficiency considerations is also reflected in the wide range of plant productivities within industrial sectors. The newest units match their U.S. counterparts in worker productivity, while the older, high-cost plants bear a much lower ratio to the most efficient than would prevail in an economy in which marginal cost governed production decisions.

Kats's comparisons of Soviet final product and industrial productivity with that of the United Kingdom, Germany, and France are based on less adequate samples than his U.S. comparison, but he employs the same technique with the same favorable conclusions. He computes final product per employee to be higher in the USSR than in either the United Kingdom or Germany in 1962, whereas estimates by this reviewer for the same year show Soviet per capita GNP to be a third lower. Similar overstatements are made with respect to Soviet industrial productivity superiority.

While the volume does employ procedural biases which yield statistical conclusions consistent with official claims, it represents gratifying progress in the careful methodological presentation of a Soviet economic position. In addition to enabling a Western reader to understand better the nature of Soviet statistics, the Kats essay also provides quantitative information not previous-

ly available. It is hoped that he will be emulated by other Soviet economists in his closer adherence to rigorous standards of scholarship.

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Wages, Productivity, and Industrialization in Puerto Rico. By LLOYD G. REYNOLDS AND PETER GREGORY with the assistance of LUZ M. TORRUELLAS. Homewood, Ill.: Richard D. Irwin, Inc., 1965. Pp. 350. $8.50.

Research for this book, which consists of three related essays, was conducted over a ten-year period, although work on the study was not continuous. The first essay, which relies upon published data, gives an overview of Puerto Rican economic development since 1945; the second and third are based on interviews with island managers and a large sample of workers conducted in 1944-45. There are "cross-linkages" among the parts, but the authors indicate that each essay "stands on its own feet." The study is heavily documented. It includes 85 statistical tables, and 60 supplementary tables are available from the authors.

Puerto Rico's record of development has been impressive. "Real GNP tripled between 1940 and 1960, and GNP per capita increased by 137.5 percent." Presumably what is meant here is GRP—gross regional product—since the island is an integral part of the national economy. Significantly, "the rise in output resulted almost entirely from an increase in output per worker." Employment has declined slightly since 1950, and changes in capital-labor ratios have been modest.

Part II deals with the management of new industries. The authors do not give many high grades in their evaluation of island managers. They speak of "horseback judgements" and a lack of quantitative information. They also point out that many managers—including some native Puerto Ricans—held naïve views about worker-motivation and aspirations at the time of their survey. Among other things, these managers assumed a backward-bending labor supply curve. But the authors found little or no support for the hypothesis that workers in Puerto Rico just want to get by; that after they have earned enough for their rice and beans they prefer leisure to higher earnings. They found, on the contrary, that island workers respond to economic incentives. There undoubtedly have been changes in management attitudes and practices (especially those relating to manpower management) since the survey, given the gains in productivity and accelerated development of the following decade.

The study sheds new light on the economic impact of rising minimum wages. When the Fair Labor Standards Act of 1938 was passed exceptions were made for Puerto Rican industries with separate minima for each. These minima have been pushed up rapidly, however, and the gap between them and the mainland minimum has narrowed. This forced Puerto Rican managers to tighten labor standards in their largely successful efforts to protect profit margins. Workers responded to rising minima—which are closer to prevailing wage levels than on the mainland—by increasing their output. Turnover and

absenteeism also declined as wages went up. While exogenous forces have no doubt contributed to rising productivity in Puerto Rico, the authors assign considerable weight to "induced increases resulting from wage pressures."

The concluding essay discusses the problems involved in assembling an industrial labor force in an underdeveloped region. During the early phases of industrilization the influence of island culture on work habits was strong. This influence appears to have waned as the work force became more disciplined. The Puerto Rican labor market is similar in many respects to labor markets on the mainland. The primary sources of job information are friends and relatives. There is considerable interfirm movement in response to economic incentives, and mobility patterns resemble those of large and rapidly growing labor markets on the mainland. One problem is that the demand for labor in the expanding industries of Puerto Rico has been primarily for females, but the workers being displaced from agriculture are predominantly male. Some of the latter migrate, but others remain and contribute to the island's continuing high rate of unemployment.

Migration has provided an outlet for population pressures in Puerto Rico, and there has been some fear that this was stripping the island of the cream of its work force. The authors found this was not the case. Workers with good jobs and satisfactory earnings are less inclined to migrate than those who have been unable to find suitable employment. There is also a significant backflow of Puerto Rican workers from the mainland to the island. The authors feel this is beneficial if, during their sojourn on the mainland, these workers acquire skills and work habits which will help them find and keep factory jobs in Puerto Rico.

There is little to criticize in this excellent volume, although there are a few lapses from a generally lucid presentation. On page 35, for example, the authors speak of "the suction of manpower out of agriculture." Two pages later they refer to "labor being extruded from the agricultural sector." The latter phrase seems more descriptive of what has been going on in Puerto Rico. While it is evident from the title and the text, the authors do not emphasize the regional character of their study. They do not, however, suggest that their findings apply without modification to other underdeveloped areas, either national or regional. Because of its breadth of coverage this book should interest a wide audience including labor economists, regional economists, regional scientists, and those in economics and other disciplines who are interested in the process of development.

William H. Miernyk

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The Quality of Labour and Economic Development in Certain Countries: A Preliminary Study. By Walter Galenson and Graham Pyatt. Geneva: International Labour Office, 1964. Pp. vi, 116. Paper, $1.50.

Until recently economists have tended to stress the importance of investment in physical capital to the exclusion of investment in human resources as determining the rate of economic growth of a country. This view has been

particularly evident in the development plans of underdeveloped countries. Investment in human resources has been regarded more as a consumption than as a productive expenditure. There is now a large and growing literature which is primarily concerned with investment in human resources as a source of economic growth. Professor T.W. Schultz, for example, speaks of labor as the principal resource and estimates that improvement in the labor force, through expenditures on education, contributed at least 21 per cent of the increase in national income in the United States between 1929 and 1957.

Most of the studies to date have been attempts to measure the impact of educational expenditure on economic development and to assess the implications in terms of educational planning in underdeveloped countries. As a case in point, several studies and reports of this sort have been sponsored and published in recent years by the Organization for Economic Co-operation and Development.

The publication presently under review extends the area of study to include in addition to education other components of labor quality—health, housing, and social security. While the tentative conclusions of the study are interesting and important, the exercise is primarily one in methodology, though the authors prefer to call it "a preliminary essay into substantive matters" (p. 54). The statistical analysis runs "in terms of multiple regression estimation of the inter-relationships among 15 variables" (p. 69). The analysis was applied to the 1950-60 data for a sample of 52 countries, which were divided into six groups "to test the behaviour of the social variables at different income levels" (p. 67).

The study itself is preceded by a summary of the aims, methods, and results of the study, written by Mr. Koji Taira of the International Labour Office. This is followed by a brief statement of previous relevant work; the method of the study and a model which defines labor in terms of its quality are then presented; a description of the indicators of economic growth and labor quality follows. The last three chapters are devoted to the statistical methods, results, problems encountered, conclusions, and possible further studies.

The precise indicators of economic growth and of labor quality used in the study were dictated by the availability of data which had to be internationally comparable, cover a sufficiently large number of countries, and cover a sufficiently long period of time to be useful for analysis. While these criteria severely limited the choice of data, the authors "have come to feel that they are meaningful in relation to real phenomena" (p. 53).

The economic variables chosen for each country are the rate of growth of gross domestic product at constant prices, valued at factor cost, the investment ratio, and the economically active population ("the weakest link in the entire study") weighted by the wage share. The indicators of labor quality are: for education—enrollments at various school levels; for health—inhabitants per physician, hospital beds per 1,000 inhabitants, calories available per head, and infant mortality; for housing—dwelling units completed per head and the ratio of fixed capital formation in dwellings to gross national prod-

uct; for social security—social security benefits paid as a percentage of national income, and average annual social security expenditures per head of population between 15 and 64 years, at constant prices.

Keeping in mind the authors' warning that "this is a preliminary study, the data are rough, and the results must be taken with more than the customary grain of salt" (p. 87), the conclusions are nevertheless important. They do challenge the assumption which many of us have made that education is the best labor quality indicator. "Neither the housing nor the educational variables, taken as separate groups, appear to afford an adequate explanation of the growth variable. . . . The health and social security groups do somewhat better, with the former in particular appearing to offer a fair degree of explanation" (p. 87). Of the idividual quality variables tested, "the level of nutrition, as measured by daily calories available per head, seemed to yield the closest relationship with economic growth" (p. 88). "The increase in higher educational enrollment showed some promise as an explanatory variable, particularly among the low income countries. . . . However, the relationship was not sufficiently strong to warrant the flat assertion that an expansion of higher education is essential to growth" (p. 88). It is to be hoped that the analysis will be extended and that the obstacles to the solution of the riddle will be removed. This is important for purposes of public policy.

The chief obstacle is the lack of appropriate data. This deficiency forced the investigators to use certain items as indicators of the quality of labor which in theoretical terms might not be the most acceptable. In addition there is doubt about the quality of some of the statistical series, "but, even more fundamental, there are basic conceptual discrepancies between what has been and what we would have liked to see measured" (p. 86).

This is a pioneer work which requires refinement as to methodology. This can be achieved. But much more has to be done to improve the quality of the statistical data appropriate for the analysis and to enable us to fill the otherwise "empty boxes."

W. J. WAINES

University of Manitoba

The Negro and Organized Labor. By RAY MARSHALL. New York: John Wiley and Sons, 1965. Pp. 327. $6.95.

Ray Marshall wrote this book to illuminate the basic forces "tending to perpetuate or change union racial practices." To accomplish this, he undertook analysis of: (1) "the factors responsible for the evolution of union racial practices," (2) "the main pattern of union racial practices," (3) "the influence of public policy on union racial practices." With illustrative examples and fluent prose, he accomplishes precisely what he intended, what no one else has attempted.

In 1944, Herbert Northrup used the five words of Marshall's title for his book, *Organized Labor and the Negro* (New York: Harper). Marshall's readers should recognize that Northrup essayed (p. xv) "an impartial study of the effect of the policies of labor unions on the welfare . . . of the Negro,"

while Marshall's concern is not with Negro welfare but with forces affecting union racial practices. Marshall's watchword is "Dispassion." Angry words by Gompers, Randolph, Meany, and Hill flash as union racial practices evolve. Marshall neither applauds nor condemns. He acknowledges personal convictions that "discrimination in employment is morally and economically wrong." But measuring opposing forces, he remains dispassionately objective (he does use "progress" to mean decline in employment discrimination). Each step in his argument is carefully documented and conscientiously footnoted—often from his personal interviews and correspondence.

There are two criticisms: Readers need an appendix defining abbreviations to identify AFDA, BCTC, CCRC, DAD, EEOC, FTA, and others when met the second, and undefined, time. Marshall sometimes sees "progress" where actual change was infinitesimal. Reacting to this, Herbert Hill bitterly criticized Marshall's book in the NAACP's magazine, *The Crisis*, April 1965.

Analyzing the evolution of union racial practices, Marshall identifies five major forces: (1) The rule that unions exist to serve members' welfare and not the public good. (2) White reluctance to work with Negroes as equals. (3) Union power dependence on union control of all workers available to employers. (4) Union desire to restrict access to each occupation. (5) White employers' former disinclination to hire Negroes except as strikebreakers. Marshall shows how these forces operated to exclude Negroes from early unions and to bring Negroes in when unionization reached occupations employing many Negroes. Where, before 1934, Negroes held jobs, union leaders' concern with total organization conflicted with white members' prejudice. Both groups welcomed compromises displacing Negroes or admitting them as second-class members. Reacting to pre-1934 exclusion and discrimination, most Negroes became antiunion.

That union-Negro communications were maintained, Marshall shows by highlighting A. Philip Randolph's prounion antiunion-discrimination leadership 1917-64. Union and Negro leaders did join forces when organization on an almost nondiscriminatory basis was proposed for coal mining and mass production industries (e.g., meat packing, automobiles) employing many Negroes. After 1957, Negro leaders redoubled pressure on unions to end all discrimination. Marshall shows how union leadership responded by hailing past progress, thus joining a dialogue in which neither party hears the other.

The book's middle section overlaps the first as it surveys union racial practices. The extremists were the railroad unions that with grim humor called themselves Brotherhoods and excluded all Negroes (on rational grounds given 1910 white assumptions). Boilermaker auxiliary locals, construction and longshoring segregated locals, and railroad freighthandlers' AFL-chartered locals illustrate categories of second-class union membership. New York and Cleveland electricians' and plumbers' informal exclusion is discussed in the context of 1963 civil rights protests. The protest in Cleveland, Marshall argues, accomplished more than the protest in New York because of unified leadership, while New York's splintered. The point is unconvincing since Cleveland's electricians Local 38 relaxed its monopoly only enough to admit

two Negroes. Marshall epitomized the union view on apprenticeship, nepotism, and amicism with the officer's protest that his union's apprenticeship program discriminated not against Negroes but against all races. Marshall surveys management trainee selection to refute Friedman's suggestion that markets tend to remove discrimination.

Surveying the South, Marshall finds white workers accepting or rejecting organization on grounds other than union racial practices or union leaders' speeches. Here, as elsewhere, Marshall emphasizes the gap between international officers' policy speeches and union locals' practices. The dependence of white members' support upon their locals' in-plant effectiveness is illustrated by examples of unsuccessful raiding efforts by groups wholely devoted to white supremacy but unable to deliver "union" benefits.

Concluding chapters analyze the impact of public policy on union racial practices. Marshall stresses government unconcern before 1942 and the puny power of World War II Fair Employment Committees. He traces the growth of government power through state commissions and the agencies of Eisenhower, Kennedy, and Johnson. He concludes that each made visible progress but none affected the basic employment patterns and associated union racial practices of many industries. Marshall finds the NLRB least among the least in past impact on union racial policies. The prospect for change turns upon the NLRB's 1964 Hughes Tool decision that union racial discrimination is an unfair labor practice subject to NLRB cease and desist orders. If federal courts sustain the decision, Marshall expects the NLRB to surpass all other government agencies in ability to induce locals to practice nondiscrimination.

Unfortunately no data exist permitting correlation of union discrimination with unemployment rates. Marshall repeatedly acknowledges the influence of unemployment rates but, lacking statistics, devotes relatively little space to this fundamental relationship.

Marshall set out to illuminate forces shaping union racial practices. Midway, he offers a generalization summarizing the entire book: "Taking unions as a whole . . . we must conclude that few of them have done much to improve the Negro's job conditions and opportunities where this was not incidental to such general objectives as wage equalization and wage leveling."

ALAN B. BATCHELDER

Kenyon College

The Labor Sector—An Introduction to Labor in the American Economy. By NEIL W. CHAMBERLAIN. New York: McGraw-Hill Book Company, 1965. Pp. x, 758. $9.50.

In his preface to this book, Professor Neil W. Chamberlain notes that it is not a revision of his previous textbook, *Labor*. The differences between the two books are sufficiently great that one must unquestionably agree that *The Labor Sector* is a new textbook.

Chamberlain has written quite a number of books in the past few years. He not only writes well, but manages to put some kind of new twist or approach in each of his writings. *The Labor Sector* has something special. The organi-

zation of this labor textbook is significantly different from that of most other texts. And from this reviewer's point of view, the standard materials of a labor course are organized in this book in a way that should make them easily teachable.

The standard materials are organized into 29 chapters and 9 appendices, all of which are grouped into eight sections. Section I covers labor as a source of income, and the material included covers the household, occupational choice, job movements, and the labor force. The second section is devoted to labor as a movement, and here we have trade union history, union-employer relations, the government of unions, collective bargaining, and theories of the labor movement.

Labor as a factor of production is discussed in Section III, and labor as a market is covered in Section IV. Section V is a single chapter, on labor's political action. Section VI, "Labor as the Subject of Social Protection," contains four chapters covering such items as minimum wage and maximum hour legislation, accidents and illness, unemployment, and old age and retirement. Each of these chapters has a special appendix. Section VII is devoted to the relation of labor to the performance of the economy. Here are chapters on the strike, the monopoly powers of unions, and a discussion of labor's role in the problems of full employment and inflation. The last section, Section VIII, is a single chapter titled "A Look to the Future."

Many of the sections are excellently done, and I am certain that few students will find fault with Chamberlain's style or with the readability of this book. Even in the rather numerous appendices, where many authors tend to place the technical or the more difficult materials, Chamberlain manages to maintain his simple and clear presentation. At the end of each chapter, the author has listed a series of questions "For Analysis and Discussion." In general, these questions are very good and undoubtedly will be provocative to students. For the instructor who so chooses, these questions can readily be used as the basis for discussion, quizzes, and examinations.

Chamberlain has taken great pains to cover in detail those items selected for this textbook. In some instances, one may perhaps say that the coverage is encyclopedic. Almost every significant piece of research in the general field of labor is referred to and is frequently quoted at some length; and the footnotes at the end of some chapters go on for four or five pages.

While the reviewer enjoyed reading the book, he wonders how the average college junior, taking his first labor course, would react to the details of the various research findings of the different labor economists cited. For the more advanced student, this book could be used as a review of recent research and of recent controversies in the labor economics field.

The principal criticism of the book is not with the quality of its contents, but with its size and format. Apparently because of its encyclopedic nature, the book was published in double columns with small print. Despite its format, the book still runs over 750 pages, which can frighten not only students, but also teachers. Even in a two-semester course, it might not be easy to

cover all the material. By more careful selection of materials, the book could have been shortened significantly.

MORRIS A. HOROWITZ

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Poor Countries and Authoritarian Rule. By MAURICE F. NEUFELD. Ithaca: New York State School of Industrial and Labor Relations, 1965. Pp. 240. $5.00.

Professor Neufeld set himself a very ambitious task: a general explanation of the emergence of one-party systems in much of the contemporary underdeveloped world. It is his thesis that "Italy, of all the countries of Europe with extended experience of industrial change . . . even today still resembles most closely the emerging nations of the world in the character of its past and present economic, cultural, political, and social development" and that the experience of Italy in these spheres "offers both close and partial parallels to similar but later trends in the poor countries of Asia, Africa, and Latin America." He feels that a reading of Italian history raises doubt about the efficiency of economic and technical measures for bringing about change in the face of deep-seated noneconomic obstacles imbedded in the cultures of the underdeveloped nations.

Nine "historical propositions" are advanced in support of this view. They are extremely broad and comprehensive: cultural nationalism precedes political nationalism; nationalist leaders lay heavy stress, in the struggle for power, upon the peculiar virtues of their own people; but they would not have achieved independence so soon "in the absence of wars and their economic and social consequences, the balance of world power, military alignments, diplomatic skill, and marked changes in public opinion." Once freedom is gained, backwardness leads to the extension of central rule into every corner of the nation, and to excessive expenditures on public works and defense at the expense of education, health, and housing. Radical solutions espoused by intellectuals who are appalled at the persistence of poverty are suppressed by the new leaders, who became increasingly authoritarian. Trade unions are weak and cannot offer effective resistance to this trend. The final irony is that authoritarianism does "not even advance industrial progress against the backward pull of historical forces any more swiftly than the looser and less involved devices of liberal parliamentary systems."

The argument is pursued almost entirely through the medium of historical vignettes, many of them no longer than a paragraph or even a sentence in length, each one detailing some bit of relevant experience; for example, "The Shah of Iran, while addressing the National Press Club in mid-April 1962, vowed to follow 'a type of democracy peculiar to ourselves,' " or "Madeira Keita of Mali also extolled the virtues of the single-party system for Africa." Figures are scattered throughout the text, often in the form of prose versions of statistical tables that would have been easier to digest in their original form. There are lists of names and places that usually do not mean much to

the uninitiated reader. Apart from Italy, the major emphasis is upon Indonesia, but one is constantly kept hopping from continent to continent in pursuit of suitable illustrative material.

A basic difficulty with the book is the failure to explain the nature of the Italian model with sufficient care. There is some discussion of its slow economic development, the lack of a substantial middle class which made social stability impossible (how about Germany?), and the political consequences of nationalism, but it is never made clear why fascism came about, and whether, indeed, the author considered it inevitable under the circumstances. One would like a much better reasoned causal sequence before equating the rise of Mussolini with the emergence of such present-day leaders as Sukarno, Ne Win, Ayub Khan, Nasser, Ben Bella, Touré, Nkrumah, and Nyerere. And surely it requires more than a bald statement to establish a parallel between the period of Giolitti and the era of Nehru.

About a third of the text and a long appendix are devoted to the role of the labor movement. Neufeld sets forth some observations on the nature of unions in the developing nations: they are weak because of the relatively small industrial sectors in which they operate; they lack trained leaders and adequate funds; they are strongly influenced by alien ideologies; they tend to rely on politics rather than collective bargaining; regional rather than industrial structure is common. But all of this has been said many times, and not much illumination is added by a little Italian labor history.

International comparisons can be very useful. They may lead to generalizations not readily discerned from the study of single national units, even large ones. This method of analysis must be employed with care, however, if the conclusions are to be anything more than mere impressions, interesting perhaps, but without any real theoretical significance. The first condition is a well-developed analytical framework, with particular attention to causal relationships. The historical experience of a single nation does not constitute an adequate model *ipso facto;* there must first be a thorough analysis of the relevance of this experience to the problems at hand, and of the critical similarities and differences in economic and social structure among the nations that enter into the comparison. The growing body of quantitative material should be employed as rigorously as its quality permits. Anecdotal citation is not an adequate substitute, even in the hands of so intuitive and experienced a scholar as Neufeld.

WALTER GALENSON

University of California, Berkeley

Employment Policy and the Labor Market. Edited by ARTHUR M. ROSS. Berkeley and Los Angeles: University of California Press, 1965. Pp. vii, 406. $7.50.

This book contains 14 papers, including an introduction by Arthur M. Ross, divided into four parts. All of the papers were presented at a research conference between June 15 and 19, 1964, and are part of a four-year program of research conducted by the University of California with the support of the Ford Foundation.

The first part is on general employment policies and consists of three chapters: (1) "Full Employment as a Policy Goal" by R. A. Gordon; (2) "Income Policy and Income Behavior" by Joseph W. Garbarino; and (3) "Unemployment and Wage-Price Politics" by Murray Edelman and Robben W. Fleming.

The second part relates to causes, concepts, and measures of unemployment. The chapters are (4) "Labor Force Participation and Unemployment" by William G. Bowen and T. A. Finegan; (5) "The Collection of Job Vacancy Data Within a Labor Turnover Framework" by Robert Ferber and Neil Ford; (6) "Seasonal Unemployment and Economic Conditions" by David C. Smith; (7) "Structural and Deficient-Demand Unemployment Reconsidered" by Richard G. Lipsey; and (8) "An Approach to an Absolute Measure of Structural Unemployment" by Barbara R. Berman.

The third part of the book is devoted to the labor market and training policies. There are four papers: (9) "Retraining: An Evaluation of Gains and Costs" by Gerald G. Somers; (10) "Apprenticeship: An Evaluation of the Need" by George Strauss; (11) "Labor Market Adjustments by Unemployed Older Workers" by Irvin Sobel and Hugh Folk; and (12) "An Integrated Positive Manpower Policy" by E. Wight Bakke. The fourth part is on the experience of being unemployed and contains one chapter, (13) "Unemployment as a Way of Life" by Paul Jacobs.

R. A. Gordon develops the relationships among the welfare goals of rate of unemployment, rate of economic growth, price stability, balance of payments, and "incomes policy." He analyzes the major goals of unemployment, economic growth, and price stability in the light of constraints imposed by these same and other variables. The combined target and constraint approach creates an interesting presentation of comparative values among the welfare goals in different countries. The United States stands out with its relatively high tolerance for unemployment. The final section of the chapter deals with disaggregating the full-employment goal, and further analysis is promised in a larger study of which the paper is a beginning report.

Garbarino enlivens the probable dilemma among the goals of full employment, price stability, and free collective bargaining. As part of a comparative study of experience in 16 countries, the author develops a very interesting cross-sectional analysis of unemployment and rates of price-change in eight countries. The results are quite comparable to U.S. experience in the years 1954-62, and similar to the results of other such studies. Clearly, as Garbarino states, "the use of one variety of wage policy rather than another" does not appear in the statistical record "to have had any consistent differential impact." Garbarino surrounds his analysis with qualifications which deserve respect, but a most interesting question is how well these relationships will stand up in the light of additional years of experience.

The paper by Edelman and Fleming is based upon a forthcoming book on the politics of wage-price decisions in four Western European countries. While it is clear and interesting that governments frequently use wage-price policies as "expressive political acts" as contrasted with "instrumental acts" with clearer discernible economic consequences, the complexities of the eco-

nomic-political variables do not compress too well into a single chapter.

Bowen and Finegan present a very interesting statistical study of the determinants of labor force participation rates for five groups of workers: prime-age males, older males, married women, teen-age males, and teen-age girls. The method used is cross-sectional analysis of labor market (metropolitan) areas. The major variables studied are unemployment, earnings opportunity, nonlabor income, education, and color. They also estimate hidden unemployment. While the results support in general a direct relationship between higher participation and employment opportunities, there are some interesting surprises in the data. The study of which this paper is a part will make a major contribution to our understanding of participation rates.

The "collection of job vacancy data" by Robert Ferber and Neil Ford is a careful piece of research, but I fear it impresses one with the complexity of developing meaningful concepts and data in this area, even though the authors are optimistic from their pilot study. Something of the same conclusion may well apply to the refinement of seasonal adjustments as analyzed by David C. Smith, though here, perhaps, the problem is more manageable.

The theoretical papers by Richard G. Lipsey and Barbara R. Berman on structural unemployment are decidedly provocative and contribute significantly to our thinking on this subject. Both authors, as emphasized by Ross in the introduction, define the concept in terms of policies and actions needed for its elimination rather than as unemployment caused by certain circumstances. The concept, however, remains a difficult one.

The study on retraining by Gerald Somers, again a part of a larger project, compares the costs and benefits of retraining. He comes to a generally favorable evaluation: "The economic gains of the trainees—and perhaps of society—derived from retraining would seem to outweigh the economic costs. If the social-psychological benefits accruing from an unemployed worker's return to the active labor market are added, the scales are heavily weighted on the positive side." There remain, however, many questions, as noted by Somers.

George Strauss concludes that "the function of apprenticeship today seems largely to be that of training skilled key men, and the proportion of job entrants seems to be quite closely related to the skill mix which the trade requires." He, therefore, sees no reason to anticipate a "crisis" in training apprentices. While granting the social desirability of giving apprenticeship a larger role, he doubts that this is realistic.

To this reviewer the most interesting finding of the Sobel and Folk paper on the labor-market adjustment of the older worker was the very limited extent to which the older worker was less adjustable than others and that, to the extent (among all employees) that adjustment was made, it tended to be forced by increased duration of unemployment.

E. Wight Bakke has a strong statement arguing for an "integrated positive manpower policy." It is difficult to challenge the logic of Bakke's position, but how far it is realistically possible to go in this direction is by no means clear to the reviewer.

Jacobs has an interesting paper on unemployment as a way of life. The

paper is based on his experience in living among and as an unemployed worker. He finds the unemployed singularly set apart in a world of their own and as individuals.

The papers in the book are uniformly of high quality. Most present original research data and are a part of an ongoing project. The introduction by Ross gives a longer and better orientation toward the papers than is possible here. In total the completed and projected research will make a substantial contribution to knowledge and should assist meaningfully in the development of improved public policies.

E. R. LIVERNASH

Harvard University

The Economics of Labor and Collective Bargaining. By WILLIAM H. MIERNYK. Boston: D. C. Heath and Company, 1965. Pp. x, 502. $8.50.

This text was designed for the "conventional full-year course in labor and industrial relations." The second half can also be used in a one-semester course in collective bargaining "if supplemented by appropriate case material." The author's method is an "integrated use of historical, institutional, and theoretical approaches"; although, as he notes, it would be misleading to imply that his is a heavily theoretical book. He states that he has made no attempt to avoid controversial issues, but has endeavored to present various points of view, having as his objective the "presentation of an analytical framework which will permit better understanding of some of the complex issues faced by unions, management, and the public."

In his preface Professor Miernyk asserts that "the organization of a textbook is less important than its scope, since the individual instructor can assign sections in any order he wishes if each section is relatively self-contained." The author has chosen the following sequence: (1) History of American Unionism; (2) Trade Unions and Public Policy; (3) The Structure, Government, and Objectives of Trade Unions; (4) Collective Bargaining—Process and Issues; (5) Labor Economics: The Labor Market and Wage Determination; and (6) Manpower Management—The State of the Labor Movement. The first portion of Section 6, Manpower Management, is a discussion of personnel management—not of manpower policies.

In the reviewer's opinion, an important objective of the first course in labor and industrial relations is to develop the understanding and analytical abilities of students. Thus, while in agreement with the author as to the importance of not avoiding controversial issues, the reviewer would prefer to have these issues treated in a manner designed to focus attention on the forces which bring these issues into being and which cause them to remain controversial. It is not sufficient to identify, as the author has done, the disputes that exist among economists and to indicate how their views differ. Instead, an author must integrate these conflicting opinions; what is needed is to direct the student's attention toward what seem to be the main causal relationships and toward the implications of conflicting policy views. In this text, the latter

type of analysis is neglected. For example the inadequate demand vs. structural transformation controversy concerning the nature of recent unemployment is identified, but the contrasting policy implications are dismissed with a statement that "a many-pronged attack on the problem of persistent unemployment might be necessary."

In his pursuit of self-contained units, the author has tended to sacrifice his analytical framework. One example is that the description of the Landrum-Griffin Act precedes by some 50 pages the discussion of the problems it was intended to correct. Another is that management attitudes toward trade unions and toward collective bargaining are not discussed until Chapter 23, virtually the end of the book.

The strongest units in the book, as one might expect from Professor Miernyk, are the sections on union growth and the future of American unions. These are well written and perceptive. The section on collective bargaining would have been stronger if space had been devoted to a discussion of the determinants of bargaining power instead of to the exposition of formal bargaining theories.

As a final comment, it should be noted that certain of the units are dated, as the text contains no material later than mid-1963.

Kenneth Strand

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Related Disciplines

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NOTES

New Journals

A new journal, *Latin American Research Review,* has published its first issue this year. Each issue will have three sections: a topical review, a current research inventory, and a newsletter. Inquiries for further information should be addressed to The Editor, *Latin American Research Review,* University of Texas, Box L, Austin, Texas 78712.

A new quarterly journal, *Manpower Journal,* began publication in April 1965, under the auspices of the Institute of Applied Manpower Research, New Delhi, India.

20th Anniversary of Employment Act

Plans have been announced for an economic symposium on February 23, 1966 to commemorate the 20th anniversary of the Employment Act of 1946. The subjects to be discussed include (1) the Employment Act objectives—after 20 years; (2) aggregative or structural approaches to achieving Employment Act objectives; (3) using fiscal and monetary policies to further Employment Act objectives; (4) sharing the increasing productivity of our economy; and (5) administration of the Employment Act. The sessions, at the New Senate Office Building, will be open to the public. Requests for tickets for the luncheon ($4) as well as the reception and dinner ($15) should be sent to: Special Committee on 20th Anniversary of the Employment Act, Room 908, Colorado Building, 14th and G Streets, N.W., Washington, D.C.

AER Manuscripts

The following manuscripts, exclusive of comments and replies and in addition to those listed in the issue of June 1965 on page 684, have been accepted for publication in subsequent issues of the *American Economic Review*:

Peter Eilbott, "The Effectiveness of Automatic Stabilizers."
H. A. J. Green, "Embodied Progress or Embodied Regress?"
A. K. Klevorick, "The Graduated Fair Return—A Regulatory Proposal."
M. C. Kemp, "The Gain from International Trade and Investment: A Neo-Heckscher-Ohlin Approach."
Harvey Leibenstein, "Allocation Efficiency vs. 'X-Efficiency.'"
Nissan Liviatan, "Multiperiod Future Consumption as an Aggregate."
Merton Miller and Franco Modigliani, "Some Estimates of the Cost of Capital to the Electric Utility Industry, 1954-57."
W. F. Owen, "The Double Developmental Squeeze on Agriculture."
Richard Quandt, "On the Size Distribution of Firms."
Tibor Scitovsky, "An International Comparison of the Trend of Professional Earnings."
O. P. Tangri, "Two Omissions in the Treatment of the Law of Variable Proportions."
D. P. Tucker, "The Dynamics of Income Adjustment to Money Supply Changes."
L. W. Weiss, "Concentration and Labor Earnings."

Announcements

The Bureau of Labor Statistics proposes to sell a General Purpose Tape of basic data from the Survey of Consumer Expenditures, 1960-61. Anyone interested should indicate in writing to the Commissioner of Labor Statistics, Bureau of Labor Statistics, U.S. Department of Labor, Washington, D.C. 20212, that he wishes to receive a contract for purchase of the tape with the final price (from $1,720 to $2,825) dependent upon the number of contracts consummated.

The Inter-University Committee on Travel Grants announces opportunities for advanced graduate students and scholars to engage in study and research in the Soviet Union, Bulgaria, Czechoslovakia, and Hungary during the academic year 1966-67. This exchange, presently in its eighth year, is made possible by the intergovernmental agreements on exchanges with the USSR and agreements with the educational ministries of Bulgaria, Czechoslovakia, and Hungary. The exchange is administered by the Inter-University Committee on Travel-Grants, a multi-university American organization whose objective is to strengthen education in the United States. The program includes a summer exchange of American teachers of Russian language with the Soviet Union.

All participants are chosen in national competition through application and interview. Participants must have proficiency in the language of the country commensurate with the needs of their programs. Applicants must be either American citizens or permanent residents of the United States. For additional information write to Dr. Howard Mehlinger, Inter-University Committee on Travel Grants, 021 Lindley Hall, Indiana University, Bloomington, Indiana 47405.

The first issue of a new information service, the *Weekly Compilation of Presidential Documents,* was recently published by the General Services Administration. The issues will carry transcripts of the President's news conferences, messages to Congress, public speeches and statements and other Presidential materials released by the White House. The publication will be sold to the public on a subscription basis ($6.00 per year) by the Superintendent of Documents, Government Printing Office, Washington, D.C. 20402.

The Associación para el Desarrollo, Inc. of Santiago, Dominican Republic, proposes to sponsor a program encouraging Dominican and foreign students to take advantage of that country's research opportunities. Scholars interested in pursuing academic research in the Dominican Republic may write to Mr. Abraham W. Lowenthal, Program Assistant, Asociación para el Desarrollo, Inc., Apartado 484, Santiago, Dominican Republic.

The Asia Foundation's Books for Asian Students program continues to seek books in very good condition published in 1950 or after and runs of professional journals. This program in ten years of operation has sent five million volumes and one million journals to thousands of institutions in Asia. The need for additional materials remains great The program will pay for all shipping: 200 pounds or less, Special 4th Class Rate—Books (send postal receipt for reimbursement); over 200 pounds, motor freight (truck) collect Donations and questions should be addressed to: Books for Asian Students, 451 Sixth Street, San Francisco, California 94103. Donations are tax deductible.

The Institute of Social Economics, Korea University, 1 Anam-Dong, Seoul, Korea, needs economics books and materials and asks for assistance in building its library resources.

The Institute of Management Sciences will hold its annual National Meeting in Dallas, Texas, February 16-19, 1966.

Deaths

Bernard M. Baruch, life member of AEA since 1921.

Joseph W. Conard, professor of economics, Swarthmore College, April 5, 1965.

James Hodgson, Faculté des Sciences Sociales, Quebec, Canada.

Oskar Lange, October 1965.

Arthur F. Messenger, associate professor of economics, Krannert Graduate School of Industrial Administration, Purdue University, May 13, 1965.

Barbara M. Miller, instructor of economics, North Carolina Wesleyan College, April 15, 1965.

Thomas W. Streeter, Morristown, New Jersey, June 12, 1965.

V. F. Timoshenko, Palo Alto, California, August 15, 1965.

Abbott Payson Usher, professor emeritus since 1949, Harvard University, June 28, 1965.

Retirements

H. Kenneth Allen, professor of economics, University of Illinois.

Antonin Basch, visiting professor, department of economics, University of Michigan.

George E. Bates, James B. Williston professor of investment management, Graduate School of Business Administration, Harvard University.

Arend E. Boer, professor of marketing, Graduate School of Business, University of Pittsburgh.

Persia Campbell, professor of economics, Queens College of the City University, New York.

William J. Carson, associate professor of finance, Wharton School, University of Pennsylvania, and executive director of the National Bureau of Economic Research.

Arthur G. Coons, president, Occidental College.

Frank W. Goodwin, professor of marketing, University of Florida.

Erwin Graue, professor of economics, University of Idaho.

Fred J. Halterman, University of California, Santa Barbara.

G. Wright Hoffman, professor of insurance, Wharton School, University of Pennsylvania.

John H. Keenan, assistant professor of business and economics, University of Maine, Portland.

Charles E. Landon, professor, department of economics, Duke University.

D. Philip Locklin, professor of economics, University of Illinois.

Harry J. Loman, professor of insurance, Wharton School, University of Pennsylvania.

Robinson D. McIntyre, professor of business administration, University of Kentucky.

Simon Naidel, professor emeritus, American University.

Gladys Palmer, research professor of industry, Wharton School, University of Pennsylvania.

Rollin G. Thomas, professor of economics, Krannert Graduate School of Industrial Administration, Purdue University.

Visiting Foreign Scholars

Frank P. R. Brechling: visiting associate professor of economics, Massachusetts Institute of Technology, 1956-66.

Michael Bruno: visiting associate professor of economics, Massachusetts Institute of Technology, 1965-66.

N. G. Butlin, Australian National University: Irving Fisher research professor of economics, Yale University, fall 1965.

José Encarnación, Jr., University of the Philippines: Institute for Mathematical Studies in the Social Sciences, Stanford University, 1965-66.

Barry L. J. Gordon: visiting lecturer, department of economics, Massachusetts Institute of Technology, 1965-66.

Kai-Loo Huang, University of Singapore; Nanyang University: visiting professor, department of economics, University of Wisconsin, 1964-65.

Frank Jarrett, University of Adelaide, Australia: visiting professor of economics, University of Minnesota.

Murray Kemp, University of New South Wales, Australia: visiting professor of economics, University of Minnesota.

Albert Kervyn, Planning Office of the Belgian Government: Irving Fisher research professor of economics, Yale University, spring 1966.

Don Patinkin, Hebrew University, Jerusalem: visiting professor, Johns Hopkins University, October 1965.

Michael M. Postan: visiting professor of economic history, Massachusetts Institute of Technology, fall terms, 1965-67.

Peter Robson, University College, Nairobi, Kenya: visiting professor, department of economics, University of Illinois.

Vladimir Stoikov, Economic Branch of the International Labor Office, Geneva: visiting associate professor of labor and industrial relations, University of Illinois.

Alan A. Tait, Trinity College, Dublin, Ireland: visiting lecturer of economics, University of Illinois.

Akira Takayama, University of Manchester, England: visiting associate professor of economics, University of Minnesota.

Jacques Waelbroeck: visiting professor of economics, Massachusetts Institute of Technology, 1965-66.

Promotions

Peter S. Albin: assistant professor of economics, New York University, Washington Square College.

Abbas Alnasrawi: assistant professor, department of commerce and economics, University of Vermont.

Martin Anderson: associate professor of finance, Graduate School of Business, Columbia University.

William J. Barber: professor of economics, Wesleyan University.

Sam Barone: associate professor, St. Louis University.

Joseph M. Belth: associate professor of insurance, School of Business, Indiana University.

John C. G. Boot: professor of management science, State University of New York at Buffalo.

Howard Bourne: associate professor, department of economics, Wayne State University

Harold E. Bronson: special lecturer in economics, University of Saskatchewan.

Bernard Brown: associate professor of economics, University of Houston.

Byron B. Brown, Jr.: associate professor of economics, Wisconsin State University, Eau Claire.

John C. Burton: associate professor of accounting, Graduate School of Business, Columbia University.

John F. Burton, Jr.: assistant professor of economics, Yale University.

David Cass: assistant professor of economics, Yale University.

Whitney A. Chamberlain: associate professor of accounting, Old Dominion College.

K. Lawrence Chang: associate professor of economics, Western Reserve University.

N. K. Choudhry: associate professor of economics, University of Toronto.

Richard N. Cooper: associate professor of economics, Yale University.

Mrs. N. Z. Davis: assistant professor of economics, University of Toronto.

Herbert Denenberg: associate professor, department of insurance, Wharton School, University of Pennsylvania.

Norton T. Dodge: associate professor of economics, University of Maryland.

Floyd Durham: associate professor of economics, Texas Christian University.

Eldon J. Dvorak: associate professor, California State College, Long Beach.

Richard S. Eckaus: professor of economics, Massachusetts Institute of Technology.

Robert D. Eilers: associate professor, department of insurance, Wharton School, University of Pennsylvania.

Franklin M. Fisher: professor of economics, Massachusetts Institute of Technology.

Robert Flammang: associate professor of economics, Louisiana State University.

M. June Flanders: associate professor of economics, Krannert Graduate School of Industrial Administration, Purdue University.

Max E. Fletcher: professor of economics, University of Idaho.

Robert W. Fogel: professor of economics, University of Chicago.

D. F. Forster: associate professor of economics, University of Toronto.

Edward M. Foster: associate professor of economics, University of Minnesota.

William J. Frazer: associate professor of finance, University of Florida.

Erik G. Furubotn: professor of economics, State University of New York at Binghamton, Harpur College.

Francis W. Gathof, Jr.: associate professor of economics, Beloit College.

Fred M. Gottheil: associate professor of economics, University of Illinois.

Donald H. Granbois: associate professor of marketing, School of Business, Indiana University.

Peter Gregory: associate professor of economics, University of Minnesota.

Paul Green: professor, department of marketing, Wharton School, University of Pennsylvania.

Irvin M. Grossack: associate professor of business economics and public policy, School of Business, Indiana University.

Donald G. Hanrahan: associate professor, St. Louis University.

Robert W. Harbeson: McKinley Professorship of the economics of public utilities, University of Illinois.

D. G. Hartle: professor of economics, University of Toronto.

John C. Hause: associate professor of economics, University of Minnesota.

Donald D. Hester: associate professor of economics, Yale University.

Charles Hoffmann: professor of economics, State University of New York, Stony Brook.

John J. Hooker: associate professor of economics, Catholic University of America.

John G. Hutchinson: associate professor of management, Graduate School of Business, Columbia University.

Max B. Jones: professor of business management, Old Dominion College.

Arcadius Kahan: professor of economics, University of Chicago.

Benjamin J. Katz: professor of economics, New York University, University College.

Robert W. Kautz: professor of economics, University of Saskatchewan.

Paul E. Koefod: professor of economics, University of Florida.

L. F. Kristjanson: associate professor of economics, University of Saskatchewan.

A. Kruger: associate professor of economics, University of Toronto.

James W. Land: assistant professor of economics, Yale University.

John E. Latourette: associate professor of economics, State University of New York at Binghamton, Harpur College.

Chong-Sik Lee: associate professor, department of political science, Wharton School, University of Pennsylvania.

John R. Malone: professor of marketing management, College of Business Administration, University of Notre Dame.

Andrea E. Maneschi: assistant professor of economics, Yale University.

Matityahu Marcus: associate professor of economics, Rutgers—The State University.

Melvin T. McClure: assistant professor of business and economics, University of Maine.

Adrian M. McDonough: professor, department of industry, Wharton School, University of Pennsylvania.

John L. McEntaffer: assistant professor, department of economics, Bowdoin College.

John J. McGowan: assistant professor of economics, Yale University.

S. Sterling McMillan: professor of economics, Western Reserve University.

Jack Melitz: associate professor of economics, Tulane University, 1965-66; also appointed editor, *Southern Economic Journal.*

Joseph J. Melone: associate professor, department of insurance, Wharton School, University of Pennsylvania.

N. Meltz: associate professor of economics, University of Toronto.

Paul Merz: associate professor, St. Louis University.

Ronald E. Miller: associate professor, regional science department, Wharton School, University of Pennsylvania.

Harry A. Miskimin: associate professor of economics and history, Yale University.

George G. S. Murphy: associate professor, department of economics, University of California, Los Angeles.

E. P. Neufeld: professor of economics, University of Toronto.

Barbara W. Newell: associate professor of economics, Krannert Graduate School of Industrial Administration, Purdue University.

Howard Pack, assistant professor of economics, Yale University.

James A. Papke: professor of economics, Krannert Graduate School of Industrial Administration, Purdue University.

Richard E. Pasternak: associate professor of economics, University of South Florida.

William H. Patterson: associate professor of accounting, Old Dominion College.

George L. Perry: associate professor of economics, University of Minnesota.

Thomas A. Reiner: associate professor, regional science department, Wharton School, University of Pennsylvania.

Edward F. Renshaw: associate professor of economics and finance, University of North Carolina.

Robert W. Resek: associate professor of economics, University of Illinois.

Stephen A. Resnick, assistant professor of economics, Yale University.

Marcel K. Richter associate professor of economics, University of Minnesota.

Leonard Rico: associate professor, industry department, Wharton School, University of Pennsylvania.

Charles S. Rockwell: assistant professor of economics, Yale University.

Stephen W. Rousseas: professor of economics, New York University, Washington Square College.

Robert L. Sandmeyer: associate professor of economics, Oklahoma State University.

Paul E. Sands: assistant professor of business and economics, University of Maine.

John A. Sawyer: professor of economics and statisticss, University of Toronto.

Donald W. Seelye: associate professor of labor economics and business administration, University of Idaho.

Richard E. Shannon: professor, University of Montana.

Norman J. Simler: professor of economics, University of Minnesota.

Peter E. Sloane: associate professor of economics, Smith College.

Paul Smith: professor, finance department, Wharton School, University of Pennsylvania.

Robert F. Smith: associate professor of economics, Louisiana State University.

Thorn K. Snyder: chief economist, Arabian American Oil Company.

Donald Soligo: assistant professor of economics, Yale University.

William H. Starbuck: associate professor of administrative sciences and economics, Krannert Graduate School of Industrial Administration, Purdue University.

Martin K. Starr: professor of production management, Graduate School of Business, Columbia University.

D. A. Stager: assistant professor of economics, University of Toronto.

Frank G. Steindl: associate professor of economics, Oklahoma State University.

Stanley W. Steinkamp: associate professor of economics, University of Illinois.

Anthony H. Stocks: associate professor of economics, College of Commerce, West Virginia University.

Henry J. Teune: associate professor, department of political science, Wharton School, University of Pennyslvania, July 1, 1965.

Edward Van Roy: assistant professor of economics, State University of New York, Stony Brook.

Richard E. Walton: professor of administrative sciences and industrial management, Krannert Graduate School of Industrial Administration, Purdue University.

J. W. Warnock: assistant professor of political science, University of Saskatchewan.

E. Kirby Warren: associate professor of management, Graduate School of Business, Columbia University, July 1965.

M. H. Watkins: associate professor of economics, University of Toronto.

James A. Wilson: assistant professor of business administration and assistant to the dean, Graduate School of Business, University of Pittsburgh.

Mervyn W. Wingfield: professor of accounting, School of Business Administration, University of South Carolina.

Menahem E. Yaari: associate professor of economics, Yale University.

Murray Yanowitch: associate professor, department of economics, Hofstra University.

Wesley J. Yordon: associate professor, University of Colorado.

Dallas M. Young: professor of economics, Western Reserve University.

Administrative Appointments

Robert N. Anthony, Graduate School of Business Administration, Harvard University: Assistant Secretary of Defense.

James L. Athearn, professor of finance and insurance, University of Florida: dean, School of Business Administration, Montana State University.

Ruben V. Austin: dean, College of Business and Economics, University of Delaware.

W. Lee Baldwin: associate chairman, department of economics, Dartmouth College.

Edward E. Barr: vice-president, packaging group, Sun Chemical Corporation.

John F. Barron: vice-chairman, department of economics, University of California, Los Angeles.

Thomas R. Beard: acting head, department of economics, Louisiana State University, 1965-66.

Alvin B. Biscoe, Jr.: assistant director, Bureau of Economic and Business Research, University of Florida.

W. Donald Bowles: dean, College of Arts and Sciences, American University.

Colin D. Campbell: chairman, department of economics, Dartmouth College.

Jim L. Clapper: assistant dean for undergraduate administration, School of Industrial Management, Purdue University.

Paul G. Clark: chairman, Center for Development Economics, Williams College.

Paul G. Darling: chairman, department of economics, Bowdoin College.

W. Stanley Devino: dean, College of Business Administration, University of Maine.

A. Ross Eckler: Director of the Census, U.S. Department of Commerce.

Charles E. Edwards: editor of *Business and Economic Review*, University of South Carolina.

Walter L. Eisenberg, associate professor: chairman, department of economics, Hunter College of the City University of New York.

Douglas H. Eldridge, professor of public finance, Claremont Men's College: executive director, National Bureau of Economic Research.

Stephen Enke: Deputy Assistant Secretary of Defense, Economics.

Alain C. Enthoven: Assistant Secretary of Defense, Systems Analysis.

Alan D. Entine, assistant professor, department of economics: assistant dean, School of General Studies, Columbia University.

Sylvester M. Frizol: chairman, department of finance, Loyola University, Chicago.

Robert M. Gidez: director of the Appalachian Regional Commission's Planning Division.

Walter F. Hahn, Foreign Policy Research Institute of the University of Pennsylvania: assistant director, economic and political studies division, Institute for Defense Analyses.

Bernard Hall, associate professor and chairman, department of economics: acting dean, College of Business Administration, Kent State University.

Burton C. Hallowell: executive vice-president, Wesleyan University.

William Hamovitch: chairman, department of economics, Queens College, City University of New York.

Charles F. Haywood: dean, College of Commerce, University of Kentucky.

Clyde L. Irwin: assistant dean, College of Commerce, University of Kentucky.

David B. Johnson: chairman, department of economics, University of Wisconsin.

Harry G. Johnson, professor of economics, University of Chicago: president, Canadian Political Science Association, 1965-66.

Ole S. Johnson: dean, school of business, Oklahoma City University.

Mark L. Kahn: chairman, department of economics, Wayne State University.

John H. Kareken: acting chairman, department of economics, University of Minnesota, fall, 1965.

Frank L. Keller: acting chairman, department of economics, Tulane University, 1965-66.

Laurence E. Leamer: chairman, department of economics, State University of New York at Binghamton, Harpur College.

William N. Leonard: assistant president, Hofstra University.

David A. LeSourd: chairman, department of commerce and economics, University of Missouri.

Eli P. March: regional planner, Planning Division, Appalachian Regional Commission

Leonard W. Martin: chairman, department of economics, Loyola University, Chicago.

Joseph L. Massie: chairman, department of economics, University of Kentucky.

John T. Masten: chairman, department of economics, University of Kentucky.

Clyde C. Mitchell: regional director adviser in Agrarian Reform for South America FAO, January 1965; regional planning adviser, FAO, July 1965.

Chester A. Morgan: department chairman, University of Iowa, 1965-68.

Gail Mullin, associate professor of economics: assistant dean, College of Business Administration, and director, Master of Business Administration Program, Kent State University.

F. Stephen O'Brien: director of admissions for graduate study in development economics, Williams College.

Henry M. Oliver: acting chairman, department of economics, Indiana University, fall, 1965.

Wallace C. Peterson: chairman, department of economics, University of Nebraska.

Richard W. Poole: College of Business, Oklahoma State University.

C. Hoyt Price: director, office of telecommunications, Bureau of Economic Affairs, Department of State.

Julius Rezler: acting director, Institute of Industrial Relations, Loyola University, 1965-66.

Anthony V. Sinicropi: associate director, bureau of Labor and Management, University of Iowa.

Bernard F. Sliger: dean of academic affairs for the main campus, Louisiana State University.

Don Soule: associate professor of economics and associate director, Bureau of Business Research, University of Kentucky.

Robert W. Stone, Federal Reserve Bank of New York: vice-president of National Bank of Detroit, Michigan.

Robert H. Strotz, professor of economics: dean of the College of Arts and Sciences, Northwestern University.

Robert W. Tufts: chairman, department of economics, Oberlin College.

Peter N. Vukasin: chairman, social sciences division, State University of New York at Binghamton, Harpur College.

Richard J. Ward: chief, Planning Division, Office of Development Planning, Near East South Asia, Agency for International Development, Washington, D.C.

Willis D. Weatherford, Jr., Swarthmore College: dean, Carleton College.

Jude P. West: associate director, Bureau of Labor and Management, University of Iowa.

Harry Williams, system analyst: assistant director, Economic and Political Studies Division, Institute for Defense Analyses.

George W. Wilson: professor of economics and chairman, department of economics, Indiana University.

John Wise: acting chairman, department of economics, University of Hawaii, 1965-66.

Thomas A. Yancey: vice chairman, department of economics, University of Illinois.

Elliot Zupnick: acting executive officer of Ph. D. program in economics, City University of New York.

Charles J. Zwick: Assistant Director, Bureau of the Budget.

Appointments

John H. Abernathy: instructor, Southeastern Louisiana College.

Robert M. Aduddell: instructor in economics, Loyola University, Chicago.

Sydney N. Afriat: professor of economics, Krannert Graduate School of Industrial Administration, Purdue University.

Yoginder Alagh: lecturer in economics, Swarthmore College.

William Albrecht: assistant professor of economics, University of Iowa.

Carlos F. Diaz Alejandro: associate professor of economics, University of Minnesota.

Ralph S. Alexander: visiting professor of economics, Rutgers—The State University, 1965-66.

Robert Z. Aliber: associate professor of international economics and finance, Graduate School of Business, University of Chicago.

John R. Allan, Queen's University: department of economics, McMaster University, Ontario.

Guvenc Alpander: assistant professor in business and economics, University of Maine, Portland.

A. A. Anderson: assistant professor of marketing, University of Florida.

Roy C. Anderson: lecturer in economics, California State College, Long Beach.

Roman R. Andrus: visiting assistant professor of marketing, department of business administration, University of Iowa.

Leon Applebaum: associate professor, labor education and research service, Ohio State University.

Henry G. Aubrey: professor of economics, Sarah Lawrence College.

John H. Auten, Rice University: U. S. Treasury, Washington, D.C.

Robert N. Baird, University of Kentucky: assistant professor of economics, Western Reserve University.

Roy Bahl, Jr.: assistant professor of economics, college of Commerce, West Virginia University.

Eric N. Baklanoff, Vanderbilt University: director, Latin American Studies Institute, and associate professor of economics, Louisiana State University.

Michael Balinski, Wharton School, University of Pennsylvania: City College of New York.

Hiram C. Barksdale: professor of marketing, College of Business Administration, University of Georgia.

D. Marshall Barry, Northwestern University: part-time instructor in economics, Lake Forest College.

Carl L. Barton, NASA: center for naval analyses of the Franklin Institute, Mathematics and Operations Research Division.

John W. Bay: assistant professor in business and economics, University of Maine.

Jack N. Behrman: professor of international business administration, University of North Carolina.

Jere R. Behrman: lecturer in economics, Wharton School, University of Pennsylvania.

Louis Beleky: assistant professor of economics, Long Island University, C. W. Post College.

David A. Belsley: assistant professor, Dartmouth College.

Thomas Bennett: instructor in economics, Ohio State University.

Barbara Berman Bergmann, Brookings Institution: associate professor of economics, University of Maryland.

Herbert M. Berk: lecturer in political science, Wharton School, University of Pennsylvania.

Melvin K. Bers: associate professor of political economy, Graduate School of Public Affairs, State University of New York.

Stanley M. Besen, Institute for Defense Analysis: assistant professor of economics, Rice University.

Charles P. Blitch: associate professor of economics, Old Dominion College.

Arthur Bloomfield: visiting professor, department of economics, City University of New York.

Roger E. Bolton: assistant professor, department of economics, Williams College.

Sam H. Book: instructor in economics, New York University, University College.

H. Marshall Booker: assistant professor of economics, Old Dominion College.

Thomas R. Bossort, Jr.: professor of management, School of Business, Indiana University.

David A. Bowers: assistant professor of economics, Western Reserve University.

Patricia F. Bowers: department of economics, Brooklyn College.

Allen J. Braff: associate professor of economics and business, Whittemore School of Business and Economics, University of New Hampshire.

Gerald E. Breger: associate professor of business administration, University of South Carolina.

William L. Breit, Louisiana State University: associate professor of economics, University of Virginia.

Robert D. Britt: assistant professor of economics, Smith College.

Basil A. Browne: instructor in economics, University of Montana.

Karl Brunner, University of California, Los Angeles: visiting professor, department of economics, Michigan State University, 1965-66.

Heinrich Bruschke, University of Minnesota: assistant professor, St. Louis University.

Lillian G. Buchanan: instructor in economics, University of Saskatchewan.

John E. Buehler, State University of New York at Buffalo: assistant professor of business administration, Florida State University.

Donald E. Calvert: assistant professor and assistant dean, department of business administration, State University of New York at Buffalo.

Anthony S. Campagna: assistant professor, department of commerce and economics, University of Vermont.

Charles W. Campbell III. instructor, department of economics, University of Kentucky.

William M. Capron: senior staff, Brookings Institution, October 1, 1965.

Robert C. Cauthorn: associate professor of economics, West Virginia Center for Appalachian Studies and Development and College of Commerce, West Virginia University.

Margaret K. Chandler: professor of business, Graduate School of Business, Columbia University.

John H. Chapman: assistant professor of economics, Regional Research Institute and College of Commerce, West Virginia University.

Steven Cheung: assistant professor of economics, California State College, Long Beach.

V. S. Chitré: assistant professor of economics, University of Toronto.

Kee Il Choi, Harvard University: assistant professor of economics, College of William and Mary.

Soon Chough: assistant professor of economics, Whittemore School of Business and Economics, University of New Hampshire.

Gregory C. Chow: adjunct professor of economics, Columbia University, 1965-66.

Albert Christopher: assistant professor of business and economics, University of Maine, Portland.

William K. Chung: instructor, department of economics, Denison University.

Peter B. Clark: acting assistant professor, department of economics, University of California, Los Angeles.

Dale O. Cloninger: instructor in economics, University of South Florida.

John O. Cole: instructor in economics, Brown University.

Eugene E. Comiskey: assistant professor of industrial management, School of Industrial Management, Purdue University.

Muriel Converse: assistant professor, department of economics, Wayne State University.

Hector Correa: associate professor, department of economics, Wayne State University.

Morris A. Copeland: visiting professor, department of economics, University of Missouri, Columbia, February 1966.

Parzival Copes: professor of economics and head of department of economics and commerce, Simon Fraser University, Vancouver.

Joseph D. Coppock: professor of economics, Pennsylvania State University.

Joseph M. Crews: instructor in economics, Pennsylvania State University.

John G. Cross: assistant professor, department of economics, University of Michigan.

Ralph W. Cummings, University of Michigan: instructor, University of Illinois.

A. R. Curran: instructor in management, College of Business Administration, University of Georgia.

Lauchlin B. Currie, economic consultant to Columbia: distinguished visiting professor and research consultant to department of economics and Latin American Studies Center, Michigan State University, fall, 1965.

Ernest Dale: visiting professor of Industry, Wharton School, University of Pennsylvania, July 1, 1965.

Eliseo DaRosa: instructor, department of economics, University of Kentucky.

Samir Dasgupta: visiting assistant professor of economics, University of Saskatchewan.

Paul V. Daverio: faculty lecturer, School of Business, Indiana University.

Earl P. Davis: associate professor of accounting, University of Georgia.

John C. Deiter: assistant professor of economics, University of South Florida.

John W. Delaplaine, economist, Harvard University Development Advisory Service—Colombia Advisory Group—Bogota: Office of Program Advisor, Inter-American Development Bank, Washington, D.C.

Thomas F. Dernburg: professor of economics, Oberlin College.

Vernon L. Deslattes: assistant professor, Louisiana State University, New Orleans

Mona Dingle: professor, department of economics, University of Missouri, Columbia.

John V. Donovan: instructor in economics, Whittemore School of business and economics, University of New Hampshire, September 1965.

George W. Douglas, lecturer in economics, University of North Carolina.

Richard B. DuBoff: lecturer in economics, Swarthmore College.

J. Michael Dunton, University of Texas: instructor in economics, Del Mar College, Corpus Christi, Texas, fall, 1965.

Melvin A. Eggers: visiting professor of economics, Cornell University.

Mohamed A. El-Hodiri: assistant professor of economics, School of Industrial Management, Purdue University.

Hugh N. Emerson, Texas A&I College: professor of economics and chairman, business and economics department, Albright College.

Robert Emond: assistant professor of economics, Denison University.

Thomas P. Enger: assistant professor of economics, St. Olaf College.

James E. Estes: associate professor of management, school of business administration, University of South Carolina.

Richard H. Evans: assistant professor of business administration, Washington State University.

Albert A. Ewald: associate professor of accounting, University of South Carolina.

Marc Z. Fabrycy: instructor in economics, School of Commerce, New York University.

Sandra L. Farrar: instructor in business and economics, University of Maine, Portland.

Alim S. Fatemi: instructor in economics, University of Akron.

John C. H. Fei: professor of economics and Asian studies, Cornell University, fall, 1965.

John P. Fertakis: assistant professor of business administration, Washington State University.

J. Michael Finger, University of North Carolina: assistant professor, department of economics, Duke University, 1965-66.

Douglas Fisher: assistant professor of economics, Queens College of the City University of New York.

Irving Fisher: logistics department, RAND Corporation.

M. Blanche Fitzpatrick: instructor, department of economics, Boston University.

Roderick A. Forsgren: assistant professor of business and economics, University of Maine.

Jay D. Forsyth: instructor in economics, Colorado College.

Francesco Forte: senior staff (Rockefeller professor of international affairs), Brookings Institution.

Louis A. Foster: visiting professor of accounting, University of Florida.

Susan Foster: instructor in economics, Vassar College.

Charles R. Frank, Jr.: assistant professor of economics, Yale University.

R. D. Fraser: assistant professor of economics, Queen's University at Kingston.

Delmar Frazier, University of Florida: associate professor, Austin Peay State College, Clarksville, Tennessee.

A. Myrick Freeman III: assistant professor, department of economics, Bowdoin College.

Jay R. Galbraith: faculty lecturer, School of Business, Indiana University, 1965-66.

Gerald Garb: associate professor, department of economics, University of Kentucky.

Victor Garlin: lecturer in economics, San Francisco Medical Center, University of California.

Leonard J. Garrett, Wharton School, University of Pennsylvania: assistant professor of industry, Temple University.

Irving Gershenberg: assistant professor of economics, University of Connecticut, fall, 1965.

Matthew J. Gibney, Jr.: lecturer in economics, University of Maryland.

Frederick J. Glover: visiting lecturer in economics, Dartmouth College.

Charles J. Goetz: assistant professor of economics, University of Illinois.

Sara L. Gordon, Stanford University: acting assistant professor of economics, University of Hawaii.

Paul F. Grady: visiting professor of accounting, University of Florida.

Warren S. Gramm: associate professor of economics, College of Economics and Business, Washington State University.

Ralph Gray: associate professor of economics, DePauw University.

Sally H. Gray: instructor in economics, DePauw University.

H. A. J. Green, University of Toronto: Simon Visiting Professor, University of Manchester, 1965-66.

Frank Greenwood: associate professor of management, College of Business Administration, University of Georgia.

Michael J. Greenwood: assistant professor, department of economics, Kansas State University.

Ralph Grosswiler, Beloit College: assistant professor of economics, Colorado College.

George F. Hadley, University of New South Wales: visiting professor of economics, University of Hawaii, 1965-66.

Owen E. Hague: assistant professor, department of economics, Brooklyn College.

Nadav Halevi: visiting lecturer in economics, Wharton School, University of Pennsylvania.

James A. Hanson: assistant professor of economics, Brown University.

Donald J. Harris, University of California: instructor, department of economics, University of Illinois.

John R. Haskell: instructor in economics, Whittemore School of Business and Economics, University of New Hampshire.

Jack J. Hayden: assistant professor, School of Business, Indiana University, September 1965 to June 1968.

G. Helleiner: associate professor of economics, department of political economy, University of Toronto.

Heinz R. Heller: assistant professor, department of economics, University of California, Los Angeles.

Udo E. G. Heyn: lecturer in economics, Indiana University.

David K. Hildebrand: lecturer in statistics and operations research, Wharton School, University of Pennsylvania.

William Hildred, University of Texas: instructor, department of economics, St. Edwards University, Austin, fall, 1965.

Robert E. Hill, Kent State University: professor of economics and dean, School of Business, Southern Illinois University.

Yhi Min Ho: assistant professor, department of economics and finance, University of Houston.

Robert Holbrook: assistant professor, department of economics, University of Michigan.

William W. Hollister: visiting assistant professor of economics, Columbia University, 1965-66.

William P. Huenefeld: lecturer in economics, Yale University.

Jack Hulbert: visiting instructor in business education and office management, School of Business, Indiana University, 1965-67.

Ronald C. Horn: associate professor of business administration, University of South Carolina.

Glen W. Herrin: assistant professor of accounting, School of Business Administration, University of South Carolina.

Louis C. Jacoby: instructor in economics, St. Olaf College.

Robert W. Johnson: professor of industrial administration, Krannert Graduate School of Industrial Administration, Purdue University.

Shirley Johnson: assistant professor of economics, New York University, Washington Square College.

Eric L. Jones: visiting associate professor of economics, Krannert Graduate School of Industrial Administration, Purdue University.

George M. Jones, Joint Legislative Budget Committee: economic systems analyst, Matson Research Corporation, San Francisco.

Pamela Haddy Kacser: associate professor, American University.

Alfred L. Kahl: assistant professor of finance, College of Business Administration, University of Georgia.

Richard J. Kalish: assistant professor of political economy, Graduate School of Public Affairs, State University of New York.

Nake M. Kamrany, Battelle Memorial Institute: planning directorate, System Development Corporation, and lecturer in economics, University of California, Los Angeles.

Lawrence J. Kaplan, New York City Department of Relocation: associate professor of economics, College of Police Science, City University of New York.

K. William Kapp, Brooklyn College, retired: University of Basel, Switzerland.

Harold Katz: instructor in economics, New York University, Washington Square College.

Donald W. Katzner: lecturer in economics, Wharton School, University of Pennsylvania.

Paul Kelch: assistant professor of economics, State University of Wisconsin.

Michael Keren: acting instructor in economics, Yale University.

J. Daniel Khazzoom: assistant professor of economics, New York University, University College.

Billy F. Kiker: assistant professor of economics, School of Business Administration, University of South Carolina.

Sung J. Kim: lecturer in regional science, Wharton School, University of Pennsylvania.

Robert W. Knapp, General Motors Corporation: assistant professor of business economics, University of California, Los Angeles.

Hugh W. Knox: lecturer in economics and city planning, University of North Carolina.

Peter H. Knutson: lecturer in accounting, Wharton School, University of Pennsylvania.

Stephen J. Kohn: lecturer in economics, Queens College, City University of New York.

Richard F. Kosobud: associate professor, department of economics, Wayne State University.

Marvin H. Kosters: economics department, RAND Corporation.

Ching-Wen Kwang, State University of New York at Buffalo: faculty, California State College, Hayward.

Richard La Barge: associate professor, department of economics, Florida State University.

Donald M. Lamberton: visiting professor, Graduate School of Business, University of Pittsburgh.

Kelvin Lancaster: visiting professor, department of economics, City University of New York.

William M. Landes: assistant professor of economics, Stanford University.

Michael A. Lebowitz: assistant professor of economics, Simon Fraser University, Vancouver, Canada.

Norman B. Lefton, University of Chicago: acting assistant professor of economics, University of Hawaii.

Robert Lekachman: professor and chairman, department of economics, State University of New York.

E. J. Leverett: assistant professor of insurance, College of Business Administration, University of Georgia.

Russell Duene Lloyd, Bureau of Land Management, U.S. Department of the Interior: assistant director of Forest and Range Experiment Station, Colorado State University.

D. Phillip Locklin: visiting professor of economics, University of Florida.

Eugene L. Loren: acting assistant professor, department of economics, University of California, Los Angeles.

Robert M. Lovejoy: assistant professor of economics, State University of New York at Binghamton, Harpur College.

Kenneth S. Lyon: assistant professor of economics, College of Economics and Business, Washington State University.

Will Lyons: visiting associate professor of economics, Amherst College.

Ronan MacDonald: associate professor of economics, Long Island University, C. W. Post College.

Ruth Mack: adjunct visiting professor, department of economics, City University of New York, 1965-66.

C. Duncan MacRae: assistant professor of economics, Massachusetts Institute of Technology.

Ralph A. Maggio: assistant professor of industrial management, School of Industrial Management, Purdue University.

Ruth Mahr: acting assistant professor of economics, Cornell University, fall, 1965.

Gail Makinen: instructor, department of economics, Wayne State University.

Michael S. Malone, University of Kansas: assistant professor of economics, Washington and Jefferson College.

Donald R. Market: assistant professor, department of economics, Texas Christian University.

Zeno Martin: assistant professor of economics, School of Business Administration, University of South Carolina.

Joseph R. Mason: instructor, department of economics, Boston University, 1965-66.

Hirofumi Matsusaki: assistant professor of business and economics, University of Maine.

John W. Matthews: associate professor of commerce, Simon Fraser University, Vancouver, Canada.

Peter Max, Battelle Memorial Institute: Senior consultant, National Economic Research Associates, Washington, D.C.

Thomas H. Mayor, Brookings Institution: assistant professor of economics, University of Maryland.

Gary L. McDowell: assistant professor of economics, Dartmouth College.

Rose McElhattan: instructor in economics, School of Business Administration, Temple University.

Suresh J. Mehta: visiting lecturer in industry, Wharton School, University of Pennsylvania.

G. Hartley Mellish: assistant professor of economics, University of South Florida.

Gary K. Michael: assistant professor, department of commerce and economics, University of Vermont.

William H. Miernyk, Regional Research Institute: professor of economics, College of Commerce, West Virginia University.

James R. Millar, Cornell University: assistant professor of economics, University of Illinois.

H. Laurence Miller, J.: Wadsworth A. Williams professor of economics, Carleton College.

L. Charles Miller, Jr.: acting instructor in economics, Yale University.

Robert Miller: assistant professor of marketing, department of business administration, University of Iowa.

Edward J. Mitchell: economics department, RAND Corporation.

Wayne L. Mock: assistant professor of business, Graduate School of Business, Columbia University.

John Moes: associate professor, department of economics, Florida State University.

Khan A. Mohabbat, State University of New York at Buffalo: assistant professor of economics, Rutgers—The State University.

Thomas G. Moore, Carnegie Institute of Technology: associate professor, department of economics, Michigan State University.

Donald Moriary: instructor in economics, St. Louis University.

George R. Morrison: associate professor, department of finance, State University of New York at Buffalo.

Dean W. Morse: assistant professor, department of economics, Columbia University, 1965-66.

Dennis C. Mueller: assistant professor of economics, Simon Fraser University, Vancouver, Canada.

Willard F. Mueller, Bureau of Economics, Federal Trade Commission: Visiting professor, department of economics, Michigan State University, fall, 1965.

Mian A. H. Naz, Department of Planning, Baltimore: assistant professor of economics, Morgan State College.

William G. Nelson, IV: assistant professor of economics, Washington University, St. Louis.

Marc Nerlove: professor of economics, Yale University.

Egon Neuberger: visiting professor, department of economics, University of Michigan, 1965-66.

Raymond P. Neveu, University of Pittsburgh: assistant professor, School of Business, Indiana State University.

Donald A. Nichols: acting instructor in economics, Yale University.

Karl H. Niebyl: visiting professor of economics, School of Business Administration, Temple University.

Roger Noll, Harvard University: instructor in economics, California Institute of Technology.

David Novack: assistant professor of economics, Vassar College.

D. M. Nowlan: lecturer in economics, department of political economy, University of Toronto.

Richard V. Nuttall, Jr.: associate professor of management, School of Business Administration, University of South Carolina.

Jeremiah J. O'Connoll: assistant professor of industry, University of Pennsylvania.

David A. Olson: assistant professor of economics, Smith College.

June O'Neill: instructor in economics, School of Business Administration, Temple University.

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Lucian Palmer: instructor, department of economics, University of Southwestern Louisiana.

Richard W. Parks: assistant professor of economics, University of Chicago.

Sarah C. Peters, Yale University: acting assistant professor, department of economics, University of Washington.

Robert L. Peterson: visiting lecturer in economics, University of Montana, 1965-66.

Richard H. Pettway: assistant professor, department of accounting and financial administration, Michigan State University.

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Wallace E. Reed, University of Chicago: assistant professor, department of economics, Duke University, 1965-66.

William Reher, University of Iowa: faculty, Texas State College, Arlington.

John G. Rice: lecturer in regional science, Wharton School, University of Pennsylvania.

Franz Ritzmann: lecturer in economics, Yale University.

Lee P. Robbins: assistant professor of economics, Franklin and Marshall College.

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Robert J. Saunders: assistant professor of economics, College of Commerce, West Virginia University, September 1, 1965.

Jeffrey M. Schaefer: instructor in economics, Rutgers—The State University, 1965-66.

William M. Scammell: professor of economics, Simon Fraser University, Vancouver, Canada.

Sayre P. Schatz: visiting professor of economics, department of economics, Columbia University, 1965-66.

Dan E. Schendel: visiting assistant professor of industrial management, School of Industrial Management, Purdue University.

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Fred F. Schiller, New York University: assistant professor of economics, University of Vermont.

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James H. Schulz: instructor in economics, Whittemore School, University of New Hampshire.

Barney Schwalberg: assistant professor, department of economics, Brandeis University, 1965-66.

Lawrence E. Schwartz: assistant professor of economics, Queens College of the City University of New York.

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John Shaw: assistant professor of economics, Fresno State College.

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John C. Shearer: associate professor of economics, Pennsylvania State University.

Craig Sherbrooke: logistics department, RAND Corporation.

Don R. Sheriff: associate professor and director, Bureau of Labor and Management, University of Iowa.

Hirofumi Shibata: assistant professor of economics, Queen's University at Kingston.

Irving H. Siegel: senior research staff, W. E. Upjohn Institute, Washington, D.C.

Kilman Shin: assistant professor of economics, Denison University.

Arlene Silvers: instructor in economics, School of Business Administration, Temple University.

Hans Singer: visiting professor of economics, City University of New York, fall, 1965.

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Jack L. Smith: assistant professor of business and economics, University of Maine, Portland.

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James D. Smith: assistant professor, American University.

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Gordon R. Sparks: assistant professor of economics, Massachusetts Institute of Technology.

Ronald L. Sparling: faculty lecturer, School of Business, Indiana University, 1965-66.

Michael H. Spiro: assistant professor of business administration, Graduate School of Business, University of Pittsburgh.

Jack C. Stabler: assistant professor of economics, University of Saskatchewan.

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Louis Stern: associate professor, department of economics and finance, University of Houston.

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David W. Stevens: assistant professor of economics, Pennsylvania State University.

Thomas M. Stevenson, Jr., Ball State College: assistant professor, department of economics, St. Louis University.

William J. Stober: assistant professor of economics, Louisiana State University.

Ernst W. Stormsdorfer: assistant professor of economics, Pennsylvania State University.

Gerhard Stuvel: visiting professor of economics, Wharton School, University of Pennsylvania.

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Anita Arrow Summers: lecturer in economics, Swarthmore College.

Boris C. Swerling: professorial lecturer in economics, Georgetown University.

Richard J. Swersey: lecturer in industry, Wharton School, University of Pennsylvania.

Edwin F. Terry: assistant professor of economics, State University of New York.

Earl A. Thompson: assistant professor, department of economics, University of California, Los Angeles.

Walter F. Thompson: associate professor of business, Grove City College, September 1965.

Andris Trapans: logistics department, RAND Corporation.

Harry G. Trend, Wayne State University: economic analyst, Radio Free Europe, Munich.

William J. Truitt: assistant professor of economics, Louisiana State University.

Bernard Udis: associate professor and director, Bureau of Economic Research, University of Colorado.

Hiroya Ueno: visiting professor of economics and regional science, Wharton School, University of Pennsylvania.

Donald R. Van Houten: assistant professor of business administration, School of Business, Indiana University.

Meenakshisunder Venkatesan: assistant professor of business administration. Whittemore School, University of New Hampshire.

Thomas Vietorisz: professor of economics, Graduate Faculty, New School for Social Research.

Jose Villamil: lecturer in regional science, Wharton School, University of Pennsylvania.

Festus J. Viser, New York University: professor of economics and chairman, department of economics, Memphis State University.

Robert C. Vogel: lecturer in economics, Wesleyan University.

Theodore Voyadjis: instructor in economics, School of Business Administration, State University of New York at Buffalo.

John E. Walder, Jr., Research Triangle Institute: research director, Investment Bankers Association of America.

John Walker, University of Utah: instructor in economics, Idaho State University.

Kenneth F. Wallis: lecturer in economics, Yale University.

Paul A. Weinstein, Columbia University: associate professor of economics, University of Maryland.

Doyle L. Weiss: assistant professor of industrial management, School of Industrial Management, Purdue University.

Donald R. Wells: assistant professor of economic research, University of Mississippi.

Lester C. Westbrook: instructor in accounting, College of Business Administration, University of Georgia.

Fred M. Westfield, Northwestern University: professor of economics, Vanderbilt University.

Janet M. Whisler: instructor in economics, Beloit College.

William J. Whitmore: instructor, department of business administration, University of Kentucky.

B. J. Widick: assistant professor, department of economics, Wayne State University.

James A. Wilde: lecturer in economics, University of North Carolina.

G. Dee Willis: instructor in accounting, College of Business Administration, University of Georgia.

Robin D. Willits: associate professor of business administration, Whittemore School of Business and Economics, University of New Hampshire.

Clifford Wilson: instructor, department of business administration, University of Kentucky.

Rufus Wixon, Wharton School, University of Pennsylvania: professor of accounting, University of Florida.

'er G. Wood, Jr.: assistant professor of economics, School of Business Administration, rsity of South Carolina.

o Yamane: visiting lecturer in economics, New York University, Graduate School ts & Science.

ou-Shuang Yan: assistant professor of economics, Haverford College.

cy B. Yeargan: professor of accounting, College of Business Administration, Univer- of Georgia.

hard D. Young, Stanford University: associate professor of economics, Rice Univer-

derick A. Zeller, West Virginia Center for Appalachian Studies and Development: ate professor of economics, College of Commerce, West Virginia University.

ıald H. Zoll: instructor in management, College of Business Administration, Uni- y of Georgia.

Leaves for Special Appointments

ımis Abdul-Magid, College of William and Mary: economic advisor, Ministry of ce and Industry, Kuwait, 1965-66.

nk M. Bass, Purdue University: Lucas visiting professor in engineering production, rsity of Birmingham, England.

ry J. Benninger, University of Florida: Escuela de Administración y Finanzas, llin, Colombia.

hur M. Borak, University of Minnesota: visiting professor of economics, Khartoum, ı.

ın Bowman, University of Illinois: visiting assistant professor, department of eco- s, University of Wisconsin, 1964-65.

orge E. P. Box, University of Wisconsin: Ford Foundation visiting professor, ıate School of Business Administration, Harvard University, 1965-66.

ıl Brinker, University of Oklahoma: visiting professor of economics, University of Mexico, fall 1965-66.

ight S. Brothers, Rice University: visiting professor of economics, Graduate School ısiness Administration, Harvard University, 1965-66.

ın A. Buttrick, University of Minnesota: director, AID Project, Bogotá, Colombia, 965.

bster C. Cash, University College of Rhodesia and Nyasaland: associate professor onomics, Florida State University.

rgan Collins, University of Michigan: visiting professor of economics, Harvard rsity Graduate School of Business Administration, 1965-66.

hard N. Cooper, Yale University: Deputy Assistant Secretary of State for Economic s.

wrence L. Crum, University of Florida: University of Texas.

k F. Culley, University of Iowa: dean, College of Liberal and Fine Arts, University beria, 1965-67.

win R. Dean, Columbia University: Nigerian Institute of Special and Economic rch, University of Ibadan, 1965-66.

ın W. Dorsey, University of Maryland: staff economist, President's Council of omic Advisers, 1965-66.

gniew M. Fallenbuchl, University of Windsor: adviser to the Government of h Guiana and professor, University of Guyana, 1965-66.

ıl E. Fenlon, Colorado State University: staff of American Association of University ssors, Washington, D.C.

ıis Fier, Brooklyn College: Fulbright professor, Faculté de Droit et des Sciences omiques, University of Nice, France, 1965-66.

George Fisk, Wharton School, University of Pennsylvania: guest professor, University of Lund, Sweden.

Lehman B. Fletcher, Iowa State University: National School of Agriculture, Chapingo, Mexico, 15 months.

Hans W. Gerhard, Duke University: visiting professor, department of economics, University of Wisconsin-Milwaukee, spring 1966.

George D. Green, Stanford University: visiting assistant professor of economics, University of Minnesota.

Jack M. Guttentag, Wharton School, University of Pennsylvania: National Bureau of Economic Research.

Daniel Hamberg, University of Buffalo: visiting Fulbright lecturer, Bologna (Italy) Center of Advanced International Studies, Johns Hopkins University.

C. Lowell Harriss, Columbia University: visiting professor, Princeton University, spring, 1966.

John C. House, University of Minnesota: Institute for Defense Analysis, Washington, D.C., spring 1966.

William W. Haynes, Indian Institute of Management, Ahmedabad: visiting professor, Harvard Business School, 1965-66.

George Heitmann, Pennsylvania State University: Ministry of Planning and Development, Government of Libya, 1965-66.

James G. Hilton, University of South Carolina: Fulbright lecturer, Norway.

Ralph H. Hofmeister, University of Minnesota: assistant professor, AID program, Bogota, Colombia, 1965.

Leonid Hurwicz, University of Miinnesota: visiting lecturer, University of Bangalore. India.

Manley R. Irwin, Whittemore School of Business and Economics, University of New Hampshire: staff, Federal Communications Commission Common Carrier Bureau.

Edward B. Jakubauskas, Iowa State University: director, Iowa Manpower Council.

Subbiah Kannappan, Michigan State University: faculty adviser, International Institute for Labour Studies, Geneva, 1965-66.

Maurice D. Kilbridge, University of Chicago: Ford Foundation visiting professor. Graduate School of Business Administration, Harvard University, 1965-66.

Lawrence Klein, University of Pennsylvania: visiting professor, Princeton University.

Leonard J. Lonopa, University of Iowa: Kent State University, 1965-66.

Lester Lave, Carnegie Institute of Technology: assistant professor of economics, Northwestern University, 1965-66.

Paul W. MacAvoy, Massachusetts Institute of Technology: staff member, Council of Economic Advisers, 1965-66.

George Macesich, Florida State University: Fulbright fellow, University of Belgrade, 1965-66.

Jesse W. Markham, Princeton University: Ford visiting professor of economics, Graduate School of Business Administration, Harvard University, 1965-66.

Benton F. Massell, RAND Corporation: director of economic research, University College, Nairobi, Kenya, until December 1966.

W. David Maxwell, Tulane University: adviser to the universities, Bangkok, Thailand, 1965-66.

E. Scott Maynes, University of Minnesota: economic consultant, Instituto Di Tella, Centro de Investigacionas Economica, Buenos Aires, Argentina.

J. Sayer Minas, Wharton School, University of Pennsylvania: continuing at Waterloo College, Ontario, 1965-66.

Paul A. Montavon, University of Notre Dame: economic adviser to the A.I.D. Mission, Quito, Ecuador, 1965-67.

Robert Mundell, Brookings Institution: Ford Foundation visiting professor of economics, University of Chicago, 1965-66.

Lee C. Nehrt, Indiana University: adviser, Ministry of Planning and Economic Affairs in Tunisia, 1965-67.

Egon Neuberger, RAND Corporation: visiting professor of economics, University of Michigan, 1965-66.

Monroe Newman, Pennsylvania State University: chief economist, Appalachian Regional Commission, 1965-66.

Arnold A. Paulsen, Iowa State University: consultant, U. S. Department of Agriculture, Washington, D.C., 1965-66.

Jonas Prager, University College, New York University: assistant professor, Bar-Ilan University and Bank of Israel, 1965-66.

Gustav Ranis, Yale University: assistant administrator for Program Coordination, Agency for International Development.

Warren Robinson, Pennsylvania State University: chief of the demographic research section, Institute of Development Economics, Karachi, 1965-66.

Jerome Rothenberg, Northwestern University: Ford Foundation faculty research fellow, Oxford University, 1965-66.

Ryuzo Sato, University of Hawaii: visiting professor of economics, Brown University, 1965-66.

Stanley Schor, Wharton School, University of Pennsylvania: Director of Statistics, American Medical Association, Chicago, 1965-66.

Charles L. Schultze, University of Maryland: director, U. S. Bureau of the Budget.

Harry Schwartz, *New York Times:* visiting lecturer, Princeton University, spring 1966.

Martin Segal, Dartmouth College: staff, President's Council of Economic Advisers, 1965-66.

Ezzedin M. Shamsedin, University of South Carolina: Fulbright lecturer in Iraq.

William M. Shenkel, University of Florida: College of Business Administration, University of Georgia.

James A. Storer, Bowdoin College: assistant to the director for economics, Bureau of Commercial Fisheries, U. S. Department of the Interior, 1965-66.

George Strauss, University of California, Berkeley: visiting professor of business administration, State University of New York at Buffalo.

Gerald E. Thompson, University of Nebraska: visiting professor, department of economics, University of Michigan, 1965-66.

Richard H. Tilly, University of Michigan: visiting assistant professor of economics, Yale University.

Melville J. Ulmer, University of Maryland: Fulbright professor, Netherlands School of Economics, Rotterdam, 1965-66.

William F. Vendley, School of Industrial Management, Purdue University: visiting lecturer in accounting, College of the Virgin Islands.

Lewis E. Wagner, University of Iowa: Damascus, Syria, four months.

Neil Wallace, University of Minnesota: staff, logistics Department, RAND Corporation, 1965-66.

Clark L. Wilson, Batten, Barton, Durstine & Osborn, Inc.: Ford visiting professor of economics, Harvard Business School.

D. M. Winch, University of Alberta: visiting professor of economics, department of political economy, University of Toronto.

Resignations

John W. Bowman, University of Illinois.

Phillip D. Cagan, Brown University.

Kang Chao, University of California, Berkeley.
M. Umer Chapra, University of Kentucky.
Kuk Soo Chung, Vassar College.
Patrick de Fontenay, Vassar College.
Herbert Geyer, Tulane University.
William R. Hoskins, Indiana University.
Charles W. Howe, Purdue University.
Douglas N. Jones, U. S. Air Force Academy.
P. H. Kacser, Catholic University of America.
Ernest Koenigsberg, Wharton School, University of Pennsylvania.
Hendrik Konijn, City University of New York.
Louis Lefeber, Massachusetts Institute of Technology.
Clarence D. Long, Johns Hopkins University.
H. I. MacDonald, University of Toronto.
Russell H. Mack, Temple University.
Sally B. Maybury, University of Vermont.
Russell L. Moran, University of Illinois.
Peggy Musgrave, Wharton School, University of Pennsylvania.
Richard V. Nuttall, Jr. University of Pittsburgh.
H. Rose, University of Toronto.
Walt W. Rostow, Massachusetts Institute of Technology.
Bernard Sarachek, Rutgers—The State University.
Rosemary MacLellan Scanlon, Old Dominion College.
Harry G. Trend, Wayne State University.
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Peter J. D. Wiles, City University of New York.

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SUPPLEMENT

THE STRUCTURE OF ECONOMISTS' EMPLOYMENT AND SALARIES, 1964

COMMITTEE ON THE NATIONAL SCIENCE FOUNDATION REPORT ON THE ECONOMICS PROFESSION

Price $1.00

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THE STRUCTURE OF ECONOMISTS' EMPLOYMENT AND SALARIES, 1964

SUMMARY OF FINDINGS

Some 12,000 American economists are included in the National Register of Scientific and Technical Personnel, a program of the National Science Foundation carried out in cooperation with the American Economic Association and ten other professional societies. In 1964, 10,000 of these economists reported basic salaries on full-time professional work which ranged from less than $5,000 to more than $100,000. The arithmetic average of these salaries was $13,670, but 60 percent of them were less than that amount. Half of the salaries were above and half were below the median of $12,000. The largest concentration of economists' salaries, 11 percent, occurred within $500 of $10,000.

Definition of Economists. The criteria used to define an "economist" have a considerable influence on the numbers and the salaries of those who qualify. To meet the standards of the National Register, an economist as here defined had to be known to some professional society such as the American Economic Association, to respond to the NSF questionnaire, to report an earned Master's degree or equivalent experience, and to designate some field of economics as his field of greatest special competence. In contrast, the classification standards of the Census of Occupations are much less restrictive, with the result that the Census of 1960 counted 22,000 "economists" who had median earnings of only $7,700 at that time. In the teaching segment of the economics profession, for which standards of qualification are most likely to agree, the 1960 Census median salary of $7,100 is reasonably consistent with the 1964 median of $9,700 of those on the National Register considering the general rise in salaries during the intervening period of time.

Salaries in the Economic and other Professions. The economists' median salary of $12,000, here reported, was the same as that of the physicists and the statisticians and was from $1,000 to $3,000 above those of any of the other nine professions surveyed for the National Register.* One-third of the economists were employed by industry or business at a median salary of $14,400, exceeding that of any of the other eleven professions by amounts ranging from $300 to $5,400. The Federal government employed one-tenth of the economists, as here de-

* The twelve professions included in the National Register were: economics, statistics, psychology, sociology, linguistics, physics, chemistry, mathematics, biology, meteorology, earth sciences, agriculture.

fined, and paid them a median salary of $13,700, which surpassed that of each of the other eleven professions on the National Register by amounts ranging from $700 to $4,400. Another 42 percent of the economists were employed by educational institutions (including all economists at those institutions whether engaged in research, administration, or teaching, and including those engaged for the 9-10 month academic year as well as those paid for a full calendar year). The median salary paid by educational institutions to economists was $10,100, which was lower by $100 to $400 than those paid to the professions of agriculture, statistics, and meteorology, but from $100 to $1,400 higher than the median salaries paid by educational institutions to the other eight professions. All of these are averages of basic sciences rather than gross incomes and they are influenced, of course, by the varying distributions of those in the different professions, not only by type of employer but also by other characteristics which are associated with differences in salary.

Characteristics Related to Salary Structure: Summary Evaluation. Seven characteristics of the economists were studied and each was found to have some measurable relationship to differences in their salaries. When each of these characteristics was considered separately, the relative salaries of individual economists were found to be most importantly related (in descending order) to: (1) years of professional experience, (2) primary work activity (teaching, research, administration, etc.), (3) age of the economist, and (4) the type of employing organization. Much smaller degrees of relationship were found between total salary variation and: (5) the particular economic field of greatest competence, (6) academic degree, and (7) sex. These may be called the *gross* relationships.

However, these seven characteristics also tended to be related to each other (intercorrelated). When the effect of these interrelationships between characteristics was eliminated (by a multiple regression technique) the most impotrant *net* relationships with differences in economists' salaries were (in descending order) with the characteristics of: (1 and 2) professional experience and type of employer (not separable because of interaction), (3) academic degree, and (4) primary work activity. Much smaller degrees of *net* relationship were found between salary and (5) age, (6) sex, and (7) particular economic field of greatest competence.

Both the gross and the net relationships between salaries and each of the selected characteristics of the economists are illustrated in Figure 1 (See Section V, pp. 66-67 of this report). The net relationships are obtained from a regression analysis that includes all seven of the characteristics listed above. These net regression coefficients, plotted

as the right-hand set of bars, show the net percentage salary differences associated with membership of an economist in each specified class.

"Level of highest degree" illustrates the difference between the gross and net relationships. Economists with no more than a Bachelor's degree received more than the average economists' salary, as is indicated along the middle line of bars in Figure 1. However, it was *not* better to have a Bachelor's degree rather than a Ph.D., as becomes clear when allowance is made for the other factors related to salary. Thus the *net* relationship of possession of a Bachelor's degree to salary was negative (right-hand set of bars), even though the *gross* relationship (middle set of bars) was a positive one.

"Sex of respondent" illustrates the impact of the relative number of economists on the overall salary level. The women economists received lower salaries than the men in, for example, nearly every field of special competence and, indeed, the net (adverse) relationship to salary of being a woman was almost as great as the gross relationship. Nevertheless, the difference in sex was of relatively slight importance to the salary level of economists as a whole, because of the very small proportion of women (left-hand bar in Figure 1) among the whole group of economists as here defined.

Professional Experience. Economists at the beginning of their professional careers (with less than 2 years' experience) had an average (median) salary of $7,800, while those with 20 or more years of experience had salaries averaging $16,000. The universal rule was that the larger the number of years of professional experience (at least up to 20 years or more), the higher was the economists' average salary. This rule applied, not only as a general tendency, but also for the economists with each combination of academic degree and type of employer. Taking all the economists together, the additional salary associated with one year of additional experience was about $400 or 4.8 percent for those who already had 5 years' experience. The incremental salary value of experience then declined in regular steps, so that those with 20 years' experience had an advantage of about $250 or 1.9 percent per year of additional experience over those with 15-19 years' experience. Between the different types of employers, the *rate* of salary increment per year of additional experience varied considerably. The Federal government granted the highest rate of salary increase per year of experience (6.3 percent) during the first 4 years, but the lowest rate (1.6 percent per year) after its economists had had 20 years of experience. Educational institutions provided the lowest rate of annual increments up to 14 years (2.6 to 3.3 percent per year) and thereafter rates (1.9 to 1.7 percent) which were only slightly higher than those of

the Federal government. Industrial and business employers had the highest incremental rates for all those beyond 4 years' experience, averaging more than 5 percent per year up to 14 years' experience and nearly 3 percent per year for those with longer experience.

Academic Degree. Taking all types of employment together, the economists with a Ph.D. degree had an average (median) salary of $12,100; those with a Master's degree, $11,000; those with a Bachelor's degree, $13,500; and the few (60) with less than a Bachelor's degree, $16,700(!) The chief explanation of this apparent inversion of expected relationships is that industry and business, paying the highest salaries to economists at all levels of academic attainment, also employed three-quarters of those with a Bachelor's degree or less, while educational institutions, paying the lowest salaries, employed 72 percent of the Ph.D.'s. A secondary explanation is that the industrial employees who did not have a higher degree consisted predominantly of economists with by far the largest numbers of years of professional experience. In each separate type of employment, the expected relationship did exist between level of degree and salary. Thus the Ph.D.'s had markedly higher salaries than the economists with the lesser academic degrees even within industry, and at similar levels of experience the industrial and business employers generally paid the Master's higher salaries than the Bachelor's.

Work Activity. An average (median) salary of $16,100 was paid to those economists who were primarily engaged in management or administrative activities, a group which comprised 30 percent of all the economists on the National Register. Nearly one-third of the economists were engaged in teaching and three-fourths of the salaries of the teachers were paid for in an academic year of 9 to 10 months. The median salary of the teaching economists, whether for an academic or calendar year was only $9,700. Economists primarily engaged in research had a median salary of $11,400 while those in production activities were paid an average (median) $11,700. The exceptionally low average salary of the teachers does reflect, in part, the effect of their shorter working year, but even after adjustment is made for differences in working time, the basic salaries of teachers are still less than that of economists in any other broad class of work activity. The relatively low compensation of the teachers is especially noteworthy in view of the high proportion of Ph.D.'s in teaching. A special tabulation of gross *incomes* (including consultation fees, extra summer pay, etc.) showed that, even after these additions to basic salary, those economists engaged in teaching still had lower average incomes than those in any other major class of work activity. The teaching economists did earn nearly as much as those engaged in research and development, but

they earned one-third less than those primarily engaged in management or administrative activity.

Type of Employer. Notice has already been taken of the sharp contrast between averages of $14,400 paid by industry, of $13,700 paid by the Federal government, and of $10,100 paid by educational institutions. The different types of employer, however, did not employ in equal proportions economists who had the same experience and academic degree to do the same class of work, for the same number of months per year. When account is taken of these related differences, the net relationship of type of employer to differences in salary, although not clearly separable, appears somewhat less important than the more obvious gross relationship.

Age. The economists surveyed who were less than 25 years old had a median full-time salary of $7,800. Average salaries were successively higher at greater ages up to between 50 and 64 years of age where a salary plateau of $15,000 was reached. For economists over 65 years of age, average salaries were below this plateau, those of 70 years or more having an average salary of $12,000. The net relationship between age and salary was relatively small, because of the somewhat parallel relationship between age and the more uniform and persistent factor of the number of years of professional experience. At any given age level, salaries varied considerably according to experience, but at any given experience level age had only a minor effect on salaries.

Sex. The very small number of women among the economists surveyed had a median salary of $9,900, in contrast to the men's median of $12,000. Moreover the women received lower salaries than men in every economic field of special competence except the field of Population, Welfare programs, and Standards of Living, where the women's salaries averaged $12,000 compared with $11,500 for the men. The patterns of experience, age, and other characteristics were so different for women than for men as to preclude any very precise salary comparisons between the sexes—especially in view of the small number of observations available for the women economists. The multiple regression analysis, however, confirmed the existence of significant salary differences between men and women.

Fields of Special Competence. The economists who stated that their education and experience had given them their greatest special competence in the more business-oriented fields of economics had average salaries of $13,000, whereas those most competent in economic history averaged only $9,800. However, when the differing years of experience and distributions by type of employer are taken into consideration, the field of specialization within economics appears to have had a relatively minor net relation to levels of salary. The special field of an econo-

mist's training does appear to be related to his chances of obtaining and retaining a job with the type of employer which pays the higher or the lower general levels of salary, but economists with the same length of experience and performing similar functions for any given type of employer are not likely to find that their particular fields of special competence will be importantly related to the average salary they receive.

COMMITTEE ON THE NSF REPORT ON THE PROFESSION

N. ARNOLD TOLLES
New York State School of Industrial and Labor Relations, Cornell University

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Washington University, St. Louis

EWAN CLAGUE
Commissioner of Labor Statistics U. S. Department of Labor

FOREWORD

In 1964, the economics profession was included for the first time as part of the National Register of Scientific and Technical Personnel of the National Science Foundation. Accordingly, the American Economic Association was one of eleven professional societies[1] which cooperated with the Foundation in distributing and editing a questionnaire mailed to individuals in a broad range of the physical and social science professions. The replies to this questionnaire, mailed during the second quarter of 1964, provide a new, reasonably up-to-date, and much deeper source of information than has been available on the number, characteristics, and compensation of economists throughout the United States.[2]

The National Science Foundation itself has released a brief report on the total 223,854 scientists registered in 1964,[3] including 12,143 economists, and it will shortly publish a new edition of its *American Science Manpower* covering the 1964 data for each of twelve broad areas, including the economics area as a whole.[4] However, it is the policy of the Foundation to encourage each of the cooperating professional societies to analyze the detailed data pertaining to their own professions, especially those parts of the data pertaining to the structure of salaries. Accordingly, the American Economic Association appointed the undersigned as a committee to analyze and report on the NSF information relating to economists.

This Committee published its brief initial report[5] to call attention to the new data from the National Register and to invite suggestions for the types of analysis that AEA members might particularly desire. The most fruitful of the resulting suggestions proved to be that of Dr. Emanuel Melichar, Economist, Board of Governors, Federal Reserve

[1] The other ten cooperating organizations were: American Chemical Society, American Geological Institute, American Institute of Biological Sciences, American Institute of Physics, American Mathematical Society, American Meteorological Society, American Psychological Association, American Sociological Association, Federation of American Societies for Experimental Biology, and the Center for Applied Linguistics of the Modern Language Association of America.

[2] A list of the tabulable items covered by the 1964 questionnaire was appended to the first preliminary report of this Committee, *Papers and Proceedings of the Seventy-seventh Annual Meeting of the American Economic Association . . . American Economic Review*, Vol. LV, No. 2, May 1965, p. 633.

[3] "Salaries and Professional Characteristics of U. S. Scientists, 1964." *Reviews of Data on Science Manpower*, NSF 64-27, Vol. 1, No. 2, December 1964.

[4] The 1962 data were published as NSF 64-16, July 1964, but these 1962 data were limited to nine fields of science (214,900 returns).

[5] *American Economic Review, op. cit.*, pp. 629-633.

System. Dr. Melichar was persuaded to carry out his own suggestion, and to draft what became Section V of this report.[6]

Section I of this report describes the rather complex definition of the economics profession and the resulting general characteristics of the group of economists whose salaries are analyzed here. To avoid repetition, formal tables have been omitted from Section I, since these may be found in subsequent sections as well as in an Appendix. Section II presents the general level of economists salaries in 1964 according to the data available to the Committee and compares these results with alternative measurements of economists' compensation and with the salaries of the other professions surveyed by NSF. The principal emphasis of the present report is on the structure of salaries within the economics profession, a subject which has been explored only sketchily in the past and one to which the National Science Foundation requested the AEA to give specific attention. Section III presents the detailed data and Section V provides a summary of the relative importance of the principal characteristics of economists as related to their salaries. Although sex was one of the seven characteristics included in the regression analysis of salaries as presented in Section V, the number of women economists was too small to make this characteristic a significant one in relation to differences in salaries among economists as a whole. In view of the unique patterns of women's employment and compensation, the available data on the women economists has been developed separately in Section IV of this report, rather than as one of the dimensions of economists' salary structure presented in Section III. The Appendix contains a series of tables to supplement the tabular material in the text. The Technical Note, preceding the

[6] Mrs. Alice Hanson Jones, Washington University, in addition to her general service on the Committee, acted as a special consultant to the chairman on numerous technical matters and prepared the draft of Section IV, on Women Economists. Mr. Ewan Clague, Commissioner of Labor Statistics, U. S. Department of Labor, was the chief architect of the Committee's general plan of analysis and he gave the draft reports his experienced, critical attention. Professor Harold Williamson, Secretary-Treasurer of the AEA, had managed the original cooperation between AEA and NSF and his advice was essential at all stages of the Committee's work, including the authorization and financing of that work by the Executive Committee of AEA. Professor N. Arnold Tolles, Committee chairman, carried out the analysis of the detailed tabulations as received from NSF and drafted most of this report, subject to Committee revision and approval. Most of the work on the present report had to be done during the six weeks following July 31, 1965, when the basic salary tabulations were first received. This work was carried out at the New York State School of Industrial and Labor Relations at Cornell University, with the approval of Dean David G. Moore, and with the irreplaceable assistance of Mr. T. J. Walker, Mr. Ross Azevedo, and the chairman's secretary, Mrs. Joan Monson. Essential to the entire project was the cooperation and consultation enthusiastically provided by Dr. Milton Levine, Director, and Mr. J. James Brown, Associate Director, National Register of Scientific and Technical Personnel Studies Group, National Science Foundation.

Appendix, is intended to assist the reader in interpreting the precise usage of words in the text and tables.

Despite its length, this report contains an analysis of only a small fraction of the data which will ultimately be provided under the existing tabulation plans of the National Science Foundation. Because available data for 1964 could not be processed in time for publication in 1965, this report is almost entirely based on only one of the 25 sets of tabulations which NSF had planned before this Committee started its work. As a result, there is, for example, a seriously inadequate analysis of the academic-year salaries of teachers, of the salaries of members of the American Economic Association, and of salary differences based on different work activities. There is no analysis whatever of gross incomes (as distinct from basic salaries), or of either the salaries or incomes of part-time employees. The fact that all the processed salary data received from NSF were in the form of pre-computed medians, deciles, and quartiles (rather than listings or distributions by intervals) saved the Committee much computing time but also estopped any definitive combinations or breakdowns of the data as received. For example, in Section V where the highly significant factor of length of work experience is discussed, nearly one-third of the economists appeared in the open-end interval of 20 or more years of experience, but it was not possible, within the time available, to break this interval into meaningful segments. Thus it was not fully possible to test the relative importance of age versus experience as factors differentiating salaries. In these matters, as in many others, the Committee was faced with a hard choice between a superior but indefinitely-delayed report, and one based on partial data which still could be published by December 1965. It chose the latter course.

Future analysts may well turn their attention to data already obtained for the 1964 Register which were not available to the Committee, because even the massive tabulation plans of the NSF had not provided for them. One example of this kind of potential for future research is revealed by the somewhat inconclusive findings in Section II-2, of this report as to whether and to what extent the economists' salaries are *really* superior to those of the other professions, *caeteris paribus*. Answers should be sought by subjecting the salary data for each of the professions to a simultaneous multivariate analysis of the kind which has been attempted here only for the economics profession itself. Even more informative would be the discovery of the salaries (and earnings) which would emerge if different definitions of "economist" were used in place of the special one which has served as the sole basis for all NSF tabulations thus far. Responses to the National Register questionnaire (items 6, 7, 12, 18) provide a ready-made basis for

at least five alternative definitions of the profession (See Section I-1 below), and some readers may believe that one or more of these alternatives would be preferable. The effect of alternative classifications could not be explored, because any timely report had to be confined to data obtained under the basic classification system adopted by NSF before this Committee began its work.

Even broader questions might be raised as to the coverage, the scope, and the detailed items and instructions used in collecting the National Register data, but we have not considered such questions to be within our terms of reference.

These and other limitations of the data should not be allowed to obscure a proper appreciation of the unique scope and depth of information on the professions which has been provided by the National Register. This Committee is honored by its opportunity to analyze these data in an attempt to make some contribution to a better understanding of the current status of the economics profession and, perhaps, to provide a basis for future studies.

N. ARNOLD TOLLES, *Chairman*
ALICE HANSON JONES
EWAN CLAGUE

THE STRUCTURE OF ECONOMISTS' EMPLOYMENT AND SALARIES, 1964

I. *Definition, Composition, and Employment of Economists*

1. *Definition and Gross Numbers*

The National Science Foundation classified 12,143 persons as economists from its National Register for 1964,[7] whereas the Bureau of the Census had counted 22,424 economists as part of the "experienced civilian labor force" of 1959.[8] This difference of more than 10,000 persons does not appear to reflect any decline in the number of economists between 1959 and 1964, but rather to result from different standards for defining the occupational group of "economists" and for judging whether particular individuals should be included within the somewhat elastic boundaries of the profession of economics.

The Census method of occupational classification starts with an informal job title supplied during an interview between a census enumerator and a member of the household of each person who was employed at the time of enumeration, or who had been employed and appeared to be currently available for employment. (A 5 percent sample of the population is used.) The name of each job is then used as the basis for a central office classification of the entire national labor force as far as possible, into one of 297 standardized occupational titles. This method results in a very inclusive count of "economists," not only because of the Census attempts to reach a sample of every classifiable person in the United States, but also because no minimum standards of education, extent of experience, proficiency, or attachment to the profession are used to refine the brief and sometimes inflated job titles initially supplied to the census emumerators.[9]

The basic purpose of the National Register is to locate (and thus make potentially available to meet urgent national needs) those persons who are expert in each particular scientific field. This purpose

[7] National Science Foundation, "Salaries and Professional Characteristics of U.S. Scientists, 1964," *Reviews of Data on Science Resources.* NSF 64-27, Vol. I, No. 2, December 1964. See Appendix Tables A and B, for total number of registered economists, classified by years of professional experience, field of specialization, academic degree and type of employer.

[8] Total of 3,605 "professors and instructors" of economics and 18,819 scientists listed under "economics" in the broad category of "social scientists." U.S. Bureau of the Census, *U.S. Census of Population: 1960. Subject Reports. Occupational Characteristics.* Final Report PC (2)-7A, Table 1, pp. 1, 2.

[9] For the condensed official description of the relevant Census procedures, cf. Census, *op. cit.*, pp. vi-ix.

differs from that of the Census which is to count each member of the entire civilian labor force and to allocate him to an occupational category which suggests better than any other of a limited list of occupations the character of his most recent job. It should not be surprising that the collection of experts in the various special fields of economics as contained in the National Register should be only about half as numerous (12,143 in 1964) as those (22,424 in 1959) for whom the census title "economist" (or "college teacher—economics") merely suggests the character of their most recent jobs better than any alternative Census title.

To qualify as a member of any of the twelve professions covered by the 1964 National Register, a person needed to be actively associated with a relevant professional society, to respond to the NSF questionnaire, and to *meet a minimum standard of educational attainment* or professional experience. To be classified as an economist, a person meeting the above qualifications needed to designate as the field of his greatest professional competence one of the special subjects which had been prelisted under "Economics." This general statement may now be supplemented by some details of the National Register operation to indicate the effect of these professional standards on the number and characteristics of the 12,143 persons considered to be economists for the purpose of the NSF tabulations analyzed in this report.

In 1964, a total of 443,000 questionnaires were mailed to a largely unduplicated list[10] compiled by the eleven cooperating professional societies. In comparison with this professional list of 443,000 names, the Census of 1960 from within its broader category of "professional, technical and kindred workers," had classified a total of more than 2 million persons in occupations which might have been within the scope of the National Register, had the less-strict standards of the Census been used.[11]

From the combined mailings to 443,000 names, responses were ob-

[10] Duplicate names within each broad professional area were largely eliminated before mailing by the major cooperating societies. Further elimination was accomplished by the recipients of the questionnaire themselves, by the societies during the editing and exchanging of the returned questionnaires, and by the NSF during re-editing. The final mechanical elimination at the NSF Record Center involved only some 5,000 duplicates from among the 273,000 returned and edited questionnaires.

[11] Of the 1960 total of 7,234,906 professional, technical, and kindred workers, 2,137,012 were classified in one of the following groups of "detailed occupations": accountants and auditors; college presidents, deans, professors, and instructors (n.e.c. and by specified subject, except medical sciences and nonscientific subjects); engineers, technical; natural scientists; personnnel and labor relations workers; social scientists' technicians n.e.c.; and professional, technical, and kindred workers, n.e.c. (cf. Census, *op. cit.*, Table 1, pp. 1-2).

tained from 273,000 or 62 percent.[12] These 273,000 responses were then screened by whichever cooperating society represented the profession each respondent had selected as the one with which he identified himself.[13] Thus, the AEA staff in screening the 11,614 questionnaires from those who had identified themselves as economists, eliminated all those who did not show a Master's or higher academic degree, or the equivalent professional experience. Some 18 percent of all the returned questionnaires were eliminated by this screening, chiefly for failure to meet this education-experience standard,[14] leaving a 1964 National Register total of 224,000 "scientific and technical personnel" in all 12 of the professions covered by the survey.

From a list of 1,172 subject-matter or methodological "specialties" which was enclosed with the questionnaires, each respondent was asked to list up to four "specialties," in descending order, for which his own combined education and experience had made him the most competent.[15] The 12,143 persons herein classified as "registered economists" consisted of those out of the total of 224,000 on the 1964 National Register who, by selecting, as the first of their choices, any one of the 59 specialties which had been listed under "Economics," had indicated that their greatest competence was in some part of the economics profession. This number was about the same as the combined membership of the four cooperating economic societies and it was much larger than the number who would have been classified as economists on any alternative basis of selection from the National Register.[16]

[12] The response rate from the 13,687 questionnaires mailed by AEA itself was 71 percent. To an undetermined extent, the questionnaire method of survey may have eliminated some persons who would have met every other professional requirement. However, not so much the lack of response as the deliberately selected standards of professionalism were responsible for the smaller numbers than those shown by the Census as "economists." The test of willingness to return the questionnaire, meanwhile had the desirable effect of eliminating those who had only a formal, inactive, or casual connection with a professional society.

[13] Individual identification with a particular profession was indicated by the response to questionnaire item No. 6. See the Technical Note at the conclusion of this report.

[14] The AEA screening eliminated 16 percent of the 11,614 responses which its staff reviewed. The 18 percent was the rate of rejection by the combined cooperating societies of the total of 273,000 responses. Some unusable returns were rejected during the screening process, but the principal reason for rejection was failure to show sufficient education or professional experience.

[15] Response to questionnaire item No. 18. See Technical Note at the conclusion of this report. A separate question (No. 12) asked the respondent to designate the specialty represented by his *current employment*. No analyses based on responses to question No. 12 are provided in this report.

[16] An unduplicated list of 13,687 persons represented the combined membership of the American Economic Association, the Midwest Economics Association, the Western Economics Association, and the Farm Economics Association, plus nonmember subscribers to publications and non-member attendants at the meetings of these societies. From this

When the substantial difference between the 1964 National Register and Census (1959) totals of economists is closely examined, it appears that the lower National Register figure largely reflects its use of more precise and strict standards for inclusion as a member of the economics profession. It is instructive, in this connection, to notice the respective divisions of the totals as between teachers of economics and all other economists:

	Nat. Reg. (1964)		1960 Census (1959 data)	
Total economists	Number	Percent	Number	Percent
	12,143	100.0	22,424	100.0
Teachers	3,469	28.6	3,605	16.1
All others	8,674	71.4	18,819	83.9

The National Register (NSF) count of economists engaged in teaching is only 136 less than the Census count of "college professors and instructors: economics"—a difference well within the range which might be expected as a result of varying reporting procedures. For the teaching group—once virtually the only group of professional economists—similarity of employment standards, standardization of titles, and the almost identical National Register and Census definitions of fields of economics make the two counts almost the same.

By contrast, the NSF figure for the non-teaching group of economists is less than half as large as that of the Census. Some of this

list, the so-called "AEA respondents," numbering 9,655, returned the questionnaire mailed by AEA. (Data furnished by AEA.)

AEA members, taken separately, number 10,603, and from the AEA members 5,424 responses were received from those who also reported their salaries.

Had "economists" been defined as those respondents who preferred that designation to any of the other professions covered by the NSF survey, the number of economists would have been 11,535. (Questionnaire item No. 6.)

Not precisely known is the number of persons, otherwise qualified for the National Register, who would have been economists on the basis of their current employment, but this number may be estimated at about 10,500. (See questionnaire items 12, 18.)

If 5 years or more of employment experience involving professional work in economics had been required of National Register respondents, the number qualifying would have been about 9,500. (Questionnaire items No. 12, 17.)

Had educational attainment been used as the basic criterion for selecting the economists from the National Register, the number would have been between 8,600 and 9,800—about 8,600 if Masters and Ph.D.'s had been selected from among those who regarded themselves as economists (Questionnaire items 6 and 7), and about 9,800 if Masters and Ph.D.'s had been selected from those who reported their greatest competence in any one economics specialty (Questionnaire items 7 and 18).

Finally, it is possible to interpret the economics profession as restricted to those who teach the subject at the college or university level. By that restricted definition the number of economists would have been only about 3,500. (Questionnaire items No. 8 and 12.)

difference may be due to underreporting to the National Register. This is suggested (but not proved) by the fact that 38 percent of the total of NSF mailed questionnaires were not returned and is perhaps also suggested by the fact that the combined (unduplicated) membership list of the four economics associations (13,767)—used as the core, but not the entire basis, of the economist portion of the NSF survey—fell 5,657 short in 1964 of the number of economists reported by the Census in 1960. It is also clear, however, that the NSF criteria of professional society listing; professional interest sufficient to induce return of a questionnaire, professional self-identification, and a Master's degree or equivalent experience would exclude large numbers who would provide current job titles to Census enumerators which would get them classified as economists. In any event, it is less important to obtain an agreed number of economists than to know how the characteristics and compensation of economists are affected by precisely known definitions of a group or sub-group within the broad area of the teachers and practitioners of economic knowledge and techniques. For such purposes, there is need for more, rather than fewer, definitions of "economists."

To date, the only detailed tabulations of the National Register data have been those based on classification of each profession on the basis of the respondent's selected field in which he claimed the greatest special competence. (Questionnaire item No. 18.) It would be interesting to learn how, not only the gross numbers of economists, but more importantly, their characteristics and salaries, would be affected if members of the profession were defined, alternatively, as those:

1. With various academic degrees *in* economics—rather than as those with *any* Ph.D. or Master's degree, and regardless of the specialty in which greatest competence is claimed (Questionnaire item No. 7);
2. With total experience primarily in fields of economics—rather than those with experience in any professional field, and regardless of the specialty in which the greatest competence is claimed (Questionnaire item No. 17);
3. Currently employed in work involving a field of *economics*—rather than those currently employed in *any* professional capacity, and regardless of the specialty in which greatest competence is claimed (Questionnaire item 12);
4. Identifying themselves professionally as economists—rather than those claiming greatest specialization in an economic subject (Questionnaire item No. 6);
5. Claiming *any* specialty within the area of economics—rather than merely those claiming *greatest* competence in an economics specialty (Questionnaire item No. 18);
6. Selecting *all* specialties from within the area of economics—rather

than merely claiming an economic specialty as being the *one* of the respondent's *greatest* competence (Questionnaire item No. 18).

Any of these sets of tabulations might be made from the type of questionnaire now used by the NSF, and it is believed that those based on definitions Nos. 3, 4, 5 and 6 above, could be made with relative ease from 1964 data already collected. However, the criterion of greatest specialization, on which this report is based, not only provides the largest number of economists which can be obtained from National Register data (except, possibly, No. 5, above) but also may be said to have the merit of being: more relevant to current status and salary than is the content of earlier education (No. 1, above), less troublesome to collect and code than the characteristics of all past experience (No. 2, above), more stable as a basis for classification than current employment (No. 3, above), more objective than a general identification with the economics profession (No. 4, above), more likely to identify the most highly-specialized economists than a criterion allowing classification as economists of those with only a secondary specialization in the economics area (No. 5, above), and less restrictive than a requirement that all listed specialties be within the economics area (No. 6, above). In favor of suggested criterion No. 6, it may be said that a person claiming high competence in several economics fields is more likely to approach the intellectual ideal of a professional economist than a person who merely has some special competence in some one of a long list of relatively narrow economics specialties.

2. *Fields of Special Competence*[17]

Each of the 12,143 persons, classified as economists on the National Register of 1964, stated[18] that his greatest competence was in one of the fifty-nine economics specialties which were listed in the attachment to the 1964 questionnaire.[19] Since this selection determined the classification itself, the sum of these individual specialties effectively defines the subject-matter content of economics for the purpose of this report. The fifty-nine specialties were grouped under twelve major fields of economics. The following list of the number of persons selecting specialties within each of the twelve fields not only helps to characterize the economists who are the subject of this report, but also

[17] Appendix Table A shows the total number of registered economists, by years of experience, by field, and academic degree. Table E provides the same data on field, but by age group, and degree.

[18] Answers to questionnaire item No. 18. See Technical Note at the conclusion of this report.

[19] Included in the count of 59 is the option of selecting the item, "other, specify," under each of the twelve major fields listed on the questionnaire.

provides information, not previously available, to suggest the relative contemporary importance of major fields within economics.[20]

	Number	Percent
Economics, total	12,143	100.0
General Economic Theory General equilibrium theory (including general welfare economics); economic fluctuations; economic forecasting; macroeconomic theory; methodology; microeconomic theory; other.	1,241	10.2
Economic History; History of Thought Economic history; history of thought; other.	265	2.2
Economic Systems; Development and Planning Economic systems; economic planning; national economic development; regional economic development; other.	864	7.1
Economic Statistics Econometrics; input-output and programming methods; social accounting; statistical methods; other.	483	4.0
Monetary and Fiscal Theory and Institutions Central government finance; fiscal policy; commercial banking and other short-term credit; consumer finance & mortgage credit; monetary theory and policy; state and local finance; other.	978	8.0
International Economics Foreign exchange, international finance; imperialism; colonialism; trade, commercial policy; other	532	4.4
Business Finance and Administration; Marketing and Accounting Accounting; advertising and sales; business finance; business organization; insurance, (private) investment and security markets; managerial economics and industrial management; marketing; other.	4,742	39.0
Industrial Organizations; Government and Business: Industry Studies. Industrial organization and market structure; business, price, and related policies; policies concerning competition and monopoly; government ownership and operation; wartime operations and control; public utilities; transportation, communications; studies of manufacturing, construction, and service industries; other.	804	6.6
Land Economics Agricultural economics, forestries and fisheries; economic geography; natural resources; mining; other.	1,254	10.3
Labor Economics Labor markets; public policy; role of government; trade unions; collective bargaining; labor management relations; wages, hours, conditions of employment; other.	662	5.5
Population; Welfare Programs; Standards of Living Consumer economics; level and standards of living; population; migration; public housing; welfare programs and social security; other.	164	1.4
Economics, other	154	1.3

Nearly half (45.6 percent) of all the economists claimed greatest competence in a specialty within the two business and industrial fields.

[20] This listing is in the order of fields and specialties presented under "Economic" in the attachment to the questionnaire. Subsequent tables in this report carry abbreviated titles for these general fields.

Nearly one-quarter (23.5 percent) of them identified their greatest competence as being within the more generalized fields of theory, history, economic systems, and economic statistics[21]—with general economic theory alone representing one-tenth (10.2 percent) of all the registered economists. The monetary-fiscal and international fields, taken together, claimed the specialized talents of one-eighth (12.4 percent) of the economists. Land economics, on a par with general economic theory, represented the greatest specialized competence of another tenth (10.3 percent), while 5.5 of all the economists specialized in the labor economics field.[22] The eleven specified fields comprehended the specialties claimed by over 98 percent of all those who were classified as economists, leaving only 1.3 percent to be identified under "Economics, other."

3. *Personal Characteristics*

Sex and Age[23]

Ninety-six percent of the 12,143 economists on the National Register in 1964 were men. The median age of the men and of the whole group was 42 years, while that of the 483 women reporting age was 43.7 years. The proportion of men and the overall median age were both considerably higher among economists than among all the 223,854 scientific and technical personnel registered by the National Science Foundation (92 percent men and 38 years median age of the whole group respectively).[24]

The relatively strict criteria for eligibility to the National Register probably had the effect of limiting the number of women and of younger persons of both sexes among the NSF registered economists in 1964, as compared with those classified as economists by the Census in 1960. Women had constituted 12.6 percent of the more broadly defined Census groups, as compared with approximately 4 percent of the more strictly professional National Register group of economists. Following are the comparative percent distributions by age for each sex:[25]

[21] Economic statistics and economic forecasting may have been considerably underrepresented by the NSF survey of 1964, in view of the fact that the Economic statistics (and to a lesser extent the subfield of economic forecasting) did not include many statisticians who designated other areas of statistics as their respective fields of greatest professional competence.

[22] Labor economics is, of course, only one part of the interdisciplinary area of industrial and labor relations.

[23] Section IV and Appendix tables C, D. P, R, S, below, provide amore extensive analysis of the status of the women economists. Appendix Table E shows the total numbers of economists who reported salary, by age group, field of special competence and academic degree.

[24] "Salaries & Professional Characteristics of U.S. Scientists," *op. cit.* Table 1.

[25] The original census data are from: U.S. Bureau of the Census, *U.S. Census of Population: 1960. Subject Reports. Occupational Characteristics.* Final Report PC(2)-7A, Table 4, pp. 31-50.

Age group	Men		Women	
	Register	Census	Register	Census
Total	100.0	100.0	100.0	100.0
Under 25 years	1.2	4.2	2.1	16.6
25 to 34 years	25.1	31.4	22.3	21.8
35 to 44 years	33.9	30.3	29.6	27.4
45 to 54 years	23.9	20.5	23.4	19.9
55 to 64 years	11.1	10.6	16.4	12.1
65 years and over	4.8	3.0	6.2	2.2
Number reporting age	11,639	19,590	483	2,834
Median years of age	42.0	39.8	43.7	39.2

Academic Degree Attained[26]

Registration in the profession of economics in 1964 required at least a Master's degree or equivalent professional experience. Of the 12,143 economists registered, 2,613 with a Bachelor's degree and 90 who did not have a college degree were registered because they were deemed to have had sufficient professional experience. Following is the percent distribution of highest academic degrees attained by the 12,143 registered economists and its components of 11,650 men and 493 women:

	Total	Men	Women
Total	100.0	100.0	100.0
Ph.D. degree	41.9	42.0	39.6
Master's degree	34.6	34.1	46.2
Bachelor's degree	21.5	21.9	12.4
Less than a bachelor's	0.8	0.8	0.8
Professional medical degree	0.0+	0.0+	—
No report of degree	1.2	1.2	1.0

Professional Experience[27]

The highly professional character of the 12,143 economists who are the subject of this report is further illustrated by the fact that by 1964 they had had an average (median) of 14.5 years of professional experience. In comparison, the whole group of scientists on the 1964 Na-

[26] Appendix Tables A and B show the total numbers of registered economists by academic degree and, experience: (Table A) also by field of specialization and (Table B), also by type of employer. Table E presents the numbers by age group and field of special competence. Appendix Table H shows the number with each type of academic degree who were engaged in each of three work activities and had specified years of experience. Tables J-L provide further details for those with selected academic degrees who were engaged, respectively, in teaching, management, and research activities.

[27] Appendix Tables A and B show the total number of registered economists, by years of professional experience and degree and respectively, (A) by field of specialization and (B) by type of employer. Appendix Tables H and L show the numbers with specified years experience who were engaged in specified work activities.

tional Register reported an average (median) of 12.0 years of professional experience.[28] Following are the percent distributions of the total of 11,338 economists who reported their years of experience and of the 10,892 men and the 446 women in the group who did so:

	Total	Men	Women
Total	100.0	100.0	100.0
Less than 2 years	3.3	3.2	3.8
2 to 4 years	12.8	12.7	15.0
Less than 5 years	16.1	15.9	18.8
5 to 9 years	17.6	17.6	17.1
10 to 14 years	18.3	18.6	12.3
15 to 19 years	15.2	15.4	11.2
20 years and over	32.8	32.5	40.6

4. *General Structure of Employment*

The "Not Employed" and the Unemployment Rate

All but 510 of the 12,143 economists who were registered were also currently employed at the time they returned the questionnaire (summer, 1964). Of the 510 who were not currently employed, 175 were students at the time of registration. If these 175 students are subtracted from both the total "not employed" and the total of registrants, there remain 335 not employed among 11,968 who were presumably available currently for full-time employment. The implied unemployment rate among the *registered* economists was therefore 2.8 percent.[29]

Since the NSF questionnaire was not designed for a survey of unemployment, the unemployment rates computed from the returned questionnaire are not strictly comparable with the familiar national rates of unemployment. On the one hand, it is not certain that all of the nonstudent registrants were actually in the labor force at the time they returned the questionnaire. On the other hand, it seems probable that a higher proportion of the 30 percent who did not return the questionnaire were unemployed than those who did reply. Nevertheless, the relatively low computed unemployment rate of 2.8 percent suggests a very good market for economists in 1964.[30]

[28] Computed from the numbers reporting years of experience as shown in Table 1, "Salaries & Professional Characteristics of U.S. Scientists," *loc. cit.*

[29] However, among the 478 women economists (excluding students) the similarly computed unemployment rate was 10.3 percent. See also Section IV, below.

[30] The ratio of all those "not employed" (including students) to the total registered was 4.2 percent for the economists, as compared with 4.3 percent for all scientists and technicians on the National Register. (Computed from "Salaries and Characteristics of U.S. Scientists, 1964", *op. cit.* Table 5)

Type of Employer[31]

Of the 11,633 currently employed economists, 97 failed to report the types of employer for which they were working, and a further 207 were spread thinly over a number of types of employer. There remained 11,329 registered economists who were currently working for seven major categories of employer. Following are the percent distributions of these 11,329 economists and of the comparable total of 211,546 on the National Register:

	Economists	Total registrants
Total, 7 types of employer	100.0	100.0
Educational institutions	44.7	36.8
Federal government	11.3	11.1
Other government	2.5	3.5
Military services	0.7	2.6
Non-profit organizations	4.1	4.1
Industry and business	35.0	39.9
Self-employed	1.7	2.0

Work Activity[32]

Although 5,061 of the registered economists were employed by educational institutions, only 3,469 of them were engaged primarily in teaching. Such a comparison illustrates the general distinction between the type-of-employer and the work-activity classifications. Four leading classes of work activity accounted for 84 percent of the economists and for 81 percent of all the scientists on the 1964 National Register. Following are the percent distributions of the 10,145 economists and of the total of 181,745 scientists who reported any one of these four leading classes of primary work activity:[33]

[31] Appendix Table B shows the total number of registered economists by years of professional experience, and type of employer. Tables M-O provide further details for the principal separate types of employer.

[32] Table 12 in Section 10, below, shows the total number of registered economists as well as the smaller number reporting salary by work activity and academic degree. Appendix Tables H-L provide additional details, as to the numbers engaged in selected work activities, by years of experience and academic degree.

[33] The difference between the 10,145 economists here distributed and the total of 12,143 registered represented: 510 not employed, 397 not reporting type of work activity, and 1,091 reporting "other" work activities. For total registrants, the 42,109 classified under "other" work activity includes those not employed and not reporting work activity and accounts for the difference between the 181,745 here distributed and the total of 223,854. (See "Salaries and Characteristics of U.S. Scientists, 1964", *op. cit.*, Table 6.)

	Economists	Total registrants
Total	100.0	100.0
Management or administration	34.8	25.4
Teaching	34.2	22.7
Research and development	19.0	42.8
Production and inspection	12.0	9.1

The balance between work activities for the whole group of classified scientists was quite different than the economists alone. Research and development was relatively twice as important to all scientists as it was to the economists alone. One-fourth of the classifiable total registrants, as compared with one-third of the economists, were engaged in management or administration. Less than one-fourth of the whole group of scientists, compared with one-third of the economists, were primarily engaged in teaching.

II. *The General Level of Economists' Salaries*

1. *The Salary Group Studied and the Compensation Unit Selected*

The basic group of economists, whose salaries are analyzed in this report, consists of 9,981 persons who reported their 1964 annual salaries for full-time, civilian, professional work. Table 1 shows the relationship of these 9,981 economists to the total of 12,143 economists on the 1964 Register of the National Science Foundation.

The data (even if available) are clearly to be excluded from this salary analysis for persons not reporting current employment status (50), for students (539), and for those not currently employed (335). Because the purpose of this study was to present salaries earned by economists in their professional capacity, 450 employed persons who were not currently engaged in professional work were also excluded. As compensation was reported on an annual basis, uncharacteristic salaries were eliminated by excluding the 223 part-time workers, even when these were employed in professional work. Inasmuch as those economists on active duty in the military organizations proved to be a very small group, only about half of whom reported their salaries, the data to be analyzed were made more homogeneous and representative by excluding the 44 reports of military salaries. Of the remaining 10,502 economists who were currently employed full-time on civilian work, 95 percent (9,981) reported their current salaries. Because this excellent overall rate of volunteer reporting did not necessarily occur in every sub-group, the tables which follow generally include both the number of persons reporting each particular type of salary and the applicable total number in that sub-group which con-

TABLE 1—NUMBER OF ECONOMISTS[1] IN SALARY ANALYSIS

(Exclusions from total number of economists registered by the National Science Foundation in 1964, to arrive at number of economists reporting salaries from full-time professional civilian employment)

Classification	Number: Not in salary analysis	Number: Net
Total registered economists		12,143
Total excluded from salary analysis	2,162	
Total, economists reporting salaries on civilian full-time, professional employment		9,981
Total registered economists		12,143
No report of employment status	50	
Economists of known employment status		12,093
Students	539	
Economists available for full-time employment		11,554
Not employed	335	
Employed economists		11,219
Not employed in professional work	450	
Economists employed professionally		10,769
Part-time professionally employed	223	
Full-time professionally employed economists		10,546
On active military duty	44	
Civilian full-time, professionally employed		10,502
Not reporting salaries	521	
Total, economists reporting salaries from civilian full-time, professional employment		9,981

[1] "Economists" here defined as respondents who reported their greatest professional competence, based on a combination of education and experience, to be in the general area of economics. Response to questionnaire item No. 18. (See Technical Note at the conclusion of this report.) Appendix Table C shows the corresponding numbers of men and women separately.

tributed to the total of 12,143 registered economists as described in Section I of this report. The word "economists," when used without qualification in Sections II-V of this report, is to be understood to refer to the salary-reporting group of 9,981 economists who were engaged in 1964 in full-time, civilian, professional work.

The unit of measurement of compensation presented in this report is the basic annual salary associated with the respondent's principal pro-

fessional employment as of January 1964.[34] Among the several possible measures of remuneration, basic salary is generally the easiest to interpret and the most reliably reported on a mailed questionnaire. It is important to realize that the salaries here reported do *not* measure the total *incomes* of the economists. An analysis of economists' professional incomes could, indeed, be made from data obtained for the National Register and such an analysis might well be the subject of a future study.[35]

2. Salaries of All Economists, with Some Comparisons

Total Registered Economists. The basic annual salaries of all the economists on the 1964 National Register ranged from less than $5,000 to more than $100,000 (Table 2, Note 1). Within this very broad range, the salaries most frequently paid (mode) were close to $10,000—10.5 percent of all the economists being paid within $500 of that amount. One-tenth of the economists had salaries of $7,800 or less (lowest decile), while the highest paid one-tenth of them had salaries of $20,000 or more (highest decile). The salaries of the middle one-half of the economists ranged from $9,300 to $16,000 (inter-quartile range). The median salary, dividing the lower paid from the higher paid half of the economists, was $12,000. The arithmetic mean salary (aggregate salaries divided by the number receiving them) was $13,670. This mean is considerably higher than the median because of the influence on the mean of the exceptionally high salaries of a small minority of the economists.[36]

[34] The salary data used in this report were obtained by the responses to questionnaire item No. 15, reproduced in the Technical Note at the conclusion of this report. Note that the current professional job (item No. 12) for which salary was reported (item No. 15) did not necessarily involve any specialty in economics (item No. 18).

[35] Basic salary most seriously understates total income in the case of teachers paid for a 9-10 month academic year (and hence of the teaching component of the economists employed by educational institutions). Hence, the Committee did obtain some income data for the teachers and for the total of the economists, which data are discussed in Section III-11 below. These income data were furnished by Dr. Emanuel Melichar, author of Section V of this report. They were derived from responses to questionnaire item No. 16 ("estimated gross professional income") and were obtained from the National Register tapes, released to Dr. Melichar at the request of the Committee, to enable Section V of this report to be prepared by him. The Committee's own publication schedule did not permit any general analysis of economists' professional incomes.

[36] The geometric mean (anti-log of the arithmetic mean of the logs of the salary values), a type of average which mitigates the influence of the exceptionally high salaries, was $12,450. Geometric means are used as the basis of the analysis in Section V of this report. In Sections II-IV of this report, the form of "average" used will be the median, unless otherwise stated.

Data on median, decile, and quartile salaries were obtained from NSF tabulations furnished directly to the Committee. The other salary data (except the salary distributions for AEA members and AEA registrants) were furnished to the Committee by Dr. Emanuel Melichar, who computed them from NSF tapes covering the identical group of

TABLE 2—SALARIES OF ECONOMISTS, AS DEFINED BY FIVE METHODS

(Number and median, decile, quartile salaries and inter-quartile range of salaries of successive components of the total National Register economists)[1]

Defined group	Number reporting salary	Thousands of dollars of reported salary: Median	Lowest decile	Lower quartile	Upper quartile	Highest decile	Inter-quartile range
All salaries							
1. A.E.A. Members[2]	5,424	11.6	7.5	8.9	15.5	19.1	6.6
2. Non-member, self-identified economists[3]	2,459	11.7	7.9	9.5	14.9	18.2	5.4
3. A.E.A. Registrants[4]	7,883	11.6	7.5	9.0	15.0	19.0	6.0
Full-time salaries only							
4. Net additional economic specialists[5]	2,098	14.4	8.9	11.0	19.7	25.5	8.7
5. Total registered economists[6]	9,981	12.0	7.8	9.3	16.0	20.0	6.7

[1] Corresponding salary distributions are shown in Appendix Table F.
[2] Members of the American Economic Association reporting salaries, regardless of field of greatest competence. (Identified by response to questionnaire item No. 21.)
[3] Respondents, other than AEA members, who regarded themselves as economists. (Response to questionnaire item No. 6.)
[4] Total of groups 1 and 2, i.e. respondents from any cooperating society who regarded themselves as economists (questionnaire item No. 6), regardless of field of greatest special competence.
[5] Respondents claiming greatest competence in some economics specialty (questionnaire item No. 18) who were not AEA members and did not regard themselves as economists in preference to other professions. The figure does not include those with unknown current employment status, students, those employed non-professionally, those with part-time employment, and those on active military duty.
[6] Total of groups 3 and 4, i.e. all registered professional personnel who claimed greatest competence in some special field of economics, excluding those listed in Note 5.

American Economic Association Members and Other Registered Economists. Some interesting examples of the influence of definition on the resulting compilations of economists' salary levels are provided by the National Register tabulations made thus far, even though some of the interpretative data are not yet available (Table 2).[37] The median salary of the 5,424 members of the American Economic Association who reported salary was $11,600 (Table 2, line 1), as compared with a $12,000 median for the total of 9,981 registered economists as defined for the purposes of this report (Table 2, line 5). The principal contrast was at the upper end of the salary distribution. For example, the highest-paid tenth of the AEA members received $19,100 or more, whereas the corresponding tenth of the total registered economists had salaries of $20,000 or more. The salaries most frequently received by AEA members were close to $9,000 (±$500, whereas $10,000 was most frequently received by the total of the registered economists.[38]

The group of 2,459 registered personnel who, although not members of the American Economic Association, identified themselves professionally as economists (Table 2, line 2) had generally lower, but less

economists. The tapes were released to Dr. Melichar by NSF at the request of the Committee.

[37] The salary distributions, from which the data in Table 2 are derived, are provided in Appendix Table F. The analysis which follows was limited by the lack of detailed salary tabulations for the 5,424 AEA members and the 7,883 "AEA registrants" at the time this report was prepared.

[38] Data from Appendix Table F.

scattered salaries than did AEA members. This group had fewer relatively low salaries (compare the lower decile and quartile amounts) and at the same time, fewer relatively higher salaries (higher quartile and decile amounts), so that half of those who called themselves economists regardless of society membership were paid salaries within a $5,400 range (Table 2, column 7). The corresponding middle range for the AEA members was $6,600. One-third of all the salaries of these 2,459 non-AEA profesional economists lay within the $3,000-range between $8,500 and $11,400, and one-eighth of them were paid within $500 of $10,000 per year.[39]

It was the inclusion as economists of all registered personnel who claimed greatest competence in any single economic specialty (Table 2, line 4) which lifted the median salary of the total of economists to $12,000 and spread the higher end of the salary distribution upward. These 2,098 economics specialists, who were not AEA members and did not prefer to regard themselves as economists, had a median salary of $14,400, and indeed, the lowest paid one-quarter of them were paid as much as $11,000, or nearly as much as the lower half of the AEA members were paid. Even more striking were the higher levels of salary reached by these "economists-by-specialty-only," the highest-paid tenth of them receiving salaries of $25,500 to more than $100,000. Although the salary paid most frequently to these economic specialists was about $10,000, the middle half of them had salaries which scattered over a range of $9,000, between $11,000 and $19,700 (Table 2 column 7).[40]

The differing salary levels of the AEA members, the self-identified economists, and the total of economic specialists appears to have been largely the result of the differing proportions of each of these groups who worked for the various types of employer. The following listing of median salaries shows that the AEA members and those identifying themselves as economists generally received as high or higher median salaries in each type of employment, taken separately, as did the total

[39] Data from Appendix Table F.

[40] Data from Appendix Table F. The discussion of lines 2 and 4 of Table 2 has been oversimplified in one respect. The data shown on line 2 were obtained by subtracting the numbers indicated by line 1 from those indicated by line 3, at each salary interval (Appendix Table F), and similarly the data on line 4 were obtained by subtraction of line 3 from line 5 in the underlying salary distribution. Actually, a few individuals who were AEA members (line 1) identified themselves with other professionals who were AEA members (line 3), and some of those who indicated greatest competence in an economics specialty (line 5) had not chosen economics as their preferred profession (line 3). Lines 2 and 4, therefore, represent the salaries of the net additions in each case, rather than those of the gross numbers added to the preceeding groups. However, this detail does not modify the meaning of the analysis in the text.

the registered economists.[41] As employed by educational institutions e AEA registrants (all those preferring the designation of "econo-ist," including AEA members) received the highest median salary ıd the AEA members taken alone received the lowest median salary ɔm this one type of employer.

Type of employer	AEA members	AEA registrants	Total economists
Educational institutions	$ 9,700	$10,200	$10,100
ndustry or business	15,000	14,100	14,400
Federal government	14,500	13,700	13,700
Other government	12,000	11,500	11,700
Non-profit organizations	15,000	15,000	15,000
Self-employed	20,000	20,000	20,000
Other employment	14,500	14,000	14,300

therwise, it was the AEA members themselves who received as high higher median salaries than those of the other two groups. But, as ll be seen more completely in Part III of this report, the educational stitutions paid the lowest salaries to all the groups of economists and dustry paid the highest salaries to each of them.[42] The most impor-nt contrasts in the deployment of economists reporting salary among pes of employer are indicated by the following summarized percent stributions:[43]

Type of employer	AEA members	AEA registrants	Total economists
Total	100.0	100.0	100.0
Educational institutions	62.3	56.9	44.6
Industry or business	13.3	16.2	33.3
All other employers	24.4	26.9	22.1

ore than three-fifths of the AEA members, as compared with 45 per-nt of the total number of economists as classified for the National egister, were employed in the low-salary educational institutions. arely more than one-eighth of the AEA members, in contrast to one-ird of the total of economists, were employed by high-salary indus-y or business. In fact, over half of the net gain in salary reports ob-ined by extending the definition of "economist" from AEA members anyone with an economic specialty (9,981 vs 5,424 reports) consist-

[41] Appendix Table G shows the percent distribution of salaries of AEA members, by pe of employer.

[42] As will also be seen in Part III-8 of this report, the particular economists who were ıployed by industry or business had had the larger average number of years of pro-ssional experience—the factor which proved to be most closely associated with salary vels.

[43] Data summarized from Appendix Tables B and G.

ed of salary reports from employment in industry or business. Even more striking is the fact that 98 percent of the addition to total salary reports of the specialists who had not identified themselves primarily as being economists (9,981 vs 7,883 reports) were reports from industrial or business employees. If the 5,424 AEA members reporting salary had been distributed between employers as were the total of 9,981 economists, the median salary of AEA members would have been $12,500, or $500 more than the actual average of all the persons classified as economists on the National Register.[44]

TABLE 3—SALARIES IN TWELVE PROFESSIONS, 1964

(Total numbers and numbers reporting salary to National Register; median, decile and quartile amounts of full-time professional salary)

Field of employment	Thousands of respondents		Thousands of dollars of reported salary				
	Total registered	Salary reported[1]	Median	Lowest decile	Lower quartile	Upper quartile	Highest decile
All fields	223.9	181.0	11.0	7.1	8.6	14.0	18.0
Social Sciences							
Economics	12.1	10.0	12.0	7.8	9.3	16.0	20.0
Statistics	2.8	2.5	12.0	8.0	9.5	14.9	17.2
Psychology	16.8	14.0	10.3	7.3	8.5	12.9	16.3
Sociology	2.7	2.3	10.1	7.3	8.5	13.0	16.0
Linguistics	1.4	1.0	9.0	5.8	7.2	11.8	15.0
Physical Sciences							
Physics	26.7	20.2	12.0	7.4	9.0	15.0	18.7
Chemistry	63.1	52.2	11.0	7.2	8.7	14.0	17.5
Mathematics	17.4	14.7	11.0	7.0	8.7	14.7	18.5
Biology	27.1	21.8	10.7	6.5	8.3	14.5	19.0
Meteorology	5.5	2.9	10.6	7.8	9.0	12.9	15.5
Earth Sciences	17.9	13.6	10.3	7.1	8.5	13.0	17.0
Agriculture	9.5	8.5	9.2	6.2	7.5	11.4	14.0
Other fields	20.8	17.2	11.1	7.0	8.6	15.0	18.5

[1] Excluding 42,844 respondents on active military duty, those with unknown employment status, students, not employed, and employed non-professionally.

Economics and Other Professions. Table 3, reproduced from the Committee's initial report,[45] shows that the overall median full-time

[44] Similarly, if those identifying themselves, professionally, as economists ("AEA registrants") had been distributed between employments as were the total of economists, their median salary would have been $12,300.

[45] See *American Economic Review*, May 1965, *loc. cit.* Table numbers, titles, and arrangement have been adapted to the form of the current report. The phrase "scientific and technical group" was used in the first report to denote the same category which is called a "profession" in the current report (Economics, Statistics, Psychology, etc.), and is

ABLE 4—SALARIES BY SELECTED TYPES OF EMPLOYER IN TWELVE PROFESSIONS, 1964

(Educational institutions, federal government, industry or business: numbers reporting salaries; median salaries)

Field of ecialization	Number of respondents in thousands				Median salaries-thousands of dollars			
	All reported[1] employers	Educational inst.	Federal govt.	Industry and business	All reported[1] employers	Educational inst.	Federal govt.	Industry and business
al registered	180.7	61.3	22.6	78.8	11.00	9.6	11.0	12.0
l sciences								
onomics	9.9	4.5	1.2	3.3	12.0	10.1	13.7	14.4
tistics	2.5	0.6	0.6	1.0	12.0	10.4	13.0	12.0
chology	14.0	7.4	1.3	1.2	10.3	9.7	12.0	14.1
iology	2.3	1.9	0.1	0.1	10.1	10.0	12.9	14.0
guistics	1.0	0.8	0.1	0.1	9.0	9.0	10.7	12.0
cal sciences								
ysics	20.2	7.9	2.8	8.5	12.0	9.6	12.0	13.5
emistry	52.1	9.2	3.9	36.1	11.0	9.3	10.8	11.7
thematics	14.7	5.9	1.1	6.6	11.0	8.7	12.1	13.0
logy	21.8	13.4	2.8	2.4	10.7	10.0	11.0	12.5
teorology	2.9	0.3	1.8	0.5	10.6	10.5	10.6	11.0
rth sciences	13.6	2.9	2.2	7.3	10.3	8.8	11.0	11.0
riculture	8.5	2.4	3.2	1.3	9.2	10.2	9.3	9.0
fields	17.2	4.2	1.5	10.3	11.1	8.3	12.1	12.0

eporting both full-time salary and type of employer including the following types of employers, in addition to hree types specifically shown: state and local government, military organizations, public health service, non- organizations, self-employed, and others.

ry of the total of registered economists was considerably higher n that of nine of the other eleven professions included in the 1964 tional Register. Furthermore, Table 4, also reproduced from the lier report, shows that industry and business and the Federal gov- ment each paid higher average salaries to economists than to any of other eleven professions, and that in educational institutions econ- ists' salaries were higher than those of eight of the eleven other fessions surveyed.

)f course, comparisons of general averages of this kind do not nec- arily indicate relative rates of pay of those with the same training l experience when employed under similar conditions. The Com- tee has not had the opportunity to assess either the representative- s of the National Register salaries reported for the professions out- e of economics nor to compare the salaries paid to those of similar and professional experience. However, the superiority of econo- sts' average salaries does not appear to depend on differing propor-

the same category as the National Science Foundation calls a "field of employment." the purposes of the current report, the term "field" is used to denote a subordinate within economics, such as "general economic theory". In Tables 3 and 4 the mbers of respondents" are shown in even thousands, whereas the exact numbers shown in the other tables of the current report. In Table 4, the "number of re- ndents" refers to the number reporting salary, rather than the total number registered, detail is omitted for state and local government, non-profit organizations, self- loyed, and "other" types of employer. The detailed tables which follow in the current ort show data for these additional types of employment.

tions of employment by different types of employer or by different classes of work activity. And even if the definition of "economist" had been restricted to members of the American Economic Association, their median salary of $11,600 would still have been above that of all but physics and statistics among the eleven other professions surveyed.

Other Surveys of Economists' Compensation. The $12,000 median salary here reported for the year 1964 was 56 percent higher than the $7,706 median earnings reported by the Census for the year 1959.[46] This enormous difference in reported compensation cannot be explained by the difference between "salaries" and "earnings", or by any bias in the reporting of either of the two figures, or by the 5-year difference in the time of observation, or even by the combination of these factors. Rather, the $4,300 difference between the two averages comfirms the analysis in Part I-1, above, which showed that the 22,400 Census "economists" were, by definition, largely a different group of people than the 12,100 "economists" on the National Register or the 9,981 of them who reported full-time salaries in civilian employment.

The Census "earnings" may have been somewhat lower than the National Register "salaries" because of the inclusion by the Census of the relatively low annual earnings of students and part-time employees. There are also some limited possibilities of an upward bias in the National Register data because of the somewhat incomplete coverage and inaccurate reporting which must be expected in the case of voluntary responses to a mailed questionnaire. However, the National Register did obtain a 75 percent response *from the universe it attempted to reach* and 95 percent of those responding reported their salaries. It does not appear likely that the professional economists solicited would have had sufficient motive or lack of knowledge to permit any large and persistent overstatement of their salaries.[47]

The five-year lapse of time between the Census and National Register observations may account for much of the difference in the reported compensation of economics *teachers*, but not in the compensation of the other economists. If we were to assume an annual salary increase of 6 percent compounded, the $7,098 average for the teachers in 1959

[46] The 1960 Census showed earnings during the year 1959. $7,706 is the median obtained by combining the two Census groups of "professors and instructors—economics" (median, $7,098) and "social scientists—economists" (median $7,865). Compare Census, *loc. cit.*

[47] Since the questionnaires were not job applications, respondents had nothing to gain by deliberately overstating salaries, and if government scrutiny (such as that of Internal Revenue) were mistakenly feared, the tendency would have been to understate. The possibility that salary might be confused with income was minimized by provision of a separate question item asking for "gross professional income" (No. 16). If take-home pay had been mistaken for basic salary, the latter would have been understated.

would have risen to about $9,500 by 1964, the latter figure being reasonably consistent with the median salary of $9,700 shown by this survey for economists engaged in teaching (Section III-10 below). On the same assumption, however, the median salary of the total of the Census "economists" would have risen from $7,706 in 1959 to only $10,300 in 1964—still $1,700 below the National Register median of $12,000.

As has been seen in I-1, above, the standards for determining whether a person is teaching economics are fairly definite and uniform, and the resulting totals of teachers indicated by the two surveys (3,605 by Census and 3,469 by National Register) are not far apart. For all the other economists, however, the requirements for admission to the National Register defined a group (8,700 total) whose salaries should be expected to be much higher than the 18,800 classified as "economists" by the Census if their informally reported job titles suggested any kind of connection with economics at any level of competence and responsibility.

The salaries indicated by two privately conducted, recent studies of professional economists are more closely compatible with those reported here than with the Census group. Edwin G. Gooding has shown a median professional salary of $11,600 for the academic year, 1964-65.[48] This median is $400 lower than the $12,000 shown here for all professional economists in 1964, but it is exactly the same as the median for the American Economic Association members (Table 2, above). These were actually the two sources from which Gooding's sample of economists was obtained.[49]

This Committee, through the kindness of Daniel L. Rosen, was shown the detailed findings of his recent survey of salaries of 445 "business economists."[50] Without releasing the results of Mr. Rosen's study, it may be said that the median basic salary shown by Rosen is reason-

[48] See *New England Business Review*, December 1964, p. 2, (footnote) and pp. 8-14.

[49] Gooding's universe consisted of: (1) 1,180 from a random 10 percent sample of AEA members, and (2) 1,250 persons who registered at the Public Employment Service clearing house as organized at the December 1963 annual meetings of the professional societies in Boston. While the numbers of responses from each source is not stated, the relative numbers are not material in view of the identical median shown for the AEA members and registrants. Gooding's data consisted of respondents' estimated salaries for the academic year, 1964-65, made chiefly if not entirely, about December 1963. The results should not be expected to differ greatly from those on the National Register questionnaire, which were made between April and June 1964, even though the NSF questionnaire asked for annual salaries "as of January 1964."

[50] Copies of the tables, including data which cannot be obtained from NSF tabulations, may be made available under certain circumstances through Mr. Rosen, who is Senior Economic Specialist, Chase Manhattan Bank, New York, N.Y.

ably consistent with those presented here, when the "business economists" he surveyed are correctly matched with the appropriate subgroup of economists on the National Register.

Comparison of the average salaries here shown with those indicated by the Census, by Gooding, and by Rosen, shows that an average salary of "economists" is quite meaningless unless those to be considered as "economists" are carefully defined. There is no reason to doubt that the salaries shown for economists on the National Register for 1964 are characteristic of those of professional economists engaged in full-time professional work, provided that the National Register operating definition of "professional" is accepted.

III. *Six Dimensions of Economists' Salary Structure*

Plan of Section III. The tabulations of National Register data permit an unusually detailed analysis of differing levels of salary, as these are related to each of seven characteristics: (1) type of employer, (2) academic degree, (3) age, (4) professional experience, (5) work activity, (6) field of special competence, and (7) sex. In view of the very small proportion of women among the economists, the sex characteristic is not treated as a major factor related to overall salary structure, but instead, the special patterns of the women's employment and salaries are discussed separately (Section IV, below). This section (III) deals with the remaining six characteristics.

Modern computer technique makes it possible to deal simultaneously with all six of these salary differentiating factors so as to express their relative degree of net association with salary differences. The results of such an analysis are presented in the concluding section of this report (V). The present section, meanwhile, attempts to present the major patterns of the economists' salary structure in the more familiar form of the specific dollar amounts of salary received by each classified group of economists.

In addition to a simple presentation of the salaries by those representing each gradation of each characteristic, taken by itself, the following twelve subsections reflect a recognition of the fact that each one of the six characteristics is somewhat interrelated with each of the other five. Thus the average salary of the economists with a Ph.D. degree was affected by: the proportion of the Ph.D.'s employed by industry, educational institutions, etc; by the distribution of their ages; by the functions (work activities) they performed, and by the other characteristics in turn. However, any attempt to classify salaries by each factor, combined with subclassification by all the other factors, would result in an unwieldy report in which the significant general sal-

ary patterns would be lost in a maze of detail. The actual content of the following twelve subsections has been determined partly by the limitations of the tabulations received and partly by the exercise of judgment in the selection of those data believed to be of general interest. Many other combinations of the salary and employment data, beyond those discussed, may be made from the information to be found in the Appendix tables.

The sequence of topics is a commonsense one, whereby the findings of each subsection lead on to the considerations raised by the following one. The obvious contrasts in salaries paid by different types of employer having been presented first (III-1), one becomes curious about the relationship of salary to academic degree and about the interrelationship between degree and type of employer (III-2). Next to be explored are the familiar kinds of data as to age in relation to salary, age being first considered by itself (III-3) and then as interrelated with educational attainment (i.e. academic degree, III-4). The finding that age has both a positive and a negative relationship to salary leads to an exploration of the association between age and the rather novel data here available on length of professional experience, as these factors were, in turn, differentially associated with salary (III-5). The finding that professional experience is the factor with the most pervasive relationship to salary prompts a more detailed scrutiny of the experience factor than of any of the other factors. First as experience, taken by itself, is related to salary (III-6), then as experience is interrelated with two factors—academic degree (III-7) and type of employer (III-8)—already found to be significant; and finally the simultaneous interrelation of experience degree and type of employer (III-9) is examined.

Since an economist's type of employer does not completely identify the kind of work he performs, the relationship of work activity to salary is next considered, along with the interrelated factor of academic degree (III-10). Because teaching is found to be by far the lowest-paid major work activity of the economists, a special analysis is made of the extent to which the low annual salaries of the teachers merely reflect their unusually short (9-10 month) academic year (III-11). Finally, the National Register tabulations include some unique data on salaries as classified by the special field of economics for which each respondent reported that he had the greatest special competence. The last subsection (III-12) presents the median salary of those most competent in each economic field, along with differences by field in three other factors affecting salary, namely: experience, age, and the proportions of those from each field employed by industry and by educational institutions.

1. *Type of Employer, Considered Separately*

To say merely that economists' salaries differ according to the kinds of employers for which they work would be a gross understatement. In 1964, only the State and local governments paid salaries (median, $11,700) which were even close to the all-economist median of $12,000. Each of the other seven classified types paid an average salary which deviated by 14 to 67 percent from the general average of $12,000, as may be seen from the listing, in ascending order, of average salary by type of employer.[51]

	Median salary (dollars)	Percent difference from all economist median salary	Percent of total salary reports
Total	12,000	0	100.0
Educational institutions[52]	10,100	−16	44.6
State and local govts.	11,700	− 2½	2.7
Federal government	13,700	+14	12.3
Employer not reported	14,000	+17	0.4
Other employers	14,300	+19	1.8
Industry or business	14,400	+20	33.3
Non-profit organizations	15,000	+25	4.1
Self-employed	20,000	+67	0.8

These data confirm the existence of two quite distinctive markets for the services of economists. The largest market is dominated by the educational institutions which paid 16 percent less than the all-economists average salary level. The next largest market is provided by industry and business (and the closely associated non-profit organizations) where the average salary was 20 percent above the general average and 42.6 percent higher than that of the educational institutions.[53]

2. *Academic Degree and Type of Employer*

At first glance, lines 4, 19 and 26 of Table 5 would seem to show that the *higher* an economist's degree the *lower* his salary! On closer examination, the two differentiated markets for economists—education

[51] Data from Table 5, below, consolidated by type of employer, as identified by responses to questionnaire item No. 10.

[52] Three-quarters of the teachers' salaries were paid for 9-10 months of official duties (Section III-11, below), but at least one-quarter of those employed by educational institutions were in activities other than teaching. In any case we are concerned at this point with what salaries the educational institutions paid, rather than what the individuals earned.

[53] For some individual economists, the two markets overlap, of course. Thus the highest paid one-quarter of the Ph.D.'s in educational institutions had salaries of $14,000 or more, while the lowest-paid quarter of those in industry and business were paid $12,000 or less (See Table 5, below).

TABLE 5—SALARIES OF ECONOMISTS, BY DEGREE AND TYPE OF EMPLOYER
(Numbers and median, decile, and quartile salaries for full-time professional work)

Degree Type of employer[1]	Number of economists		Thousands of dollars of reported salary				
	Total registered[2]	Salary reported[3]	Median	Lowest decile	Lower quartile	Upper quartile	Highest decile
1. Total registered	12,060	9,981	12.0	7.8	9.3	16.0	20.0
2. Not reporting degree	142	98	13.0	7.7	10.0	16.5	35.0
3. Total reporting degree	11,918[4]	9,883[4]	12.0	7.8	9.3	16.0	20.0
4. Ph.D. degree	5,077	4,544	12.1	8.4	9.8	16.0	20.0
5. Educational inst.[5]	3,440	3,255	11.0	8.1	9.2	14.0	17.0
6. Federal govt.	450	442	14.8	10.3	12.0	17.5	19.2
7. Other govt.	82	79	15.0	9.7	12.0	18.0	21.5
8. Non-profit. org.	216	196	16.5	11.0	13.5	20.0	25.0
9. Industry-bus.	548	458	18.9	12.0	15.0	25.0	33.0
10. Other	95	84	15.0	11.0	12.6	20.5	26.0
11. Master's degree	4,161	3,171	11.0	7.0	8.4	15.0	19.0
12. Educational inst.[5]	1,433	1,048	8.0	6.2	7.0	9.9	12.5
13. Federal govt.	563	537	12.6	8.4	10.0	15.2	17.5
14. Other govt.	136	126	10.5	7.5	8.7	13.0	15.5
15. Non-profit org.	191	161	12.5	8.5	10.3	16.5	21.0
16. Industry-bus.	1,409	1,190	13.1	8.7	10.2	18.0	24.0
17. Self-employed	69	25	18.0	12.0	12.0	24.0	25.0
18. Other	81	70	12.5	7.5	10.0	16.0	21.0
19. Bachelor's degree	2,588	2,106	13.5	8.0	10.0	17.6	24.0
20. Educational inst.[5]	151	124	8.0	6.3	7.0	10.4	14.1
21. Federal govt.	249	238	12.5	8.0	10.0	16.3	18.0
22. Other govt.	57	54	10.0	7.5	8.7	13.0	15.4
23. Non-profit org.	51	46	14.3	8.4	12.0	17.0	22.5
24. Industry-bus.	1,885	1,581	14.0	8.4	10.5	18.1	25.0
25. Self-employed	70	33	16.0	9.0	12.0	30.0	40.0
26. Less than bachelor's degree	90	60	16.7	9.6	12.0	25.0	30.0
27. Industry-bus.	61	46	17.0	9.7	12.0	26.0	35.0

[1] Totals and subtotals include data for categories represented by less than 25 persons for which individual salary information is not shown.
[2] Excluding a total of 83 on active military duty, of whom a total of 44 reported their basic annual salary.
[3] Excluding: unknown employment status, students, not employed, and employed non-professionally.
[4] Including 2 respondents with professional medical degrees.
[5] Including salaries paid for 9–10 months, as well as for 11–12 months.

and industry—provide the solution to this paradox, as may be more easily seen from the following rearrangement of the median salaries shown in Table 5:

	Ph.D.	Master's	Bachelor's	Less than Bachelor's
Educational institutions	11.0	8.0	8.0	—
Industry or business	18.9	13.1	14.0	17.0
Federal government	14.8	12.6	12.5	—
State and local governments	15.0	10.5	10.0	—
Non-profit organizations	16.5	12.5	14.3	—
Self-employed	—	18.0	16.0	—

Educational institutions, taken separately, paid Ph.D.'s $3,000 more than those with lesser degrees, but they employed hardly any full-time professional economists with only a Bachelor's degree (124 of 4,455

reporting salary). Yet they did employ 72 percent of all the Ph.D.'s (3,255 of 4,455 reporting salary).

In industry alone, the Ph.D.'s actually averaged $4,900 more than those with only a Bachelor's degree, but the salaries of these Bachelors, at the same time, exceeded the salaries of even the Ph.D.'s in educational institutions by $3,000 a year. Industry employed three-quarters of all those with a Bachelor's degree or less. It was the concentration of the Bachelors in high-salary industry which brought their overall average up to $13,500, while the concentration of Ph.D.'s in low-salary educational institutions brought the Ph.D. average down to $12,100.

The relatively low salaries of those with Master's degree—in industry and non-profit organizations, actually lower than those of the Bachelors—do not indicate that a Master's degree is of no economic value. The Masters' salaries should be evaluated in the light of their relatively short periods of professional experience (See III-7 and 9).

3. *Age Considered Separately*

The median age of the 9,966 economists who reported *both* age and salary for full-time professional work in 1964 was 42 years. The ages of these economists ranged from 20 years to over 70 years, but nearly four-fifths of them were from 30 to 54 years of age, and nearly one-fifth were in the single five-year age group of 40 to 44 years.[54]

Economists' average salaries progressed upward with age to a plateau of $15,000 in the fifteen-year span between 50 and 64 years, and then were lower again—even for those continuing on full-time professional work—as is indicated by the following digest of data from Table 6.

Age (years)	Median Salary	Absolute Difference	Percent difference over preceding group	
			5-year group	Annual average
20–24	$ 7,800	$ —	—	—
25–29	8,400	600	7.7	1.5
30–34	9,700	1,300	15.5	3.1
35–39	11,500	1,800	18.6	3.7
40–44	13,000	1,500	13.0	2.6
45–49	14,800	1,800	13.8	2.8
50–54	15,000	200	1.4	0.3
55–59	15,000	0	0.0	0.0
60–64	15,000	0	0.0	0.0
65–69	14,500	−500	− 3.3	−0.7
70 and over	12,000	−2,500	−17.2	−3.4

[54] In each of the respects noted above, the 9,966 economists classified for the age-salary analysis were closely similar to the entire group of 12,143 economists on the National Register. The elimination for present purposes of those not currently engaged in

4. *Age and Academic Degree*

For each five-year age group, Table 6 shows the distribution of salaries paid to those with each of three degrees. When rearranged to facilitate comparison of the age-salary relationship of those with each degree, the median salary data from Table 6 appear as follows:

Age (years)	Median salaries ($000's)		
	Master's	Ph.D.'s	Bachelor's
25–29	8.0	8.8	8.4
30–34	9.2	9.6	10.0
35–39	11.0	11.0	12.5
40–44	12.8	12.5	15.0
45–49	14.2	13.6	17.0
50–54	14.7	14.3	17.2
55–59	14.5	15.0	17.0
60–64	13.5	15.0	20.0
65–69	12.0	15.0	—

Considering first the Masters who were employed about equally in low-salary education and in high-salary industry (II-2, above), one may note the rapid salary progression up to age 49, the slight, if any progression between 50 and 59, and the lowered salary averages with advancing age beyond 60 years. When comparing the absolute levels of salaries of the Ph.D.'s with those of the Bachelors, on the other hand, it should be recalled that 72 percent of the Ph.D.'s were in low-salaried educational industries, while about three-quarters of the Bachelors' were with high-salary employers. The overriding effect of the type-of-employer relationship explains why the Ph.D.'s had lower average salaries than the Bachelors' at every age level above 29 years and why even the Masters had higher salaries than the Ph.D.'s in the age span between 40 and 54 years.

At ages 55 and above, however, the salaries of the Ph.D.'s showed no decline with advancing age, and finally exceeded those of the Masters group, the salary advantage of the Ph.D.'s over the Masters reaching a maximum in the 65-69 years age bracket. Economists with no more than a Bachelor's degree, like the Master's and unlike the Ph.D.'s, had average salaries which tended to decline after age 54, but when those with only a Bachelor's degree reached 60-64 years, the

full-time professional work curtailed the proportions of the total registered economists who were less than 30 and more than 60 years of age, but left undisturbed the median and the approximate proportions as stated in the modal age group and in the larger group, 30 to 54 years.

TABLE 6—SALARIES OF ECONOMISTS BY AGE GROUP AND DEGREE
(Numbers and median, decile and quartile salaries for full-time professional work)

Age group Degree	Number reporting degree	Thousands of dollars of reported salary				
		Median	Lowest decile	Lower quartile	Upper quartile	Highest decile
Total	9,883	12.0	7.8	9.3	16.0	20.0
Ph.D.	4,544	12.1	8.4	9.8	16.0	20.0
Master's	3,171	11.0	7.0	8.4	15.0	19.0
Batchelor's	2,106	13.5	8.0	10.0	17.6	24.0
Other[1]	62	16.7	9.6	12.0	25.0	30.0
20–24 years	45	7.8	6.0	7.0	8.4	9.0
25–29 years	754	8.4	6.3	7.3	9.4	11.0
Ph.D.	170	8.8	7.3	8.0	10.3	11.7
Master's	361	8.0	6.0	6.9	9.1	10.6
Bachelor's	223	8.4	6.6	7.5	9.2	10.6
30–34 years[2]	1,650	9.7	7.1	8.2	11.7	14.0
Ph.D.	653	9.6	7.9	8.5	11.5	14.0
Master's	669	9.2	6.6	7.5	11.0	13.5
Bachelor's	326	10.0	7.2	8.6	12.5	15.0
35–39 years[2]	1,704	11.5	7.9	9.5	14.0	17.4
Ph.D.	754	11.0	8.2	9.5	13.8	17.0
Master's	577	11.0	7.3	8.7	14.0	17.4
Bachelor's	366	12.5	8.5	10.8	15.0	18.0
40–44 years[2]	1,889	13.0	8.5	10.4	16.5	20.0
Ph.D.	972	12.5	8.9	10.2	16.0	20.0
Master's	516	12.8	7.5	9.5	16.0	19.7
Bachelor's	395	15.0	10.0	12.1	18.0	22.0
45–49 years[2]	1,472	14.8	9.0	11.5	18.5	25.0
Ph.D.	738	13.6	9.0	11.0	17.5	22.0
Master's	364	14.2	7.9	10.5	18.3	23.0
Bachelor's	357	17.0	10.5	14.0	22.0	30.0
50–54 years[2]	1,020	15.0	9.3	11.7	18.8	25.0
Ph.D.	505	14.3	9.3	11.0	17.4	22.0
Master's	298	14.7	8.7	11.0	18.0	24.5
Bachelor's	200	17.2	10.5	13.6	22.0	30.0

[1] Less than a bachelor's degree, except 2 with professional medical degree at ages of 35–39 and 40–44 respectively. Salary data relate solely to the 60 with less than a bachelor's degree.
[2] Includes "other" degrees, not shown separately.

TABLE 6—(*Continued*)

Age group Degree	Number reporting degree	Thousands of dollars of reported salary				
		Median	Lowest decile	Lower quartile	Upper quartile	Highest decile
55–59 years[a]	681	15.0	9.3	12.0	19.0	25.0
Ph.D.	331	15.0	9.5	12.0	18.5	24.0
Master's	210	14.5	7.5	10.4	18.0	25.0
Bachelor's	135	17.0	10.4	12.5	22.1	32.0
60–64 years[a]	406	15.0	9.0	12.0	19.0	25.0
Ph.D.	240	15.0	9.9	12.0	18.2	21.3
Master's	95	13.5	8.0	10.4	17.0	23.5
Bachelor's	64	20.0	9.0	13.6	25.0	33.0
65–69 years[a]	218	14.5	9.5	12.0	18.0	22.0
Ph.D.	146	15.0	10.2	12.5	18.2	21.0
Master's	51	12.0	7.7	9.4	15.5	19.0
Bachelor's	17	[a]	[a]	[a]	[a]	[a]
70 years and over	29	12.0	8.0	8.8	16.0	20.0
No report of age	15	[a]	[a]	[a]	[a]	[a]

[a] Not shown because less than 25 persons represented.

sharply curtailed numbers who remained in full-time professional employment had the highest average salary of all—$20,000.[55]

5. *Relationship between Professional Experience and Age*

In addition to reporting his age, each National Register respondent was asked to make an independent assessment of how many years of his life he considered to have represented work of a professional character.[56] Although the resulting data on experience are less than ideal in some respects,[57] they provide a measure of a characteristic

[55] Data were not available for further analysis of salaries by the type-of-employer characteristic crossed with *age* and academic degree. However, the type-of-employer characteristic is used as a control on the subsequent analysis of *years of professional experience* in relation to salaries by academic degree (III-9).

[56] Questionnaire item No. 17. See Technical note at the conclusion of this report.

[57] (1) The professional experience in question did not necessarily consist of work in the profession of economics—especially in the case of the 2,450 of the 12,143 persons who were classified as economists although they had chosen some other profession as the one with which they preferred to be identified. (See I-1, above).

(2) Respondents were given no instructions to aid them in interpreting "professional" and "work".

(3) The rate of non-response (4 percent) was somewhat higher than in the case of the other questionnaire items (see last column, Table 7, below). (The median years of ex-

TABLE 7—NUMBER OF ECONOMISTS, BY YEARS OF EXPERIENCE AND OF AGE
(Full-time professionally employed economists, only)

Age group (years)	Total	Years of professional experience: Less than 2	2–4	5–9	10–14	15–19	20 or more	No report
Total	9,981	250	1,101	1,789	1,891	1,554	3,003	393
20–24	45	24	13	1	—	—	—	7
25–29	758	121	405	184	—	—	—	48
30–34	1,657	77	497	825	197	3	—	58
35–39	1,712	18	125	535	780	194	1	59
40–44	1,906	6	36	161	652	761	235	55
45–49	1,482	1	15	48	183	433	751	51
50–54	1,035	1	5	19	52	119	784	55
55–59	696	1	2	10	18	27	612	26
60–64	423	1	—	3	6	8	382	23
65–69	223	—	1	1	2	4	206	9
70 and over	29	—	—	—	—	1	27	1
No report	15	—	2	2	1	4	5	1

which logically should be more closely related to salary structure than is age and which, in fact, turned out to be the most pervasive single salary-differentiating factor among the six here examined.

Table 7 shows that years of age and years of professional experience are not identical characteristics. While professional experience began after age 20 in practically all cases, a variety of ages was represented by each group who had had a similar number of years of experience.

perience of the 9,588 reporting both full-time salary and years of experience (14.4 years) did correspond closely with the median of 14.5 years for all 11,269 of those reporting experience out of the 12,143 registered economists.)

(4) More serious than the above was a defect in tabulation procedure which left 30 percent of the responses in the open-end category of those with 20 or more years of experience. (Apparently the tabulation planners did not realize how durable economists are!) The group with 20 plus years of experience included 89 percent of those 55 years old and over (Table 7, below). As it was after 55 years that the age-salary relationship turned negative, one conclusion of the present report—that experience does bear a continuously positive relationship to salary while age does not—may depend on a sheer lack of detailed information as to those with 20 or more years of experience.

Conversely, economists in the same age group differed in the length of their professional experience.

The age-experience data did show a central tendency, so that a considerable proportion of those in any one five-year age group was to be found within the bounds of two five-year experience groups and, conversely, most of those in a given experience group were those whose ages fell within the span of two or three five-year age groups. This combination of coincidence and variability permits a simple analysis of the relative influence of age and experience on salary for groups sufficiently representative of both factors to be significant.[58]

Years of age and years of experience were identical, of course, for all those economists whose professional experience had begun at the same age and had been either continuous thereafter or had suffered the same amount of interruption. The question explored here is whether age or experience is more closely associated with salary level when the two characteristics diverge. The superior association between professional experience and salary is illustrated by Table 8, which presents the median salaries for those combinations of age and professional experience which were selected and bracketed in Table 7.

TABLE 8—SALARIES FOR SELECTED COMBINATIONS OF EXPERIENCE AND AGE

Age group (years)	Median salary ($000's)						
	Year of professional experience						
	Less than 2	2–4	Under 5 (estimate)[1]	5–9	10–14	15–19	20 or more
25–29	7.7	8.2	8.1				
30–34	8.1	8.6	8.5	10.0			
35–39				10.3	12.5		
40–44					11.5	14.0	
45–49						13.4	17.0
50–54							15.7
55–59							16.0

[1] Median salaries in the first two columns weighted by numbers of economists in each of the two experience groups (see Table 7).

Among the economists with less than five years of professional experience, those who were 30-34 years of age had average basic salaries which were $400 above those who were five years younger. At the same time, among the economists who were also 30-34 years of age, those

[58] Of all the ages and years of experience shown in Table 7, 72.5 percent are contained within the comparison range as bracketed. They range from 69.4 percent of those 25-29 years of age to 87.9 of those 55-59 years of age, and from 81.9 percent of those with 2-4 years of experience to 71.5 percent of those with more than 20 years of experience.

who had had five more years of experience (five to nine years, rather than less than five years) had an average salary advantage of $1,500. Using this method of comparison, the following summary may be made from data in Table 8 of the salary advantage or disadvantage associated with each five-year increase in age or in years of experience, starting with each of the indicated base levels of age and experience:

Base Level Combination of Age and Experience:		Gain or Loss (−) in Median Salary of Economists having 5 more years of:	
Age	Experience	Age	Experience
25–29	Under 5	$ 400	—
30–34	Under 5	—	$1,500
30–34	5–9	300	—
35–39	5–9	—	2,200
35–39	10–14	0	—
40–44	10–14	—	1,500
40–44	15–19	− 600	
45–49	15–19	—	1,800[1]
45–49	20 or more	−1,300	—
50–54	20 or more	+ 300	—
55–59	20 or more	−1,000[2]	—

[1] Estimated at one-half the gain of $3,600 of those with 20 or more years of experience over those with 15–19 years' experience, on the assumption that the median years of experience was 27 years for the whole group having 20 or more years. On such assumption, this 20+ group would have had 10 more years of experience than those with 15–19 years and the total gain of $3,600 would be cut in half to represent the approximate gain for a 5-year increase in experience.

[2] Median salary for age group 60–64 years, with 20 or more years of experience, $15,000.

Generalizing further from these findings, it may be said that for economists under 35 years of age, five more years of age provided an advantage in average annual salary of $300 to $400 over those with a similar amount of professional experience. Beyond age 40 (with the exceptions of those aged 55-59), additional age conferred no average salary advantage (experience being equal) and beginning at age 45 there was a strong, negative relationship between advancing age and annual salary. In contrast, additional years of experience—at least up to a total of 20 years—had a strong positive relationship to average salaries at all age levels, so that five more years of experience of those in any given age group was associated with an advantage of between $1,500 and $2,200 in average annual salary.

These findings give substance to the logical supposition that the number of years of professional experience would be a very strong, if not the strongest, single influence on differences in the salaries between economists. It follows that any refined assessment of each of the other elements of the economists' salary structure needs, if possible, to take account of the relative number of years of professional experience of

the two or more groups of economists whose relative salaries are to be compared.[59]

6. *Professional Experience, Considered Separately*

Table 9 shows the results when economists' salaries are classified according to the single factor of years of professional experience. For example, economists at the beginning of their careers (less than two years' experience) had median salaries of $7,800 in 1964, with one-tenth of them receiving $10,000 or more. In contrast, those with 20 or more years of experience averaged $15,000, the lower and upper tenths of this most experienced group falling at the $10,000 and $25,000 points of separation. These summarized data include, of course, all the economists, whatever the types of their employers, their academic degrees, their ages, etc.

The average salary advantage of each additional year of experience, as shown in Table 9 (see p. 44), may be summarized as follows:

Years of Experience		Additional salary per year of additional experience	
From	To	Additional dollars of salary	Per cent increase in salary
Under 2	2–4	$300	3.8%
2–4	5–9	400	4.8
5–9	10–14	400	4.0
10–14	15–19	300	2.5
15–19	20 and over[60]	250	1.9

Economists who had the longer periods of professional experience not only received higher salaries than those with shorter periods of experience, but this experience-salary relationship was also one of remarkable persistence and regularity. In the whole range from those who had less than 2 years of experience by 1964 to those with more than 20 years of experience, average salaries were higher for each group of those with the larger number of years of experience. The incremental salary value of each year of experience declined, as would be expected as length of service increased beyond five years, but within

[59] The factor of experience may be somewhat over emphasized in this survey because of the criteria for selecting those who were defined as "economists." It will be recalled (Section I-1, above) that respondents not having a Master's degree or higher were considered to be professional economists if they had "equivalent experience." Those registered who had a Bachelor's degree or less tended to be those with relatively long periods of experience and, in fact, tended to be the relatively high salaried employees in industry or business.

[60] Assuming that the open-end group of those with over 20 years of experience had a median of 27 years of experience, that is, 10 more years than those with 15-19 years of experience.

TABLE 9—SALARIES OF ECONOMISTS BY YEARS OF EXPERIENCE
(Median, decile, and quartile full-time salaries; professional experience only)

Years of professional experience	Number of economists: Total registered[1]	Number of economists: Salary reported[2]	Thousands of dollars of reported salary: Median	Lowest decile	Lower quartile	Upper quartile	Highest decile
Total employed	12,060	9,981	12.0	7.8	9.3	16.0	20.0
Total reporting years of experience	11,269	9,588	12.0	7.8	9.3	16.0	20.0
Under 2	357	250	7.8	6.0	6.9	9.0	10.0
2–4	1,427	1,101	8.4	6.5	7.4	9.7	11.0
5–9	1,984	1,789	10.0	7.4	8.5	12.0	14.6
10–14	2,069	1,891	12.0	8.5	10.0	15.0	18.0
15–19	1,719	1,554	13.5	9.0	11.0	16.7	20.4
20 and over	3,713	3,003	16.0	10.0	12.5	20.0	25.0

[1] Excluding a total of 83 on active military duty, of whom a total of 44 reported their basic annual salaries.

[2] Excluding: unknown employment status, students, not employed, and employed nonprofessionally; part-time employment.

the range for which data are now available. The increments were always positive.

The relation of median salaries to experience, just shown, applies also, as a general pattern, to the salaries of the lowest and highest paid tenths and quarters of the economists in successive experience groups. The more detailed analysis which follows (III-7,8,9) will show a similar positive but incrementally declining salary value of experience for each subdivided group of economists, but will also show different rates of salary increment for economists with different academic degrees and for those engaged by different types of employer.

7. *Professional Experience and Academic Degree*

So strong was the relationship of salary to length of professional experience that it modified considerably even the relation of salary to the level of academic degree. Indeed, those economists who had in common a professional experience of five to nine years all had an average salary of $10,000, whether they were Ph.D.'s, Masters or Bachelors (Table 10).

For those economists within each class of academic degree, Table 10 shows that the longer their experience the higher were their salaries—

TABLE 10—SALARIES OF ECONOMISTS BY DEGREE AND YEARS OF EXPERIENCE

(Median, decile, and quartile full-time salaries on professional work, related to years of professional experience)

Degree Years of experience	Number of economists		Thousands of dollars of reported salary				
	Total registered[1]	Salary reported[2]	Median	Lowest decile	Lower quartile	Upper quartile	Highest decile
Total registered	12,060	9,981	12.0	7.8	9.3	16.0	20.0
Reporting degree	11,918	9,883	12.0	7.8	9.3	16.0	20.0
Reporting experience	11,269[3]	9,588[4]	12.0	7.8	9.3	16.0	20.0
Reporting degree and experience	11,140[5]	9,492[6]	12.0	7.8	9.3	16.0	20.0
Ph.D. degree	4,835	4,387	12.1	8.4	9.8	16.0	20.0
Under 2 years	85	81	8.5	7.0	7.7	9.8	10.5
2–4	467	432	9.0	7.4	8.0	10.1	12.0
5–9	843	812	10.0	8.0	8.9	12.1	15.0
10–14	917	867	12.0	8.8	10.0	15.0	18.0
15–19	839	797	12.8	9.2	10.8	16.5	20.0
20 and over	1,684	1,398	15.4	10.0	12.5	19.0	24.0
Master's degree	3,786	3,019	11.0	7.0	8.4	15.0	19.0
Under 2 years	226	138	7.3	5.8	6.5	8.5	9.5
2–4	714	473	8.0	6.0	7.0	9.2	10.5
5–9	760	638	10.0	6.9	7.8	11.9	14.0
10–14	690	620	12.0	7.9	9.6	14.5	17.0
15–19	445	386	13.8	8.0	10.5	16.3	20.0
20 and over	951	764	15.0	8.8	12.0	19.0	25.0
Bachelor's degree	2,431	2,025	13.5	8.0	10.0	17.5	24.0
Under 2 years	45	30	7.2	5.8	6.0	8.2	9.0
2–4	238	193	7.8	6.5	7.2	8.5	10.0
5–9	367	329	10.0	7.5	8.7	12.0	15.0
10–14	442	387	12.6	9.2	11.0	15.0	17.5
15–19	398	344	14.8	10.0	12.0	17.4	21.5
20 and over	941	742	17.4	11.2	14.0	22.3	30.0

[1] Excluding a total of 83 on active military duty, of whom a total of 44 reported their basic annual salaries.

[2] Excluding: unknown employment status, students, not employed, employed non-professionally, and part-time employees.

[3] Including 129 persons who did not report degree.

[4] Including 96 persons who did not report degree.

[5] Including 86 persons with less than a bachelor's degree, and 2 with professional medical degrees.

[6] Including 59 persons with less than a bachelor's degree, and 2 with professional medical degrees.

not only on the average but for each proportionate salary group (lowest-paid one-tenth, etc.) The following data taken from Table 10 also show that the salary advantage of those with longer experience was the smallest for the Ph.D.'s and the largest for the Bachelors':[61]

Years of Experience	Ph.D.'s	Masters	Bachelors
Less than 2	$ 8,500	$ 7,300	$ 7,000
2–4	9,000	8,000	7,800
5–9	10,000	10,000	10,000
10–14	12,000	12,000	12,600
15–19	12,800	13,800	14,800
20 and over	15,400	15,000	17,400

As between those with different academic degrees, relative overall salary averages were affected, not only by the salary value of additional experience to each group, but also by the differing lengths of experience as between the members of each degree group. In particular, the relatively low average salary of the Master's was attributable in part to the fact that the Master's had had an average of only 12.1 years of experience in 1964, as compared with 15.0 years among the Ph.D.'s and 16.0 years among the Bachelor's.

8. *Professional Experience and Type of Employer*

Each of the leading types of employer paid economists higher salaries in direct relation to the length of their experience (Table 11). Nevertheless economists' salaries are seen to remain sharply differentiated between types of employer, even after allowance is made for the fact that those employed by the Federal government in 1964 had had an average of 16.4 years of experience, compared with 14.9 years for those in industry and 13.4 years for those in educational institutions. If each of these three employment groups had had the same 14.5 years of professional experience as the average for those in all types of employment, the adjusted median salaries would have been approximately the following:[62]

	Table 11	Adjusted
Educational institutions	$10,100	$10,400
Federal government	13,600	13,000
Industry or business	14,400	14,300

[61] These amounts of increase were greatly influenced, in turn, by the distributions of the different degree holders among types of employer. (See III-2, above and III-7, 8, below).

[62] Assuming one year's professional experience to have been worth about $300 in salary at the levels of experience here considered. (Compare the conclusions of Section III-6, above.)

TABLE 11—SALARIES OF ECONOMISTS EMPLOYED FULL-TIME BY EDUCATIONAL INSTITUTIONS, THE FEDERAL GOVERNMENT, AND INDUSTRY OR BUSINESS, BY YEARS OF EXPERIENCE

(Median, decile, and quartile full-time salaries for professional work, related to years of professional experience)

Type of employer / Years of experience	Number of economists		Thousands of dollars of reported salary				
	Total registered[1]	Salary reported[2]	Median	Lowest decile	Lower quartile	Upper quartile	Highest decile
Total registered	12,060	9,981	12.0	7.8	9.3	16.0	20.0
Total reporting experience	11,269	9,588	12.0	7.8	9.3	16.0	20.0
Reporting experience: 3 types of employer	9,726	8,650	—	—	—	—	—
Educational institutions[3]	4,807	4,299	10.1	7.2	8.5	13.0	16.3
Less than 2 years	180	127	7.6	6.0	6.5	8.6	9.4
2–4	799	615	8.0	6.2	7.0	9.0	10.0
5–9	969	882	9.0	7.0	8.0	10.5	12.0
10–14	828	780	10.5	7.8	9.0	12.5	15.0
15–19	746	715	11.5	8.3	9.6	14.0	16.5
20 years and over	1,285	1,180	13.4	8.9	10.6	16.4	19.0
Federal government	1,213	1,176	13.7	8.5	10.3	16.5	18.2
Less than 2 years	40	39	8.0	5.9	7.0	8.9	10.3
2–4	127	119	9.0	7.0	8.0	10.3	11.7
5–9	188	182	10.3	8.4	9.0	12.1	14.2
10–14	206	203	12.9	10.0	10.6	14.5	16.5
15–19	165	159	14.0	10.6	12.1	16.2	18.2
20 years and over	487	474	16.3	12.1	14.0	18.0	19.0
Industry or business	3,706	3,175	14.4	8.8	11.0	19.5	25.0
Less than 2 years	61	52	8.0	6.5	7.2	8.8	9.6
2–4	312	259	8.9	7.0	7.9	10.0	12.0
5–9	624	573	10.8	8.4	9.5	13.0	17.0
10–14	805	723	13.6	10.0	11.5	16.5	20.0
15–19	612	531	15.6	11.0	13.0	20.0	25.0
20 years and over	1,292	1,037	20.0	12.0	15.0	25.0	35.0

[1] Excluding a total of 83 on active military duty, of whom a total of 44 reported their basic annual salary.

[2] Excluding: unknown employment status, students, not employed, employed non-professionally, and part-time employees.

[3] Including salaries paid for 9-10 months, as well as for 11–12 months.

Between the leading types of employer, the salary advantage of additional experience was generally the greatest for the economists employed by industry and generally the least for those employed by edu-

cational institutions, as is shown by the following incremental values calculated from the data in Table 11:[63]

Years of experience		Additional salary per year of additional experience					
		Additional dollars			Percent addition		
From	To	Education	Fed. Gov.	Industry	Education	Fed. Gov.	Industry
Under 2	2–4	200	500	450	2.6	6.3	5.6
2–4	5–9	250	325	475	3.1	3.6	5.3
5–9	10–14	300	520	560	3.3	5.0	5.2
10–14	15–19	200	220	400	1.9	1.7	2.9
15–19	20 and over	190	230	440	1.7	1.6	2.8

The salary increments in industry when expressed as percentages are the most regular; those of the Federal government are the most unstable, exceeding even those of industry at first but in percentages falling even below those of educational institutions as experience became longer than 14 years.

Because additional experience is rewarded at different rates, the relationship between the salaries paid economists by the different types of employer depend to some extent on the particular level of experience which is considered. For convenience in making comparisons, the median salary at each experience level is expressed as a ratio of that paid by each of the three chief types of employer to that paid by each of the other two employer types, as follows:

Years of Experience	Education as percent of:		Federal Govt. as percent of:		Industry as percent of:	
	Fed. Govt.	Industry	Education	Industry	Education	Fed. Govt.
Total (unadjusted)	74.3	70.1	134.7	94.4	142.6	105.9
Less than 2	95.0	95.0	103.5	100.0	105.3	100.0
2–4	88.9	89.9	112.5	101.1	111.1	98.9
5–9	87.4	83.3	114.4	95.4	120.0	104.9
10–14	81.4	77.2	122.9	94.9	129.5	105.4
15–19	82.1	73.7	121.7	89.7	135.7	111.4
20 or more	82.2	67.0	121.6	81.5	149.3	122.7

For economists with little work experience, nearly the same average salaries were paid by educational institutions, the Federal government and industry. The more characteristic salary differentials appeared in the range between five and nineteen years of experience where educational institutions paid economists 13 to 19 percent less than did the

[63] These data are computed in the same way as the averages for all employments, given in III-6, above.

Federal government,[44] and 17 to 26 percent less than did industry or business. The Federal government paid 14 to 22 percent *more* than educational institutions and 5 to 10 percent *less* than industry for economists who had between five and nineteen years of experience. Industry, correspondingly, paid 20 to 36 percent more than educational institutions and 5 to 11 percent more than the Federal government for economists at each of these levels of experience.

9. *Professional Experience, Academic Degree, and Type of Employer*

When economists' salaries are classified simultaneously by the factors of professional experience, academic degree, and type of employer, these salaries are seen to have ranged in 1964—from $5,500 or less for the lowest-paid one-tenth of those with a Master's degree and less than two years of experience, as employed by educational institutions, to $27,000 and more for the highest-paid one-tenth of the Ph.D.'s with 20 or more years of experience, as employed by industry or business. The detailed information on median, decile, and quartile salaries by degree and years of experience are provided in Appendix Tables M, N, and O, respectively for the educational institutions, the Federal government, and industry or business. Taken together, these three tables present 40 different groups of economists for which salary gradations by years of experience may be tested.

The universal positive association between salary and length of professional work experience is confirmed, even when each group of economists with the same type of employer and the same academic degree is considered separately. Furthermore the rule continues to apply when separate consideration is given to the lowest and highest-paid tenths and quarters of each successive experience group, having the same degree and type of employer. Of course, the particular dollar and percent increments in salary associated with additional experience may vary considerably, as has already been seen when different types of employer (but not different degrees) were examined (III-8 above).

The expected relationship of salary to level of academic degree actually emerges only when both years of experience and type of employ-

[44] Inasmuch as an academic year of 9 months is 18 percent shorter than the usual year of 11 months (net of vacation), it would be tempting to surmise that the *monthly* salaries paid by educational institutions were about the same as those paid by the Federal government. However, by no means all of the economists employed by the educational institutions were paid for only 9 months of duty per year. Data were not available to the committee for adjustment of the educational institution salaries to take account of differing periods for which "annual" salaries were paid. For those engaged in teaching activities (a major, but not exclusive part of those employed by educational institutions) some information is furnished below as to salaries for the 9-10 month and 11-12 month years (section III-11).

er are considered sinultaneously with academic degree.[65] Without exception, each type of employer paid higher salaries to its own economists who had a Ph.D. degree than to those with a Master's degree, ***when the comparison is made between those whose professional work experience had been of similar length,*** and, with only one exception[66] Masters' had higher salaries than Bachelors' ***at each level of experience*** (See Appendix Tables M, N, and O). The effect of standardizing for length of experience on salary differentials by class of academic degree may be illustrated by comparing the single group with 5 to 9 years' experience with the crude averages for economists at all levels of experience ("Total"):

	Dollars		Index (Ph.D.'s = 100.0)	
	Total	5–9 years' experience	Total	5–9 years' experience
Educational institutions				
Ph.D.'s	$11,000	$ 9,700	100.0	100.0
Masters	8,000	7,500	72.7	77.3
Federal government				
Ph.D.'s	14,800	12,100	100.0	100.0
Masters	12,600	10,000	85.1	82.6
Industry or business				
Ph.D.'s	18,900	14,300	100.0	100.0
Masters	13,100	11,000	69.3	76.9
Bachelors	14,000	10,000	74.1	60.0

Wide differentials between the salaries paid economists by the different types of employers are seen to persist, even when economists with the same academic degree and similar length of professional experience are considered separately.[67] The median salaries, just listed to illustrate differentials by academic degree, may be rearranged as follows to show type-of-employer salary differentials at the specific experience level of 5 to 9 years, compared with the unrefined averages which appear when all levels of experience are mixed together ("Total"):[68]

[65] The relatively low salaries of those with a Master's degree and the apparently inverted relationship of the Ph.D.'s and the Bachelor's salaries appeared to be only partly explained when age and type of employer were considered in this connection (III-2, 3), and when experience, but not type of employer, was considered (III-7).

[66] Industry paid an identical average of $15,000 to Masters and Bachelors of 15 to 19 years' experience.

[67] Insufficient numbers of Ph.D.'s in Federal government service and in industry, and of Master's in the government service were represented to permit comparison for those with less than two years' experience.

[68] For those with five to nine years' experience, the superiority of industrial salaries of the Ph.D.'s is seen to be less pronounced than for all economists ("Total"), but the superiority of the industrial salaries for the Masters is greater compared with the Federal

	Dollars ($000's)		Index (Ed. = 100)		Index (Fed. = 100)		Index (Ind. = 100)	
	Total	5-9 yrs. exp.	Total	5-9 yrs. exp.	Total	5-9 yrs. exp.	Total	5-9 yrs. exp.
Ph.D.'s								
Education	11.0	9.7	100.0	100.0	74.3	80.1	58.2	67.8
Federal govt.	14.8	12.1	134.5	124.7	100.0	100.0	78.3	84.6
Industry	18.9	14.3	171.8	147.4	127.7	118.2	100.0	100.0
Master's								
Education	8.0	7.5	100.0	100.0	63.5	75.0	61.1	68.2
Federal govt.	12.6	10.0	157.5	133.3	100.0	100.0	96.2	90.9
Industry	13.1	11.0	163.4	146.7	104.0	110.0	100.0	100.0

10. *Work Activity and Academic Degree*

A median salary of $16,100 was paid to those economists who were primarily engaged in management or administration activities (See Table 12). Thirty percent of the economists who reported their salaries to the 1964 National Register were in this management-administration group. Nearly one-third of the economists were primarily engaged in teaching economics, and three-fourths of those teachers were paid for a working year of nine or ten months. The economics teachers as a whole received an average (median) salary of only $9,700. Between the extremes of management and teaching were the salaries of those engaged primarily in research ($11,400)[69] and in production activities ($11,700).

Although the work activity classification is different from the classification by type of employer, the salary patterns of economists under the two classifications are closely related. To illustrate the *differences* between the functional classification by activity and the type of employer classification, it may be noted that the management or administrative function (economists' average salary, $16,100) was performed by economists who were employed by educational institutions (economists' average salary, $13,700), as well as in industry or business

government and not as great compared with that of educational institutions. Had a group with a larger number of years of experience been used for comparison, the superiority of the salaries paid by industry would have been greater than that shown, due to the large experience increments in salary provided by industry (Compare III-8, above.)

[69] The National Science Foundation tabulations generally distinguish between "basic research" and "applied research," but for the economics profession this distinction does not appear to be either conceptually or numerically significant. The separate median salary for economists engaged in basic research was $11,000, while that for those in applied research was $11,500. A small number classified as engaged in "design" activities were also included in the combined "research and development" work activity for the purpose of this report.

TABLE 12—SALARIES OF ECONOMISTS, BY WORK ACTIVITY AND DEGREE

(Median, decile, and quartile full-time salaries; highest academic degree)

Full-time professional work activity Degree	Number of economists		Thousands of dollars of reported salary				
	Total registered	Salary reported	Median	Lowest decile	Lower quartile	Upper quartile	Highest decile
Total employed	11,633[1]	9,981[2]	12.0	7.8	9.3	16.0	20.0
Research and development[3]	1,927	1,680	11.4	8.0	9.4	14.2	18.0
Management or admin.[4]	3,534	1,998	16.1	10.2	12.8	20.0	26.0
Teaching[5]	3,469	3,147	9.7	7.0	8.0	12.0	15.0
Production and inspection	1,215	1,076	11.7	8.0	9.3	14.5	18.0
Other work activity	1,091	843	13.0	8.1	10.0	17.0	22.0
No report of work activity	397	237	12.5	8.0	9.6	16.0	20.0
Total reporting degree[6]	11,497	9,883	12.0	7.8	9.3	16.0	20.0
Ph.D. degree	4,915	4,594	12.1	8.4	9.8	16.0	20.0
Research and development[3]	986	948	12.4	9.0	10.0	15.4	19.0
Management or admin.[4]	1,089	974	17.2	12.0	14.1	20.5	26.4
Teaching[5]	2,368	2,252	10.5	8.0	9.0	13.0	15.5
Production and inspection	72	66	15.0	10.5	12.7	19.0	22.0
Other work activity	234	188	14.0	9.8	12.0	18.6	24.0
No report of work activity	166	116	13.2	8.8	10.0	16.0	22.8
Master's degree	3,965	3,171	11.0	7.0	8.4	15.0	19.0
Research and development[3]	740	564	10.0	7.0	8.4	12.3	15.0
Management or admin.[4]	1,145	962	15.0	9.6	12.0	18.4	24.0
Teaching[5]	968	779	7.7	6.1	6.9	9.0	11.0
Production and inspection	425	374	11.4	8.5	9.6	14.0	16.8
Other work activity	545	421	12.4	8.0	9.6	15.0	20.0
No report of work activity	142	71	11.2	7.2	8.6	14.5	18.0
Bachelor's degree	2,531	2,106	13.5	8.0	10.0	17.6	24.0
Research and development[3]	181	154	10.6	7.8	9.0	13.8	16.7
Management and admin.[4]	1,197	981	16.0	10.0	12.5	20.0	28.8
Teaching[5]	108	96	7.8	6.3	6.9	10.0	15.0
Production and inspection	690	615	11.4	7.9	9.0	14.4	17.7
Other work activity	282	217	13.0	7.7	9.9	17.0	24.0
No report of work activity	73	43	12.5	7.0	8.8	17.5	20.0
Less than bachelor's degree	84	60	16.7	9.6	12.0	25.0	30.0
Management or admin.[4]	51	41	17.2	10.3	14.0	26.0	35.0
All other work activities	33	19	[7]	[7]	[7]	[7]	[7]

[1] Excluding 510 persons not employed composed of 175 students, and 335 economists available for full-time employment (one of the former and 6 of the latter did not report degree).

[2] Excluding: unknown employment status, employed students, employed non-professionally, part-time employed, and those on active military duty.

[3] Including persons not separately identified as between basic and applied research.

[4] Including administration of research and development.

[5] Including salaries for 9–10 months, as well as 11–12 months.

[6] Including 2 persons with professional medical degrees, 1 in applied research and 1 in administration of research and development.

[7] Not shown because less than 25 persons represented.

(economists' average salary, $14,400).[70] Research and development work was performed by economists for each type of employer. Teaching, while performed almost exclusively in educational institutions, was not the only work activity of economists employed by such institutions. Production and inspection work was, of course, only one of the work activities of economists employed by industry.

To illustrate the *similarities* of salary patterns by work activity and by type of employer, it may first be noted that the highest and next

[70] See Section III-1, above, for salaries by type of employer.

highest average salaries were paid for the two activities chiefly found in the highest salary-paying type of employment (management and production functions in industry). Research work, being distributed among all types of employer, commanded the third highest salary by-work activity. Teaching, the lowest paid work activity, was performed for the lowest-salary type of employer, the educational institutions.

A second similarity of salary patterns appears when salary by academic degree is considered, as in Table 12. For each work activity, as for each type of employer, the Ph.D.'s were paid the highest salaries, but those with no more than a Bachelor's degree generally had higher average salaries than those with a Master's degree, reflecting a longer average period of professional experience than characterized the Masters.[71]

Because of these and other similarities of salary pattern in relation to work activity and to the other characteristics of the economists, the work activity classification adds less to the explanation of the all-economist salary structure than the explanations already discussed. (Compare Section V, below, which shows the net relationship of salary to work activity to have been less pronounced than that to type of employer, years of experience, or academic degree, but more pronounced than the relationship of salary to age, sex, or field of special competance.)

11. *Teaching: The Academic-Year Problem*

Most of the teachers of economics are paid for nine or ten months of official duties, whereas most other economists are paid for a working year of between eleven and twelve months, exclusive of vacations in both instances. The question arises whether the shorter working year of the teachers may account for their exceptionally low "annual" salaries. The conclusion of the analysis which follows is that the exceptionally low average salary of the teaching economists did, indeed, reflect the effect of their shorter "academic year," but that, even after adjustment is made for the differences in working time, the compensation of teachers still ranks below that of the economists in any other broad class of work activity.

Teaching was, indeed, the only class of work activity of the economists for which the principal employer paid any substantial number of them for a working period of only nine or ten months.[72] In sharp con-

[71] Compare Section III-9, above.

[72] Of the 1,680 economists reporting their primary activity as research and development, 191 were paid salaries for a 9 or 10 month period. In no other work activity, except teaching, were there as many as 100 paid for as short a working year.

trast to all the other economists at least three-quarters of those primarily engaged in teaching did receive their 1964 salaries for a nine ten month working year:

Economists: teaching as *primary* activity	3,147
Salary for 9–10 months	2,348
Salary for 11–12 months	630
Salary basis not reported	169

One approach to comparison of academic year salaries with others is to assume that all the teachers would have been paid as much as those working for the full calendar year, if they had actually worked for the full year. To explore this approach, the Committee obtained from the National Science Foundation a special tabulation of teachers' median salaries, the results of which were as follows:[78]

Economists: teaching as a primary or secondary activity	$10,000
Salary for 9–10 months	9,500
Salary for 11–12 months	11,500
Salary basis not reported	10,500

The appropriate average salary for comparison with that of other economists, would seem, at first glance, to be $11,500, an average which is $100 higher than that for economists engaged in research. A second glance reveals, however, that the general salary average for the teachers covered by this special tabulation ($10,000) is $300 above the $9,700 average shown for teachers in Table 12. The reason is because the special tabulation included economists who reported "teaching" as a *secondary* activity, as well as those who reported it as a *primary* one. Thus, the total of those who did any teaching in 1964 included some economists whose primary activity was in research or administration. A rough adjustment for this difference in classification may be made by deflating the tabulated 11-12 month salary average by the factor of 0.97—that is by the ratio of the salary of those *primarily* engaged in teaching ($9,700) to that of those who did *any* teaching ($10,000). The result of this deflation is an estimated average 11-12 month salary for those with teaching as a primary activity of $11,155. It will be noted that this $11,155 is slightly ($245) *lower* than the average salary of those primarily engaged in research ($11,400), and, of course, much lower than the average salary of economists engaged in any of the other classified work activities.

These data, and the concepts behind them, leave much to be desired.

[78] The numbers of economists in each of these groups were not available at the time this report was prepared.

One would naturally suppose that economists engaged in both teaching and research would be paid about the same average salary per month, whether they were primarily occupied at any one time with one activity or another. Thus among those doing *any* teaching during the year, the ratio of the average salary of those engaged for nine or ten months ($9,500 to that of those engaged for eleven or twelve months ($11,500) was almost exactly the same as the ratio of nine to eleven (0.818 and 0.826). However comparisons derived from basic data which include those whose primary activity was in research or administration leave unresolved the question of what salary comparison should be made between those primarily engaged in teaching and those primarily engaged in other activities.

A more serious objection to comparisons involving the eleven to twelve month salaries is that they hinge on the compensation of an uncharacteristic minority of the teachers. The majority of teachers who are paid for an academic year often contend, with some reason, that the two or three months outside of the official academic year do not constitute a "vacation" in the usual sense, but rather that they are expected to devote at least part of that non-teaching period to training and preparation for the period of teaching itself. This view finds some support from the common academic practice of paying the teacher's salary in equal monthly installments throughout the calendar year, even though the teaching session is limited to nine or ten months. But, while some teachers devote their non-teaching months to professional preparation—often costly rather than remunerative to them—others do obtain additional paid employment during the "vacation" period. Comparisons based on full calendar year salaries really imply that those teachers who are on duty for their primary employer for only nine or ten months are able to obtain equally remunerative employment during the remainder of the year.

An alternative approach to the comparison of the academic year salaries with those paid for non-teaching activities, is to consider income rather than basic salary. Because the foregoing comparisons of salaries were so inconclusive, the Committee obtained an additional special tabulation of gross professional incomes for the 9,491 economists for whom both basic salary data and gross professional income had been obtained.[74] The following averages of gross professional income in 1964 consisted of arithmetic means, rather than the median salaries used elsewhere in this report:

[74] These data on gross professional income were requested of respondents by the National Register questionnaire, but no general tabulations of them have, as yet, been made by the National Science Foundation. Dr. Emanuel Melichar, author of Section V of this report, kindly furnished the results here quoted, as obtained from the original tapes furnished to him by the NSF, at the request of the Committee.

	Mean Income
Total, economists reporting salaries and income	$15,487
Teaching	13,093
Management or administration	19,778
Research and development	13,652
Production and inspection	13,478
Other work activity	15,752
Work activity not reported	15,316

The teaching economists did supplement their basic salaries to a greater extent than did economists as a whole. The teachers' mean income ($13,093) exceeded their median salary ($9,700) by 35 percent, whereas the corresponding difference for the total of salary-reporting economists amounted to 29 percent ($15,487/$12,000 = 1.29). However, the average (mean) gross professional income of those primiarily engaged in teaching ($13,093) was still $559 below that of the research economists ($13,652). Judged on the basis of average income, then, the compensation of teachers from all professional sources was, indeed, less than that of economists in any other class of work activity. However, the gross income of the teachers was not very much lower than that of economists engaged in production or in research.

12. *Fields of Special Competence*[75]

Those economists who reported that their education and experience had given them their greatest special competence in the field of economic history or history of economic thought received a median salary of $9,800 in 1964. On the other hand, the group with greatest competence in the field which included industrial organization, government and business, and industry studies received a median salary of $13,000. Much of the difference in average salary along this $3,300 range may be attributed to differences in the length of work experience of the individuals who had been trained in each field and to differences in their distributions by type of employer. When such independent salary-influencing factors are taken into account, the particular field of special competence itself did not generally appear to have any important separate relationship to current salary.

Table 13 lists twelve groups from among the 9,981 economists who reported full-time salaries to the 1964 National Register. Eleven of these groups were composed of the individuals who reported, respec-

[75] See Section I-2 above, for greater detail as to the content of each of the fields of economics for which salaries are shown in this section, and for the total number of registered economists in each field. Additional detail on the numbers from each field reporting salaries and the tabulations of those salaries as classified by other characteristics will be found in Appendix Tables A, E and J-L.

tively, the same major field of economics as their field of greatest professional competence. The eleven specified fields are listed in ascending order of median salary—followed by a small group (1 percent of the total) of those who reported any economic specialty which was not included under any of the eleven designated fields. For each of the twelve specialty groups, Table 13 shows the number of economists reporting salary, the median salary and measures of three of the characteristics related to salary levels, namely: (a) the median number of years of professional experience, (b) the median age, and (c) the percent of the number reporting who were employed by the lowest-salary type of employer (educational institutions) and the highest-salary type (industry or business).

Economists from the first five of the twelve major fields, as listed, were in groups with median salaries of between $9,800 and $11,400—all of these averages being below the total economists' median salary of $12,000:

1. Economic History and History of Economic Thought
2. General Economic Theory
3. Economic Statistics
4. Land Economics
5. Monetary and Fiscal Theory and Institutions.

Table 13, column 3, shows that the economists from all but the first of these five separately low-salary fields had less than the general average of 14.4 years of professional experience. Moreover, the proportion of those in all five fields who were employed by educational institutions was much greater than the general average (53 to 90 percent, compared with a general average of 44.6 percent—see column 5). Conversely, one-sixth or less of these groups were employed by industry or business, in contrast to the one-third of all the economists who worked for this high-salary type of employer (column 6). Given the experience and type-of-employer characteristics of these 5 groups of economists their average salaries would have tended to be relatively low, regardless of field of special competence.[76]

Median salaries between $12,000 and $12,500—within $500 per year of the all-economists' median salary—were received by the economists from four additional fields:

[76] Median ages are shown in Table 13, column 5, mainly as a matter of general information. Section III-5, above, has shown that age, by itself, had less influence on salary than did length of professional experience. Indeed, between the ages of 55 and 64, the relationship between age and salary was neutral and beyond age 65 age was inversely related to salary. Median age data are not, of course, sufficient to show what proportions of the individuals in each field were at different particular age levels.

6. Labor Economics
7. Welfare Economics, Population, and Standards of Living
8. Economic Systems, Development, and Planning
9. International Economics

In these groups with close to the general average salaries, the outside influences on salary either had about average impact or else diverse influences tended to offset each other. The average of 16.7 years' experience of the labor economists would tend to make their salary level relatively high, but the fact that 61 percent of the labor economists were employed by educational institutions would tend to keep salaries relatively low. Group 7, (welfare, etc.) had had the longest average experience (median, 18.5 years) and less than an average proportion of the economists from this field were employed by educational institu-

TABLE 13—FIELDS OF SPECIAL COMPETENCE OF ECONOMISTS

(Number, median salary, years of experience, and age, and percent employed by educational institutions and by industry or business, of economists from each field)

Field of Special Competence	Number reporting salary[1]	Median salary ($000's)	Median experience (years)[2]	Median age (years)[3]	Percent of reporting no. In educat'l institutions	Percent of reporting no. In industry or business
Total	9,981	12.0	14.4	42.1	44.6	33.3
Economic history; history of thought	205	9.8	14.7	43.6	89.8	2.4
General economic theory	1,204	10.6	13.0	41.0	67.1	16.5
Economic statistics	389	11.0	11.4	38.2	53.0	14.7
Land economics	1,066	11.3	13.5	41.7	56.9	5.8
Monetary	800	11.4	13.5	42.5	63.5	10.0
Labor economics	546	12.0	16.7	45.6	61.2	3.5
Welfare, population, etc.	139	12.0	18.5	45.7	41.7	7.9
Economic systems, development and planning	734	12.1	13.1	41.4	45.0	13.2
International economics	450	12.5	14.1	43.0	48.4	13.1
Business finance and administration; marketing; accounting	3,860	13.0	15.1	42.1	25.9	66.5
Industrial organization, government and business; industry studies	662	13.0	15.0	43.8	42.1	26.9
Economics, other	106	13.0	16.4	43.5	41.5	19.8

[1] Excluding: unknown employment status, students not employed, employed non-professionally, part-time employed.

[2] Based on a total of 9,588 respondents who reported salary and years of experience.

[3] Based on a total of 9,966 respondents who reported salary and age.

tions. However, the relatively large proportion of women in Group 7 (see Section IV below) may have been a salary-depressing factor in this case. The experience and type-of-employer factor shown for Groups 8 and 9 might have led one to expect below average salaries. Exceptionally strong demands for talent in the areas of economic development and of international economics may have had an exceptionally favorable impact on the salaries of these two particular groups.

The economists from the two business-oriented fields, which together account for 45 percent of all economists reporting salary, each received a median salary of $13,000—$1,000 a year above the median salary of all the econmists. These relatively lucrative fields are:

10. Business Finance and Administrations; Marketing; Accounting
11. Industrial Organizations; Government and Business; Industry Studies

Economists from both of these fields had more than the average length of professional experience, and the proportion of those from Group 10 who were employed by industry or business was twice as great as the average proportion. Group 11, it may be noted, included the "government and business" specialty, involving highly-sensitive "anti-trust" policies. While the proportion of economists from Group 11 employed by industry was not high, an additional 17.7 percent of them (not shown in Table 13) were employed by the Federal government at a median salary of $15,000. When these are added to the 26.9 percent of the group who were employed by industry or business (at a median salary of $16,000), the outcome is that 44.6 percent of those in Group 11 worked for employers who did pay exceptionally high salaries for these particular talents.

A particular field of special competence may provide, or fail to provide, good opportunities to qualify for and retain work for the type of employer which pays relatively high salaries. In this sense, an economist's special field of greatest competence was related to the probable level of his salary. It cannot be said, however, that 1964 salaries were uniquely related to competence in one economic field rather than another, when the other influences on salaries were approximately the same.

IV. *Women Economists*[17]

Because employed women economists are only a small minority of the profession as here defined, the women's salaries cannot have any

[17] Mrs. Alice Hanson Jones, Washington University, a member of the Committee, prepared the draft analysis for this section, to supplement Section I-3, above, where very general comparisons between women and men economists are introduced.

considerable influence on the economists' overall salary structure (as discussed in Sections III, above and V, below). Precisely because of their relatively small numbers, however, the women's own patterns of employment and salaries would be obscured unless some separate consideration were given to them.

Of the 493 women, who made up 4 percent of the 12,143 economists,[78] a total of 322, or only 65 percent, are included in the salary analysis of this report as "economists reporting salaries in civilian full-time professional work." A negligible proportion of women economists (0.6 percent, compared with 3.8 percent of the men) were employed non-professionally, but a much higher proportion of the 493 women (10.7 percent) than of the men (1.5 percent) were part-time professional employees and hence excluded from the salary analysis. Furthermore, many more women proportionately (9.9 percent) than men (2.5 percent) were "not employed."[79] These data suggest that women who have gained professional status as economists tend to stay in the profession, insofar as they are employed, but that their family responsibilities tend to curtail partly or fully their paid participation to a greater extent than is the case with men. There were 34 registered women economists (6.9 percent of the total women, compared with 4.3 percent of the men) who were omitted from the salary analysis because they were students. This somewhat higher proportion of students among the women suggests that entrance of women into the economics profession may be on the increase.

The foregoing interpretation appears to be supported by a comparison of the age distributions for the men and women. In the age groups 20 through 29, and again from age 50 upwards, women were more heavily represented in proportion to their numbers then were the men. In the age groups, 30 through 39, and 45 through 49, women were relatively underrepresented, while in one age class, 40 through 44, they were found in the same poportion as the men. This can be confirmed by scanning the last column of the following tabulation for the age brackets in which the percent of women is greater or less than the overall average of 4.0 percent. The specific data from the 11,639 men and 483 women who reported age are as follows:[80]

[78] As defined in Section II-1, Table 1, above. Appendix Table C, which shows separate data for men and women corresponding to Table 1, provides the basis for the percentages given in this paragraph.

[79] See Section I-4, above, where the implied *unemployment* rate, excluding students, is stated to be 10.3 percent for women and 2.8 percent for men.

[80] See Section I-3, above, for comparison with the Census age distribution of "economists."

Age group	Percent of total		Percent women in each age group
	Men	Women	
Total	100.0	100.0	4.0
20–24	1.2	2.1	6.8
25–29	9.2	11.4	4.9
30–34	15.9	11.0	2.8
35–39	16.1	12.0	3.0
40–44	17.7	17.6	4.0
45–49	14.0	11.4	3.3
50–54	9.9	12.0	4.8
55–59	6.7	10.1	5.9
60–64	4.4	6.2	5.6
65–69	2.9	4.1	5.7
70 and over	2.0	2.1	4.2
	years	years	
Median age	42.0	43.7	—

As is generally the case under present-day American conditions, the women's participation rates in the economic profession are higher than those of men in the student years, generally lower in the family-rearing ages, and show an increase in the higher age brackets. In the case of women of 50 years or over, their children may be grown or the women may be widowed or separated; thus they are more able to engage in professional work than during the ages when women are more apt to be raising a family.

The median full-time salary reported by all the women economists together was $9,900, compared with the $12,000 median for the men, and an even greater spread appeared between the respective arithmetic means[31] of $10,500 and $13,800, respectively. The greater concentration of women at the lower end and of the men at the upper end of the salary range is shown by the following percent distributions of the 9,659 men and 322 women who reported their full-time professional salaries in 1964:

[31] All arithmetic means and salary distributions by intervals in this report (except those for AEA members and registrants) were supplied by Dr. Emanuel Melichar, from tapes covering the identical economists, as furnished to him by the National Science Foundation at the request of this Committee.

Salary class	Percent of total		
	Total	Men	Women
Total	100.0	100.0	100.0
Under $7,000	4.2	3.9	11.5
7,000–8,900	16.4	16.0	28.0
9,000–10,900	19.3	19.2	22.0
11,000–12,900	15.9	16.0	14.3
13,000–14,900	12.1	12.1	11.2
15,000–19,900	19.8	20.1	12.4
20,000 and over	12.3	12.7	0.6

There is a clear and strong difference between men's and women's salaries, as shown not merely by the overall averages and distributions, but also within nearly every subclassification of the economists which was available for this report. As an example, the following comparison shows the women's and men's median salaries in each of the six fields of specialization where sufficiently large numbers of women appeared to make comparison practicable. The six fields are here listed in descending order of their importance for the women, and the median years of work experience of each sex for each field are also shown:[82]

	Median salaries ($000's)		Median years of experience	
	Women	Men	Women	Men
All fields	9.9	12.0	17.9	14.3
Business finance, etc.	9.3	13.0	17.0	15.1
Labor economics	10.3	12.0	20 +	16.3
Monetary-fiscal	8.0	11.6	9.4	13.7
General economic theory	8.7	10.8	16.4	12.9
Population, Welfare programs, etc.	12.0	11.5	20 +	16.2
Economic systems and development	9.0	12.2	13.8	13.0

With the single exception of the Welfare field,[83] the women's median salaries were lower than those of men in each case, and in all but the Labor and Welfare fields, the spread was as great or even greater within the individual field than was the average spread of $2,100 ($9,900 versus $12,000).

The lower average salaries of the women economists, field for field, cannot be generally attributed to their relative lack of work experience. Except for those in the Monetary-fiscal field, the women had

[82] Similar contrasts are shown between the decile and quartile salary amounts in Appendix Table P from which the above data are extracted.

[83] In the case of the Welfare field, Appendix Table P shows that the lowest decile and the upper quartile and decile salary amounts were slightly lower for women than for men.

higher average numbers of years of experience than the men, and in four of the six cases the women's advantage in experience was substantial.[84] Thus the sex differential in salaries prevailed, even though the factor of experience worked in favor of the women's salaries—and in the Welfare field probably was influential in raising the women's median salary above that of the men. The age factor, however, reinforced the gross sex differential, with women more heavily represented in the low-salary, beginning age groups and in the plateau-salary and declining-salary groups from 50 years upwards.

Limitations of both the data and of space preclude a detailed treatment of the sex differential in salaries in relation to all combinations of each of the other factors which may be associated with salary differences between economists.[85] Even when all six of the other factors in salary differences are simultaneously taken into account, Section V of this report shows that the net salary differences associated with sex remain statistically significant. Lack of lifetime continuity of work, mentioned previously, may partially explain the lower average salaries of women than of men in the economics profession.

V. *The Net Influence on Economists' Salaries of Each of Seven Characteristics: A Regression Analysis*[86]

The analysis of Section III has demonstrated that the "true" net relationship between salaries and each of the other surveyed characteristics is often evident only after the effect of intercorrelation among the characteristics has been removed. In Section III, this problem was approached through cross-classification by the intercorrelated characteristics; here, resolution of the same problem is sought through least-squares multiple regression. Such regression analysis supplements the preceding work in the following ways: (1) all seven characteristics are considered simultaneously, so that all intercorrelations are taken into account; (2) the regression coefficients provide a concise quantification of the net relationships; and (3) measures are obtained of the relative importance and statistical significance of the net influences on salaries. In general, the regression results confirm the principal findings reported in Section III.

[84] Appendix Table Q shows the distribution of women in each of six of the twelve fields of specialization by years of experience.

[85] But see Appendix Tables R and S for some further detail.

[86] This chapter was written by Emanuel Melichar, Economist, Board of Governors of the Federal Reserve System, Washington, D. C. A more detailed report on the regression analysis, including a discussion of the techniques employed and of tests and limitations of the model, is available from the author on request.

1. *Relative Importance of the Characteristics*

Among the seven characteristics in the regression model, length of professional experience and type of employer together made the greatest contribution toward explanation of salary variation, followed closely by level of highest academic degree and the primary work activity. Age, sex, and economic specialty were found to exert relatively minor net influence. These rankings are based on coefficients of partial determination, shown in the first column of Table 14. The net contribution made by each characteristic was significant beyond the .01 probability level, according to the F-ratios also reported in Table 14.

TABLE 14—IMPORTANCE OF SELECTED CHARACTERISTICS IN EXPLAINING SALARY VARIATION

Characteristic	Net relationship		Gross relationship		Number of variables used
	Partial R^2	F-ratio	R^2	F-ratio	
Years of experience (separately for each of two employer groupings)	.085	65.50	.412	498.95	14
Level of highest degree	.081	218.45	.032	82.54	4
Primary work activity	.065	137.13	.235	612.82	5
Type of employer	.032	36.38	.169	224.70	9
Age of economist	.015	22.12	.226	415.42	7
Sex of economist	.015	154.59	.011	110.45	1
Specialty of greatest competence	.009	8.44	.040	37.40	11

Note: all relationships are significant at the .01 probability level.

In the regression analysis, each class of each of the seven characteristics was represented by a separate independent variable, with one exception introduced to take account of a major interaction between the influence of professional experience and type of employer. As was noted in Section III, the progression of salaries with additional professional experience was markedly greater in business than in educational institutions or in the Federal government. Further investigation showed that this effect persisted even after the other two important characteristics—primary work activity and level of highest academic degree—were both taken into account. In the regression model, therefore, two sets of variables were provided to measure the influence of professional experience: one for economists who worked for industrial, business, nonprofit, and other employers, or who were self-employed (Group I);

and the other for the economists employed by educational institutions and governments (Group II).

The logarithm of salary was used as the dependent variable in the model reported here. By regressing the characteristics on logarithms rather than on the actual salaries, their net influences are measured in proportional rather than absolute terms, and their cumulative effect is considered to be multiplicative rather than additive. Experimentation with these alternative models confirmed the *a priori* belief that the logarithmic form would better reflect the salary structure. The seven characteristics, represented in the logarithmic equation by 51 independent variables, explained 55 percent of the total variation among the logarithms of salaries and 41 percent of the variation among actual salaries. These coefficients of multiple determination (R^2) are statistically significant beyond the .01 probability level in an analysis based on 9,981 observations.

For comparison, Table 14 also presents the R^2 and F-ratio calculated for the gross relationship exhibited between salaries and each characteristic. (The gross relationship is defined as that found for a single characteristic when no other characteristics are taken into account.) For most of the characteristics, the net influence was less important than one would have assumed from study of the gross relationship only, as part of the latter was generally found to be attributable to other influences. This was particularly true of age, which was found to have only minor net effect after the length of experience was taken into account. On the other hand, the full influence of the level of academic degree emerged only in the net relationship, having previously been obscured by intercorrelations with type of employer and primary work activity. The strength of the salary difference between men and women also persisted unabated into the net relationship. This relationship was statistically highly significant, but not of much importance in explaining total salary variation because the women economists were few in number.

2. *The Nature of the Net Relationships*

The regression coefficients that quantify the net relationships are the heart of the regression results. Each coefficient is the net percentage salary difference from the geometric mean that was associated with membership of an economist in the class to which the coefficient applies. The results are charted in Figure 1 (pp. 66-67), over which shows the gross and net salary differences associated with each class of each characteristic. The chart also shows the proportion of the registered economists that fell into that class, to help one assess the relative

FIGURE 1. GROSS AND NET RELATIONSHIPS BETWEEN ECONOMISTS' SALARIES AND SPECIFIED CHARACTERISTICS

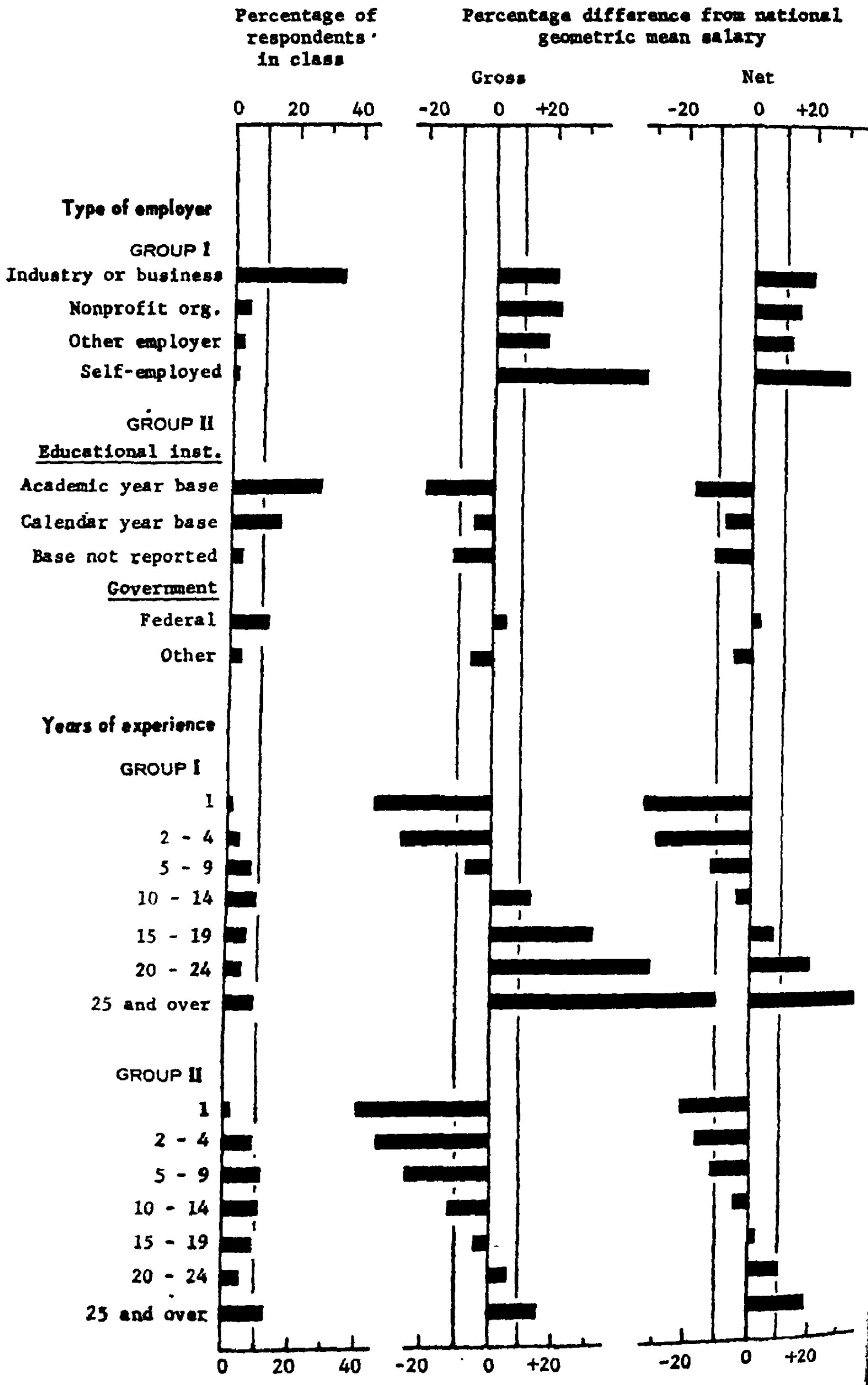
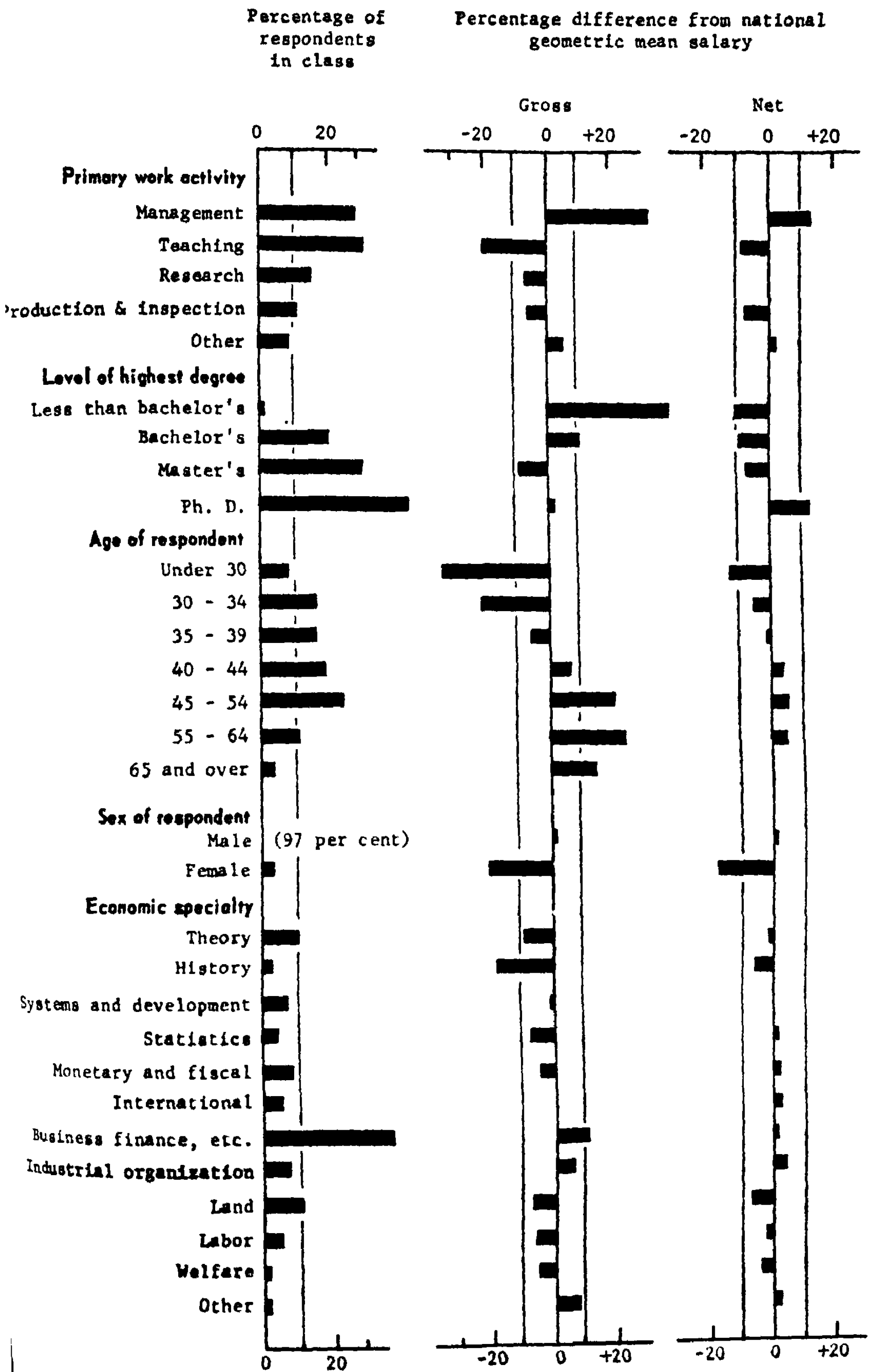


FIGURE 1. (CONTINUED)



aggregate importance of the salary differences shown. In each case that the difference between any two classes was large enough to be of much interest (5 percent or more), it is statistically significant at the .01 probability level.

As has been noted, separate net relationships between salary and years of experience were estimated for economists in each of two type-of-employer groups. In Group I, dominated by economists employed by business, the possession of 25 or more years of experience was associated with a net salary gain of 98 percent over those with only one year of experience. For Group II, comprised of the economists employed by educational institutions and governments, the comparable net gain was only 46 percent. The gain estimated for Group I represented an average annual net salary progression of about 2.25 percent over the period of approximately 30 years, while the net annual progression for Group II was about 1.25 percent. For both groups, however, the net annual rate of salary progression associated with experience was much higher in the early years and decreased steadily with more experience. For Group I, the net annual progression during the first seven years of experience averaged about 4.5 percent, but at about 25 years of experience the net annual progression was reduced to about 1.2 percent. In Group II, the comparable net annual gains were estimated at about 2 percent and 0.75 percent, respectively.[87]

For any two employer types who are either both in Group I or both in Group II, net salary differences may be cited without reference to years of experience. For instance, salaries in business tended to be 5 percent above salaries in nonprofit organizations. In educational institutions, salaries on a calendar year base tended to be 12 percent higher than those on an academic year base. Federal government salaries tended to be 10 percent above calendar-year salaries at educational institutions, and 24 percent above academic-year salaries.[88]

Salary comparisons between an employer type in Group I and one in

[87] In evaluating these net progressions of salary with additional years of professional experience, it should particularly be noted that the net influence of advancing age (to be discussed later) has been excluded as a result of its inclusion in the model as a separate characteristic.

[88] Tests of the model indicate the presence of a complex interaction that requires this finding to be qualified. Within the two primary work activities, management and research, to which comparisons between educational institutions and the federal government must be restricted, the differences cited appear to be overstated for economists with the Ph.D. degree and understated for those with a Master's degree. For example, for economists with the Ph.D. who are working primarily in research, there appears to be no significant difference between federal government salaries and calendar-year salaries at educational institutions. By the same token, the differences between federal government and business salaries for Ph.D. economists were actually wider than the over-all differences indicated in the next paragraph of the text.

TABLE 15—NET RELATIONSHIPS BETWEEN ECONOMISTS' SALARIES AND YEARS OF EXPERIENCE, BY SPECIFIED TYPE OF EMPLOYER

Years of professional experience	Type of employer: Educational institution, Academic year base	Educational institution, Calendar year base	Federal government	Industry or business
	Percentage difference from national geometric mean			
1	−35	−27	−19	−20
2–4	−31	−23	−15	−13
5–9	−27	−18	− 9	+ 4
10–14	−20	−11	− 2	+14
15–19	−16	− 6	+ 4	+28
20–24	−10	+ 1	+11	+43
25 and over	− 3	+ 8	+20	+59

Group II must be couched in terms of a specific length of professional experience. The process of combining the results for the two characteristics has been performed in Table 15 for the major employer types. Thus salaries for economists with one year of experience tended to be 10 percent higher in business than in educational institutions on a calendar year basis; but for economists with 25 or more years of experience, business salaries tended to be 47 percent higher. Similarly, for economists with one year of experience there was no net salary difference between employment by business and employment by the federal government, whereas economists with 25 or more years of experience tended to receive salaries 33 percent higher in business than in the federal government.

Fairly substantial net salary differences were also associated with differences in primary work activity, although the differences were much reduced by the simultaneous consideration of other characteristics. Economists with primary work activity of management tended to have salaries 14 percent above those in research and 24 percent above those in either teaching or production and inspection.[89] Economists in research, in turn, tended to have salaries 8 percent above those in the latter groups.

[89] An interaction found between primary work activity and years of experience, and which was not provided for in the model, requires that this finding be qualified. Among economists with little experience, those with primary work activity of management had about the same salaries as economists in other activities. On the other hand, for business-

The level of academic degree exerted a large net influence on salaries, as holders of the Ph.D. tended to receive salaries 22 percent above economists with the Master's degree and 24 percent above those with the Bachelor's. As this finding also indicates, there was no significant net salary difference between the latter two degrees. The net relationship thus indicated between salaries and level of degree differs markedly from the gross relationship, as the average salary of economists with the Bachelor's degree was higher than the average salaries of those with the advanced degrees. The analysis indicates, however, that this gross salary difference was attributable to characteristics other than the level of degree itself.

The net influence that age had on salaries after professional experience was taken into account was found to be rather small. Its major effect was among the young economists: given two economists similar in all characteristics except age, the salary of one in his late thirties tended to be 14 percent higher than the salary of one in his late twenties. The positive net progression of salary with age continued to approximately the age of 50, though at a much slower rate. Thereafter, a small net decline was associated with advancing age.

Given the same values for the other surveyed characteristics, men tended to have salaries 22 percent higher than those of the women economists.

Net salary differences between economists in the various specialty groups were much smaller than the gross differences, thus showing that most of the latter were attributable to correlated differences in the other characteristics. Only in two specialty groups—land economics and economic history—were there significant negative net salary differences. Between the low and high extremes—land economics and industrial organization—there was a net difference of about 11 percent.

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employed economists with many years of experience, the actual net salary difference associated with a primary work activity of management was apparently about 10 to 15 percentage points greater than that shown by the regression results reported.

TECHNICAL NOTE

The following terms as abbreviated in the table titles throughout this report, should be read with the meanings here indicated, and are related to items of the 1964 questionnaire for the National Register of Scientific and Technical Personnel, as follows:

DEGREE—highest earned academic degree (Ph.D., Masters, Bachelors, or less than Bachelors), from responses to:

7. College, University, or other institution ________________;

   Earned degree, if any ______________; ....

ECONOMISTS—("Economists reporting salary")—those among the REGISTERED ECONOMISTS (see below) who reported SALARY (see below) on full-time professional employment, in response to:

8. Please check the box which most fully describes your current employment status. Check only one.

   _____1—Full-time professionally employed (9 items listed)

EXPERIENCE—("Years of experience" or "Work experience")—Number of years of work experience (in any profession), stated in response to:

17. How many years of professional work experience have you

    had? _____

FIELD—("Field of specialization" or "Field of greatest competence")—The one of twelve listed groups of economics specialties (e.g. "Land Economics") which comprehended the first selection of each respondent, in answer to:

18. From the accompanying *specialties list*, select and enter on the lines below in decreasing order the four specialties in which you consider you have your greatest competence, based on your total educational and work experience.

    Greatest: ______________________________ (4 lines provided)
    Number Specialty Title

INCOME—gross professional income (presented only in special tabulation, Section III-11, above), stated in response to:

16. ESTIMATED GROSS ANNUAL PROFESSIONAL INCOME (Jan. 1 to Dec. 31, 1964): Please give your estimated gross professional income from all professional activities for the year which will end December 31, 1964. $...... .... (Gross Annual Professional Income is ALL payment received for professional activities including salary before deductions, plus bonuses, royalties, fees, honoraria, rental and subsistence allowances etc.)

REGISTERED ECONOMISTS—all respondents to the 1964 questionnaire who named any item listed under *Economics* (e.g. "5002 Economic fluctuations") in the accompanying *Specialties List*, to identify the particular specialty of greatest professional competence (See item 18. under FIELD, above).

NOTE: This classification was *not* based on the response to:

6. I regard myself professionally as (an): (check only one)

____XO-Astronomer ____50-Economist (15 choices listed)

SALARY—basic annual salary, stated in response to:

15. BASIC ANNUAL SALARY (JAN. 1964): Please give the basic annual salary associated with your principal professional employment as of January 1964. $______ If academically employed, check whether salary is for ____ 9-10 mos. or ____ 11-12 mos. (Basic annual Salary is your annual salary before deductions for income tax, social security, retirement, etc., but does not include bonuses, overtime, summer teaching, or other payment for professional work.) Do not include rental or subsistence allowances .

TYPE OF EMPLOYER—one of seven broad classes of employer, distinguished in response to:

10. Check the box of the category which is most appropriate for your principal present employer. Check only one.

____1—PRIVATE INDUSTRY OR BUSINESS (14 choices on the questionnaire)

LIST OF APPENDIX TABLES

TABLE A—NUMBER OF NATIONAL REGISTER ECONOMISTS[1], BY YEARS OF EXPERIENCE, FIELD OF SPECIALIZATION, AND DEGREE

Field Degree	Years of Experience							
	Total	Less than 2	2–4	5–9	10–14	15–19	20 and over	No report
Total reporting field	12,143[2]	372	1,446	1,993	2,078	1,729	3,720	805
Ph.D.	5,091	87	473	844	917	842	1,686	242
Master's	4,204	232	722	766	697	449	954	384
Bachelor's	2,613	52	243	369	444	400	943	162
Other[3]	235	1	8	14	20	38	137	17
General economic theory	1,241	55	190	229	212	180	296	79
Ph.D.	656	13	68	125	128	106	183	33
Master's	481	39	102	89	67	56	86	42
Bachelor's	93	2	20	13	17	13	25	3
Other	11	1	—	2	—	5	2	1
Economic history, etc.	265	6	39	38	43	32	85	22
Ph.D.	196	1	19	24	35	29	77	11
Master's	57	4	17	12	6	3	6	9
Bachelor's	9	1	3	2	1	—	—	2
Other	3	—	—	—	1	—	2	—
Economic systems, development and planning	864	34	122	163	157	118	217	53
Ph.D.	432	12	40	90	90	72	112	16
Master's	314	18	73	56	50	28	63	26
Bachelor's	102	4	8	15	16	14	35	10
Other	16	—	1	2	1	4	7	1
Economic statistics	483	31	98	91	69	50	107	37
Ph.D.	227	11	32	58	35	28	52	11
Master's	180	19	54	22	21	11	31	22
Bachelor's	70	1	10	11	12	10	23	3
Other	6	—	2	—	1	1	1	1
Monetary—fiscal	978	34	163	157	145	127	275	77
Ph.D.	606	4	63	97	92	103	208	39
Master's	287	27	86	49	39	15	42	29
Bachelor's	70	3	14	11	12	6	19	5
Other	15	—	—	—	2	3	6	4

[1] "Economists" here defined as respondents who reported their greatest professional competence, based on a combination of education and experience, to be in the general area of economics, including: students, respondents employed non-professionally, part-time professionally employed, and those not reporting salaries.

[2] Includes 83 on active military duty, not represented by data on salaries.

[3] Includes: (a) Less than a bachelor's degree (90 in all fields), (b) Not reporting degree (143 in all fields), and (c) Professional medical degree (2 in all fields).

TABLE A—(*Continued*)

Field Degree	Total	Less than 2	2–4	5–9	10–14	15–19	20 and over	No report
International economics	532	22	55	96	88	57	171	43
Ph.D.	291	7	26	55	56	26	107	14
Master's	182	14	25	34	23	23	41	22
Bachelor's	50	1	4	6	9	8	15	7
Other	9	—	—	1		—	8	—
Business finance & admin., marketing, accounting	4,742	97	415	766	855	751	1,585	273
Ph.D.	1,123	11	81	170	193	212	414	42
Master's	1,653	57	192	327	329	213	414	121
Bachelor's	1,830	29	139	262	324	303	669	104
Less than bachelor's	64	—	—	1	5	12	45	1
Other	72	—	3	6	4	11	43	5
Industrial org., govt. & business, industry studies	804	20	89	108	143	106	290	48
Ph.D.	377	8	41	39	77	59	139	14
Master's	252	10	34	50	43	26	70	19
Bachelor's	159	2	14	19	19	20	72	13
Other	16	—	—	—	4	1	9	2
Land economics	1,254	50	183	215	215	176	328	87
Ph.D.	657	13	69	121	127	117	179	31
Master's	483	31	99	82	67	47	109	48
Bachelor's	103	6	14	12	20	11	33	7
Other	11	—	1	—	1	1	7	1
Labor economics	662	16	69	84	101	96	241	55
Ph.D.	373	3	26	45	58	71	151	19
Master's	209	10	28	28	35	20	56	32
Bachelor's	73	3	14	10	8	5	29	4
Other	7	—	1	1	—	—	5	—
Welfare, population, etc.	164	3	14	23	25	20	69	10
Ph.D.	83	3	7	7	15	13	31	7
Master's	48	—	5	10	8	1	22	2
Bachelor's	31	—	2	5	2	6	15	1
Other	2	—	—	1	—	—	1	—
Economics, other	154	4	9	23	25	16	56	21
Ph.D.	70	1	1	13	11	6	33	5
Master's	58	3	7	7	9	6	14	12
Bachelor's	23	—	1	3	4	4	8	3
Other	3	—	—	—	1	8	1	1

TABLE B—NUMBER OF NATIONAL REGISTER ECONOMISTS[1], BY YEARS OF EXPERIENCE, DEGREE, AND TYPE OF EMPLOYER

Highest degree Type of employer	Years of experience							
	Total	Under 2	2–4	5–9	10–14	15–19	20 and over	No report
Total registered	12,143[2]	372	1,446	1,993	2,078[3]	1,729	3,720	805
Ph.D. degree: Total	5,091	87	473	844	917	842	1,686	242
Educational institutions	3,440	56	355	646	627	610	1,014	132
Federal government	450	12	42	75	72	58	174	17
Other government	82	3	6	10	17	12	32	2
Non-profit organizations	216	7	19	35	40	46	61	8
Industry-business	548	2	23	63	124	83	214	39
Self-employed	41	1	—	1	3	5	30	1
Military	14	2	6	1	—	3	2	—
Other employers	95	2	11	5	21	18	31	7
Not employed	176	2	7	4	8	7	116	32
Not reported	29	—	4	4	5	—	12	4
Master's degree: Total	4,204	232	722	766	697	449	954	384
Educational institutions	1,433	113	385	296	178	125	227	109
Federal government	563	24	58	79	102	66	199	35
Other government	136	4	24	23	33	13	32	7
Non-profit organizations	191	8	26	33	36	21	52	15
Industry-business	1,409	39	151	280	309	189	338	103
Self-employed	69	1	2	11	7	11	25	12
Military	43	6	8	6	7	4	3	9
Other employers	81	7	12	14	14	11	20	3
Not employed	239	27	42	19	10	8	51	82
Not reported	40	3	14	5	1	1	7	9

[1] "Economists" here defined as respondents who reported their greatest professional competence, based on a combination of education and experience, to be in the general area of economics, including: students, respondents employed non-professionally, part-time professionally employed, and those not reporting salaries (Questionnaire No. 18).

[2] Includes: 83 on active military duty, not represented by data on salaries, and 2 persons with professional medical degrees (each with 10–14 years experience) who were employed by educational institutions.

[3] Including 2 persons with professional medical degrees, employed by educational instutitions.

TABLE B—(*Continued*)

Highest degree Type of employer	Years of experience							
	Total	Under 2	2–4	5–9	10–14	15–19	20 and over	No report
Bachelor's degree: Total	2,613	52	243	369	444	400	943	162
Educational institutions	151	10	56	22	18	8	27	10
Federal government	249	4	27	34	31	39	106	8
Other government	57	1	8	8	8	7	23	2
Non-profit organizations	51	1	1	14	6	9	18	2
Industry-business	1,885	20	134	275	362	318	664	112
Self-employed	70	—	4	6	11	7	37	5
Military	25	7	5	2	2	2	2	5
Other employers	26	1	4	2	2	4	13	—
Not employed	82	8	3	4	1	4	46	16
Not reported	17	—	1	2	3	2	7	2
Less than Bachelor's degree	90	—	—	1	7	17	61[4]	4
Degree not reported: Total	143	1	8	13	11	21	76[5]	13

[4] Including 43 persons employed by industry and business.

[5] Including 33 persons employed by industry and business.

TABLE C—NUMBER OF ECONOMISTS[1] IN SALARY ANALYSIS, BY SEX
(Exclusions from total number of economists registered by the National Science Foundation in 1964, to arrive at number of economists reporting salaries on civilian full-time professional employment)

Classification	Total		Men		Women	
	Not in salary analysis	Net	Not in salary analysis	Net	Not in salary analysis	Net
Total registered		12,143		11,650		493
Total excluded from salary analysis	2,162		1,991		171	
Total, economists reporting salaries on civilian, full-time professional employment		9,981		9,659		322
Total registered		12,143		11,650		493
No report of employment status	50		45		5	
Employment status known		12,093		11,605		488
Students	539		505		34	
Available for full-time employment		11,554		11,100		454
Not employed	335		286		49	
Employed		11,219		10,814		405
Not employed in professional work	450		447		3	
Employed professionally		10,769		10,367		402
Part-time professionally employed	223		170		53	
Full-time professionally employed		10,546		10,197		349
on active military duty	44		44		—	
Civilian full-time professionally employed		10,502		10,153		349
Not reporting salaries	521		494		27	
Total, economists reporting salaries on civilian, full-time professional employment		9,981		9,659		322

[1] "Economists" here defined as respondents who reported their greatest professional competence, based on a combination of education and experience, to be in a specialty classified as within the general area of economics (Questionnaire item No. 18).

TABLE D—WOMEN ECONOMISTS: NUMBER AND SALARIES, BY YEARS OF EXPERIENCE
(Total numbers registered and numbers reporting salaries; percent of total reporting salaries; median, decile and quartile salaries)

Sex and years of experience	Number: Total registered	Number: Reporting salary and experience	Women as percent of total reporting salary and experience	Thousands of dollars of reported salary: Median	Lowest decile	Lower quartile	Upper quartile	Highest decile
Men and women								
Total	12,143	9,588	—	12.0	7.8	9.3	16.0	20.0
Women								
Total	446	303	3.2	9.9	6.8	8.0	12.8	15.0
Under 2 years	17	10	4.0	—[1]	—[1]	—[1]	—[1]	—[1]
2–4 years	67	35	3.2	7.2	5.7	6.2	8.5	9.0
5–9 years	76	44	2.5	8.0	6.0	7.0	10.0	11.3
10–14 years	55	40	2.1	9.0	6.5	7.5	10.2	12.0
15–19 years	50	40	2.6	9.3	7.8	8.7	11.8	14.0
20 years and over	181	134	4.5	12.2	8.5	10.1	15.0	16.7

[1] Not reported because less than 25 represented.

TABLE E—NUMBER OF ECONOMISTS, BY AGE GROUP, FIELD OF SPECIALIZATION, AND DEGREE
(Economists reporting salaries for full-time professional work)

Field Degree	Age group (years)												
	Total	20–24	25–29	30–34	35–39	40–44	45–49	50–54	55–59	60–64	65–69	70+	No rpt.
Total reporting field[1]	9,981[3]	45	758	1,657	1,712	1,906	1,482	1,035	696	423	223	29	15
Ph.D.	4,544	5	170	653	754	972	738	505	331	240	146	23	7
Master's	3,171	21	361	669	577	516	364	298	210	95	51	3	6
Bachelor's	2,106	18	223	326	366	395	357	200	135	64	17	3	2
Other[2]	158	1	4	9	14	22	23	32	20	24	9	—	—
General economic theory[1]	1,024	4	75	190	185	188	135	117	58	45	20	3	4
Ph.D.	591	1	26	99	116	127	88	70	25	23	12	3	1
Master's	345	2	40	73	57	53	35	34	27	14	7	—	3
Bachelor's	80	1	9	17	11	7	11	12	5	7	—	—	—
Other[2]	8	—	—	1	1	1	1	1	1	1	1	—	—
Economic history, etc.[1]	205	—	17	20	32	46	18	31	15	12	13	1	—
Ph.D.	163	—	6	8	26	41	17	28	13	12	11	1	—
Master's	34	—	8	9	6	4	1	3	1	—	2	—	—
Bachelor's	6	—	3	3	—	—	—	—	—	—	—	—	—
Other[2]	2	—	—	—	—	1	—	—	1	—	—	—	—
Economic systems, development, and planning[1]	734	—	59	127	125	140	106	74	52	25	23	2	1
Ph.D.	396	—	16	59	66	97	55	43	23	16	18	2	1
Master's	242	—	31	56	43	32	35	19	17	4	5	—	—
Bachelor's	88	—	11	12	16	9	14	11	11	4	—	—	—
Other[2]	8	—	1	—	—	2	2	1	1	1	—	—	—

[1] Includes respondents not reporting degree.

[2] Includes: (a) Less than a bachelor's degree (60 in all fields), and (b) Not reporting degree (98 in all fields).

[illegible] medical degree, in age groups 35–39 and 40–44, respectively.

TABLE E —(*Continued*)

Field Degree	Total	20–24	25–29	30–34	35–39	40–44	45–49	50–54	55–59	60–64	65–69	70+	No rpt.
	Age group (years)												
Economic Statistics[1]	389	4	38	88	70	73	44	39	19	7	5	1	1
Ph.D.	203	2	13	47	43	46	25	14	5	4	3	1	—
Master's	122	2	16	30	21	15	7	18	8	2	2	—	1
Bachelor's	59	—	8	10	6	12	10	7	5	1	—	—	—
Other[3]	5	—	1	1	—	—	2	—	1	—	—	—	—
Monetary-fiscal[1]	800	4	69	128	123	149	114	64	68	43	30	6	2
Ph.D.	534	—	28	66	76	111	89	46	55	36	21	5	1
Master's	199	4	35	44	39	28	19	11	6	6	5	1	1
Bachelor's	59	—	6	18	8	8	6	6	5	—	2	—	—
Other[3]	8	—	—	—	—	2	—	1	2	1	2	—	—
International Economics[1]	450	2	15	75	77	81	76	52	40	18	12	2	—
Ph.D.	264	—	5	40	46	49	38	36	25	14	9	2	—
Master's	136	2	9	28	26	24	26	8	11	1	1	—	—
Bachelor's	42	—	1	7	4	8	11	7	3	1	—	—	—
Other[3]	8	—	—	—	1	—	1	1	1	2	2	—	—
Business, finance & admin.; Marketing, Accounting[1]	3,860	29	334	629	706	762	605	353	238	140	56	6	2
Ph.D.	997	2	30	132	159	216	183	107	91	45	27	3	2
Master's	1,314	11	141	283	263	238	150	105	70	37	15	1	—
Bachelor's	1,451	15	161	208	274	294	256	117	69	44	11	2	—
Other[3]	98	1	2	6	10	14	16	24	8	14	3	—	—
Industrial org., govt. & business, industry studies[1]	662[4]	1	45	107	107	117	98	74	56	35	20	2	—

[4] Includes 1 with a professional medical degree in age group, 40–44.

TABLE E—(*Continued*)

Field / Degree	Total	20–24	25–29	30–34	35–39	40–44	45–49	50–54	55–59	60–64	65–59	70+	No rpt.
		Age group (years)											
Ph.D.	335	—	10	57	53	64	53	28	26	27	15	2	—
Master's	190	—	25	33	31	26	24	26	19	4	2	—	—
Bachelor's	129	1	10	17	21	25	20	18	11	3	3	—	—
Other[3]	7	—	—	—	2	1	1	2	—	1	—	—	—
Land economics[1]	1,066[5]	1	73	191	170	219	148	121	73	39	26	2	3
Ph.D.	610	—	26	100	93	149	101	75	29	22	14	—	1
Master's	369	—	41	77	64	58	34	38	29	16	10	1	1
Bachelor's	80	1	6	14	12	12	13	7	12	—	1	1	1
Other[3]	6	—	—	—	—	—	—	1	3	1	1	—	—
Labor economics[1]	546	—	25	66	69	95	106	79	50	42	12	1	1
Ph.D.	326	—	8	28	47	57	72	44	29	30	10	1	—
Master's	150	—	12	23	14	27	27	24	14	7	2	—	—
Bachelor's	65	—	5	14	8	11	7	11	5	3	—	—	1
Other[3]	5	—	—	1	—	—	—	—	2	2	—	—	—
Population, Welfare, etc.[1]	139	—	4	18	24	20	20	19	18	11	3	2	—
Ph.D.	72	—	1	9	14	11	11	6	6	9	3	2	—
Master's	36	—	1	6	7	3	4	9	4	2	—	—	—
Bachelor's	29	—	2	3	3	5	5	3	8	—	—	—	—
Other[3]	2	—	—	—	—	1	—	1	—	—	—	—	—
Economics, other[1]	106	—	4	18	24	16	12	12	9	6	3	1	1
Ph.D.	53	—	1	8	15	4	6	8	4	2	3	1	1
Master's	34	—	2	7	6	8	2	3	4	2	—	—	—
Bachelor's	18	—	1	3	3	4	4	1	1	1	—	—	—
Other[3]	1	—	—	—	—	—	—	—	—	1	—	—	—

TABLE F—DISTRIBUTION OF SALARIES OF "ECONOMISTS", AS DEFINED—THREE METHODS
(With salary distributions of economists added by Methods II and III)

Salary Interval[1]	I AEA[2] members	Non-member[3] self identified economist	II AEA[4] registrant	Net[5] additional economic specialties	III Total registered economists[6]
	(1)	(2)	(3)	(4)	(5)
Total reporting salary	5,424	2,459	7,883	2,098	9,981
Under $4,500	9	6	15	7	22
5,000	25	9	34	—	34
6,000	144	46	190	16	206
7,000	347	108	455	29	484
8,000	546	197	743	104	847
9,000	588	251	839	148	987
10,000	580	315	895	157	1,052
11,000	399	252	651	127	778
12,000	424	229	653	165	818
13,000	352	188	540	163	703
14,000	345	166	511	144	655
15,000	306	174	480	158	638
16,000	231	114	345	99	444
17,000	253	95	348	84	432
18,000	206	85	291	103	394
19,000	141	30	171	49	220
20,000	151	65	216	96	312
21,000	58	17	75	39	114
22,000	43	27	70	53	123
23,000	32	7	39	30	69
24,000	27	14	41	33	74
25,000	84	17	101	75	176
25,500 and over	133	47	180	219	399

[1] Each indicated single amount ±$500—e.g. "$5,000 denotes more than $4,500 and less. than $5,500

[2] Members and non-members attending meetings of the American Economic Association.

[3] Numbers in column 3 less those in column 1.

[4] Respondents to National Register questionnaire, who regarded themsleves professionally as economists (Questionnaire item No. 6).

[5] Numbers in column 5 less those in column 3.

[6] Respondents to National Register questionnaire who reported their greatest professional competence, based on a combination if education and experience, to be in the general area area of economics (Questionnaire item No. 18).

TABLE G—SALARIES OF MEMBERS OF THE AMERICAN ECONOMIC ASSOCIATION, BY TYPE OF EMPLOYER

(Percent distribution by salary classes)

Salary interval[1]	Total	Education	Federal govt.	Other govt.	Non-profit org.	Industry, bus.	Other[2]	No report
	Number	Number	Number	Number	Number	Number	Number	Number
Total registered	6,550[3]	3,829	729	158	319	909	247	44
Total reporting salary	5,424	3,378	705	147	289	724	168	13
	Percent	Percent	Percent	Percent	Percent	Percent	Percent	Percent
Total reporting salary	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0
Under 4,500	.2[4]	.2	.1	—	.3	.1	—	—
5,000	.5	.6	—	1.4	—	.3	—	—
6,000	2.7	3.8	.6	2.7	.3	1.2	1.2	—
7,000	6.4	8.6	2.4	5.4	2.7	2.6	2.4	—
8,000	10.1	13.7	4.1	7.5	1.7	4.6	3.6	—
9,000	10.8	14.6	3.7	8.8	3.5	5.1	4.2	7.7
10,000	10.7	12.0	10.6	11.5	5.9	7.0	5.9	23.0
11,000	7.3	8.4	2.6	9.5	7.3	6.9	5.9	7.7
12,000	7.8	7.7	8.9	8.2	7.3	6.9	9.5	—
13,000	6.5	6.5	6.3	10.9	6.6	5.7	7.1	15.4
14,000	6.4	5.2	9.8	4.1	10.7	7.7	4.2	7.7
15,000	5.6	4.4	9.1	6.8	6.6	6.5	9.5	7.7
16,000	4.3	3.6	7.8	6.1	3.5	3.6	4.7	—
17,000	4.7	2.9	11.2	3.4	8.6	5.2	4.2	7.7
18,000	3.8	2.2	9.8	6.1	4.2	4.3	4.7	7.7
19,000	2.6	1.7	6.4	1.4	4.5	2.8	3.0	—
20,000	2.8	1.3	5.1	1.4	8.3	5.4	3.6	—
21,000	1.1	.7	.4	.7	3.5	2.3	2.4	—
22,000	.8	.4	.4	.7	2.1	2.1	2.4	—
23,000	.6	.4	—	—	1.7	1.8	1.2	—
24,000	.5	.1	.1	.7	1.0	1.7	2.4	7.7
25,000	1.5	.5	.3	—	4.2	5.8	5.4	7.7
25,500 and over	2.3[5]	.5[6]	.3	2.7[7]	5.5[8]	10.4[9]	12.5[10]	—

[1] Each indicated single amount ±$500—e.g. "$5,000" denotes more than $4,500 and les than $5,500.

[2] Including 35 self-employed persons.

[3] Excluding 315 reporting members of AEA not employed.

[4] 9 persons; median $2,300; range, $600–$4,500.

[5] 133 persons; median $32,000; range, $25,500–$100,000.

[6] 15 persons; median $28,000; range, $25,500–$100,000.

[7] 4 persons reporting salaries of $26,500, $35,000, $40,000 (2)

[8] 16 persons; median $33,000; range, $26,000–$60,000.

[9] 75 persons; median $33,000; range, $25,500–$100,000.

[10] 21 persons; median $30,000; range, $26,000–$50,000.

TABLE H—SALARIES OF ECONOMISTS IN SELECTED WORK ACTIVITIES
(By degree and years of experience. Research and development management or administration, teaching)

Years of experience / Degree	Number of economists					Median salary ($000's)			
	Total[1] work activities	Selected[2] work activities	Research devel-opment	Manage-ment or admin.	Teach-ing[3]	Total[1] work ac-tivities	Re-search & de-velop-ment	Man-age-ment or admin.	Teach-ing[3]
Total registered	9,981	7,825	1,680	2,998	3,147	12.0	11.4	16.1	9.7
Reporting experience	9,588	7,528	1,606	2,875	3,047	12.0	11.4	16.0	9.7
Reporting degree and experience	9,492[5]	7,461[5]	1,596	2,837	3,028	12.0	11.5	16.0	9.7
Less than 2 years	249	193	90	19	84	7.8	8.4	—[6]	7.1
2–4 years	1,098	840	292	91	457	8.4	9.1	9.0	7.9
5–9 years	1,780	1,357	387	328	642	10.0	10.5	12.0	8.7
10–14 years	1,882	1,454	311	597	546	12.0	12.3	14.1	10.0
15–19 years	1,538	1,238	190	537	511	13.5	13.3	16.2	11.0
20 or more years	2,945	2,379	326	1,265	788	16.0	15.0	18.2	12.5
Ph.D. degree[4]	4,387	4,029	903	935	2,191	12.1	12.4	17.1	10.5
Less than 2 years	81	78	34	7	37	8.5	9.0	—[6]	7.8
2–4 years	432	401	142	15	244	9.0	9.8	—[6]	8.4
5–9 years	812	754	232	79	443	10.0	11.0	14.1	9.2
10–14 years	867	799	191	186	422	12.0	13.4	15.9	10.5
15–19 years	797	749	126	204	419	12.8	13.7	17.2	11.2
20 or more years	1,398	1,248	178	444	626	15.4	16.0	18.5	13.0
Master's degree[4]	3,019	2,204	539	921	744	11.0	10.0	15.0	7.7
Less than 2 years	138	101	51	6	44	7.3	7.9	—[6]	6.5
2–4 years	473	354	132	51	171	8.0	8.4	9.4	7.0
5–9 years	638	450	115	147	188	10.0	9.7	11.7	7.5
10–14 Years	620	425	93	221	111	12.0	11.0	13.6	8.1
15–19 years	386	278	45	146	87	13.8	12.5	15.5	8.8
20 or more years	764	596	103	350	143	15.0	13.5	17.5	9.4
Bachelor's degree[4]	2,025	1,182	150	939	93	13.3	10.7	16.0	7.9
Less than 2 years	30	14	5	6	3	7.2	—[6]	—[6]	—[6]
2–4 years	193	85	18	25	42	7.8	—[6]	8.0	7.3
5–9 years	329	152	40	101	11	10.0	9.6	11.6	—[6]
10–14 Years	387	224	26	185	13	13.6	12.1	13.7	—[6]
15–19	344	204	18	181	5	14.8	—[6]	16.0	—[6]
20 or more years	742	503	43	441	19	17.4	13.0	19.3	—[6]
Less than bachelor's[4]	59	44	3	41	—	17.0	—[6]	17.2	—
20 or more years	41	32	2	30	—	19.8	—[6]	20.9	—
All other exp.	18	12	1	11	—	—[6]	—[6]	—[6]	—

[1] Numbers and medians include respondents reporting work activities not shown separately in this table. Numbers also include a total of 237 persons who did not identify their work activity.
[2] Total of the 3 selected work activities as shown in subsequent columns.
[3] Salaries include those for 9–10 months, as well as for 11–12 months.
[4] Reporting years of professional experience as well as academic degree.
[5] Including 2 persons with professional medical degrees, 1 in applied research and 1 in administration of research and development.
[6] Not shown because less than 25 persons represented.

TABLE J—RESEARCH AND DEVELOPMENT: NUMBERS AND MEDIAN SALARIES OF ECONOMISTS IN R & D ACTIVITY, BY SELECTED YEARS OF EXPERIENCE, FIELD OF SPECIALIZATION, AND DEGREE[1]

(R & D as primary work activity; Experience: Total, 5–9 years, 20 or more years)

Field Degree	Number			Median salary ($000's)		
	Years of experience			Years of experience		
	Total[2]	5–9 yrs.	20 or more	Total[2]	5–9 yrs.	20 or more
General economic theory						
Total[3]	205	50	44	12.0	10.4	17.3
Ph.D. degree	122	34	24	13.0	11.0	—[4]
Master's degree	61	11	15	10.5	—[4]	—[4]
Economic history, etc.						
Total[3]	21	4	8	—[4]	—[4]	—[4]
Econ. systems, devel. & planning						
Total[3]	174	41	28	12.0	10.3	16.6
Ph.D. degree	93	23	14	13.5	—[4]	—[4]
Master's degree	63	15	11	10.4	—[4]	—[4]
Economic statistics						
Total[3]	127	28	19	11.8	10.8	—[4]
Ph.D. degree	77	20	10	13.5	—[4]	—[4]
Master's degree	37	4	7	10.0	—[4]	—[4]
Monetary, fiscal						
Total[3]	141	29	27	12.0	11.5	17.7
Ph.D. degree	100	21	24	13.0	—[4]	—[4]
Master's degree	36	6	2	9.5	—[4]	—[4]
International economics						
Total[3]	66	12	14	13.4	—[4]	—[4]
Ph.D. degree	38	7	9	15.0	—[4]	—[4]

[1] Separate data are shown by academic degree where a total of 25 or more respondents with any separate field of specialization reported that individual degree.

[2] Includes respondents not reporting years of experience.

[3] Includes respondents not reporting highest academic degree and those with academic degrees not shown separately in this table.

[4] Not shown because less than 25 persons represented.

TABLE J—(*Continued*)

Field Degree	Number			Median salary ($000's)		
	Years of experience			Years of experience		
	Total[2]	5–9 yrs.	20 or more	Total[3]	5–9 yrs.	20 or more
Bus. finance & admin.; mrktg.; acctg.						
Total[3]	237	61	44	10.8	10.5	13.8
Ph.D. degree	100	24	17	11.3	—[4]	—[4]
Master's degree	94	24	16	10.3	—[4]	—[4]
Bachelor's degree	38	12	9	12.0	—[4]	—[4]
Indus. org., govt. & bus., industry studies						
Total[3]	130	27	36	12.0	10.6	15.5
Ph.D. degree	61	8	16	13.5	—[4]	—[4]
Master's degree	51	12	12	10.8	—[4]	—[4]
Land economics						
Total[3]	442	111	69	10.5	10.3	13.4
Ph.D. degree	269	76	39	11.3	10.6	14.6
Master's degree	161	33	27	9.0	9.0	13.0
Labor economics						
Total[3]	86	17	27	12.0	—[4]	14.3
Ph.D. degree	43	11	12	13.7	—[4]	—[4]
Master's degree	27	4	7	10.0	—[4]	—[4]
Welfare, etc.						
Total[3]	34	6	10	12.0	—[4]	—[4]
Economics other						
Total[3]	17	3	4	—[4]	—[4]	—[4]

Table K—Management or Administration: Numbers and Median Salaries of Economists in Administrative Activity, by Selected Years of Experience, Field of Specialization, and Degree[1]
(Management or administration as primary work activity;
Experience: Total, 5–9 years, 20 or more years)

Field Degree	Number			Median salary ($000's)		
	Years of experience			Years of experience		
	Total[2]	5–9 yrs.	20 or more	Total[2]	5–9 yrs.	20 or more
General economic theory						
Total[3]	154	14	59	16.5	—[4]	20.0
Ph.D. degree	76	7	26	17.0	—[4]	17.5
Master's degree	59	6	23	14.4	—[4]	—[4]
Economic history, etc.						
Total[3]	16	1	6	—[4]	—[4]	—[4]
Econ. systems, devel. & planning						
Total[3]	203	30	79	16.5	11.8	18.4
Ph.D. degree	100	12	40	17.1	—[4]	19.0
Master's degree	63	10	23	15.4	—[4]	—[4]
Bachelor's degree	35	8	15	14.5	—[4]	—[4]
Economic statistics						
Total[3]	75	10	32	14.9	—[4]	17.0
Ph.D. degree	20	2	11	—[4]	—[4]	—[4]
Master's degree	33	5	11	13.5	—[4]	—[4]
Monetary fiscal						
Total[3]	168	17	69	16.8	—[4]	18.0
Ph.D. degree	101	8	48	18.0	—[4]	18.0
Master's degree	40	6	12	13.6	—[4]	—[4]
International economics						
Total[3]	108	9	58	17.0	—[4]	18.5
Ph.D. degree	58	5	35	18.0	—[4]	18.5
Master's degree	32	4	13	16.5	—[4]	—[4]

[1] Separate data are shown by academic degree where a total of 25 or more respondents with any separate field of specialization reported that individual degree.
[2] Includes respondents not reporting years of professional experience.
[3] Includes respondents not reporting highest academic degree and those with academic degrees not shown separately in this table.
[4] Not shown because less than 25 persons represented.

TABLE K—(*Continued*)

Field Degree	Number			Median salary ($000's)		
	Years of experience			Years of experience		
	Total[3]	5–9 yrs.	20 or more	Total[3]	5–9 yrs.	20 or more
Bus. finance & admin.; mrktg.; acctg.						
Total[3]	1,621	194	684	16.5	12.0	20.0
Ph.D. degree	319	29	152	18.0	14.1	20.0
Master's degree	521	92	167	15.5	12.0	20.0
Bachelor's degree	726	72	324	16.2	12.0	20.0
Less than bachelor's degree	28	1	22	18.0	—[4]	—[4]
Indus. org., govt. & bus., industry studies						
Total[3]	190	16	92	17.0	—[4]	18.0
Ph.D. degree	76	5	36	18.0	—[4]	18.5
Master's degree	50	8	23	15.0	—[4]	—[4]
Bachelor's degree	58	3	31	17.0	—[4]	18.0
Land economics						
Total[3]	248	20	109	14.5	—[4]	16.5
Ph.D. degree	130	7	55	16.0	—[4]	16.9
Master's degree	82	9	38	12.9	—[4]	15.0
Bachelor's degree	33	4	13	10.3	—[4]	—[4]
Labor economics						
Total[3]	141	10	65	15.0	—[4]	16.4
Ph.D. degree	55	1	24	16.4	—[4]	—[4]
Master's degree	56	4	26	14.1	—[4]	16.0
Bachelor's degree	29	5	14	14.0	—[4]	—[4]
Welfare, etc.						
Total[3]	41	2	27	14.1	—[4]	15.0
Economics, other						
Total[3]	33	5	10	15.2	—[4]	—[4]

TABLE L—TEACHING: NUMBERS AND MEDIAN SALARIES[1] OF ECONOMISTS IN TEACHING ACTIVITY, BY SELECTED YEARS OF EXPERIENCE, FIELD OF SPECIALIZATION, AND DEGREE
(Teaching as primary activity; experience: total, 5–9 years, 20 or more years)

Field Degree	Number			Median salary ($000's)		
	Years of experience			Years of experience		
	Total[2]	5–9 yrs.	20 or more	Total[2]	5–9 yrs.	20 or more
General economic theory						
Total[3]	547	120	121	9.0	8.4	11.5
Ph.D. degree	355	74	91	10.0	9.3	12.5
Economic history, etc.						
Total[3]	157	23	52	9.5	—[4]	10.5
Ph.D. degree	126	16	49	9.8	—[4]	10.5
Economic systems, devel. & planning						
Total[3]	219	48	40	9.2	9.0	12.7
Ph.D. degree	164	39	31	10.0	9.1	14.0
Economic statistics						
Total[3]	134	37	19	9.2	8.9	—[4]
Ph.D. degree	95	31	17	10.0	9.8	—[4]
Monetary, fiscal						
Total[3]	411	84	100	9.5	8.5	13.0
Ph.D. degree	299	60	85	10.2	9.0	13.5

[1] Includes salaries for 9–10 months, as well as for 11–12 months.
[2] Includes respondents not reporting years of professional experience.
[3] Includes respondents not reporting highest academic degree and those with academic degrees not shown separately in this table.
[4] Not shown because less than 25 persons represented.

TABLE L—(*Continued*)

Field Degree	Number			Median salary ($000's)		
	Years of experience			Years of experience		
	Total[3]	5–9 yrs.	20 or more	Total[2]	5–9 yrs.	20 or more
International economics						
Total[3]	183	51	50	9.3	8.5	13.6
Ph.D. degree	139	37	40	9.7	8.6	13.9
Business finance & admin.; marketing; accounting						
Total[3]	740	157	195	10.0	9.0	12.8
Ph.D. degree	487	99	133	11.0	9.6	14.0
Indus. org., govt. & bus.; industry studies						
Total[3]	212	32	62	10.5	8.4	13.0
Ph.D degree	173	23	51	11.0	—[4]	13.6
Land economics						
Total[3]	222	40	50	10.5	10.0	12.0
Ph.D. degree	153	28	33	10.7	10.3	13.0
Labor economics						
Total[3]	255	41	82	10.0	8.6	12.7
Ph.D. degree	207	28	75	10.9	9.0	12.9
Welfare, etc.						
Total[3]	40	7	11	10.0	—[4]	—[4]
Ph.D. degree	35	5	10	10.5	—[4]	—[4]
Economics, other						
Total[3]	27	5	16	10.5	—[4]	—[4]
Ph.D. degree	19	3	11	—[4]	—[4]	—[4]

TABLE M—EDUCATIONAL INSTITUTIONS: NUMBERS AND SALARIES[1] OF ECONOMISTS, BY SELECTED DEGREE, AND YEARS OF EXPERIENCE
(Median, decile and quartile salaries; Ph.D., Masters; six work experience groups)

Type of degree Years of experience	Number of economists		Thousands of dollars of reported salary				
	Total registered	Salary reported[2]	Median	Lowest decile	Lower quartile	Upper quartile	Highest decile
Total employed	5,061	4,455	10.1	7.2	8.5	13.0	16.4
Total reporting degrees	5,027[3]	4,430[4]	10.1	7.2	8.4	13.0	16.4
Ph.D. degree	3,440	3,255	11.0	8.1	9.2	14.0	17.0
Less than 2 years	56	55	8.2	7.0	7.5	9.0	9.8
2–4	355	338	8.7	7.3	8.0	9.5	10.5
5–9	646	625	9.7	7.9	8.6	11.0	12.5
10–14	627	601	11.0	8.5	9.5	13.0	15.3
15–19	610	592	12.0	9.0	10.1	14.3	17.0
20 and over	1,014	941	14.0	9.5	11.5	16.8	20.0
No report of experience	132	103	11.0	8.4	9.2	14.5	17.5
Master's degree	1,433	1,048	8.0	6.2	7.0	9.9	12.5
Less than 2 years	113	64	6.7	5.5	6.1	7.6	9.0
2–4	385	229	7.1	6.0	6.4	8.0	8.7
5–9	296	235	7.5	6.1	6.9	8.7	10.0
10–14	178	160	8.5	6.8	7.5	10.2	11.6
15–19	125	112	9.0	7.1	7.7	11.1	14.0
20 and over	227	201	10.3	7.1	8.1	12.9	15.2
No report of experience	109	47	8.4	6.0	6.7	10.5	15.5

[1] Including salaries for 9–10 months as well as 11–12 months.
[2] Excluding students and part-time employees.
[3] Including 152 persons with bachelor's degrees or less and 2 with professional medical degrees.
[4] Including 125 persons with bachelor's degrees or less and 2 with professional medical degrees.

TABLE N—FEDERAL GOVERNMENT: NUMBERS AND SALARIES OF ECONOMISTS, BY SELECTED DEGREE, AND YEARS OF EXPERIENCE
(Median, decile, and quartile salaries; Ph.D., Masters; six work experience groups)

Type of degree Years of experience	Number of economists		Thousands of dollars of reported salary				
	Total registered	Salary reported	Median	Lowest decile	Lower quartile	Upper quartile	Highest decile
Total employed	1,274	1,228	13.7	8.5	10.3	16.5	18.2
Total reporting degrees	1,267[1]	1,221[2]	13.6	8.5	10.3	16.5	18.2
Ph.D. degree	450	442	14.8	10.3	12.0	17.5	19.2
Less than 2 years	12	12	—[3]	—[3]	—[3]	—[3]	—[3]
2–4	42	39	10.3	8.6	10.0	11.7	12.1
5–9	75	75	12.1	9.9	10.4	14.1	17.2
10–14	72	72	14.1	11.0	12.5	16.0	18.4
15–19	58	58	15.0	11.3	12.1	17.2	19.3
20 years and over	174	169	17.2	13.4	15.0	18.5	20.0
No report of experience	17	17	—[3]	—[3]	—[3]	—[3]	—[3]
Master's degree	563	537	12.6	8.4	10.0	15.2	17.5
Less than 2 years	24	23	—[3]	—[3]	—[3]	—[3]	—[3]
2–4	58	55	8.4	7.0	7.4	10.0	10.6
5–9	79	77	10.0	8.4	8.7	11.0	12.1
10–14	102	100	12.1	10.0	10.3	14.0	15.0
15–19	66	61	14.0	10.0	12.5	15.0	17.2
20 years and over	199	193	15.6	12.1	13.7	17.2	18.2
No report of experience	35	28	11.7	7.0	8.6	17.1	19.0

[1] Including 254 persons with bachelor's degrees or less.
[2] Including 242 persons with bachelor's degrees or less.
[3] Not shown because less than 25 persons represented.

TABLE O—INDUSTRY OR BUSINESS: NUMBERS AND SALARIES OF ECONOMISTS, BY SELECTED DEGREE, AND YEARS OF EXPERIENCE
(Median, decile and quartile salaries; Ph.D., Masters, Bachelors; six work experience groups)

Type of degree Years of experience	Number of economists		Thousands of dollars of reported salary				
	Total registered	Salary reported	Median	Lowest decile	Lower quartile	Upper quartile	Highest decile
Total employed	3,967	3,325	14.4	8.8	11.0	19.7	25.0
Total reporting degrees	3,903[1]	3,275[2]	14.4	8.8	11.0	19.6	25.0
Ph.D. degree	548	458	18.9	12.0	15.0	25.0	33.0
Less than 2 years	2	2	—[3]	—[3]	—[3]	—[3]	—[3]
2–4	23	20	—[3]	—[3]	—[3]	—[3]	—[3]
5–9	63	60	14.3	11.8	13.0	18.0	24.0
10–14	124	110	17.2	12.7	15.0	21.0	25.0
15–19	83	72	21.0	15.0	17.0	26.0	38.0
20 years and over	214	169	22.0	13.2	17.6	27.0	45.0
No report of experience	39	25	14.4	10.8	13.4	22.0	37.5
Master's degree	1,409	1,190	13.1	8.7	10.2	18.0	24.0
Less than 2 years	39	34	8.5	7.2	7.8	9.0	10.2
2–4	151	128	9.2	7.6	8.5	10.4	12.5
5–9	280	253	11.0	8.7	10.0	13.0	15.5
10–14	309	283	13.5	10.0	11.5	16.0	20.0
15–19	189	168	15.0	11.2	13.2	19.0	24.0
20 years and over	338	265	19.3	12.5	15.0	25.0	34.0
No report of experience	103	59	12.0	7.8	9.0	20.0	25.0
Bachelor's degree	1,885	1,581	14.0	8.4	10.5	18.1	25.0
Less than 2 years	20	16	—[3]	—[3]	—[3]	—[3]	—[3]
2–4	134	110	8.1	6.9	7.5	9.0	10.0
5–9	275	255	10.0	8.2	9.0	12.0	15.0
10–14	362	320	12.8	10.0	11.0	15.0	18.0
15–19	318	274	15.0	10.8	12.8	18.0	22.0
20 years and over	664	541	18.5	12.0	15.0	25.0	32.0
No report of experience	112	65	14.0	8.0	10.4	21.0	30.0

[1] Including 61 persons with less than a bachelor's degree.
[2] Including 46 persons with less than a bachelor's degree.
[3] Not shown because less than 25 persons represented.

TABLE P—WOMEN ECONOMISTS: YEARS OF EXPERIENCE AND SALARIES OF WOMEN AND OF MEN IN SELECTED[1] FIELDS OF SPECIALIZATION
(Median years of experience and median, decile, and quartile salaries)

Field and sex	Number reporting salary and field	Median years of experience	Thousands of dollars of reported salary				
			Median	Lowest decile	Lower quartile	Upper quartile	Highest decile
All fields							
Women	303	17.9	9.9	6.8	8.0	12.8	15.0
Men	9,285	14.3	12.0	7.8	9.4	16.0	20.0
General economic theory							
Women	36	16.4	8.7	5.6	7.1	10.3	13.4
Men	956	12.9	10.8	7.1	8.5	14.4	19.5
Econ. systems, development, etc.							
Women	25	13.8	9.0	6.8	7.0	13.7	16.0
Men	676	13.0	12.2	7.6	9.2	16.5	19.5
Monetary-fiscal							
Women	36	9.4	8.0	6.3	7.0	11.5	15.2
Men	731	13.7	11.6	7.3	8.7	16.0	20.0
Business, finance, etc.							
Women	49	17.0	9.3	6.7	8.1	11.3	14.5
Men	3,668	15.1	13.0	8.4	10.0	17.5	24.0
Labor economics							
Women	43	20+	10.3	7.1	8.9	13.0	15.4
Men	474	16.3	12.0	7.7	9.0	15.0	17.5
Welfare, population, etc.							
Women	37	20+	12.0	7.0	9.6	14.1	17.0
Men	96	16.2	11.5	7.5	9.0	14.6	17.2

[1] All fields from which 25 or more women reported salary.

TABLE Q—WOMEN ECONOMISTS: NUMBER AND PERCENT OF TOTAL, BY FIELD OF SPECIALIZATION AND YEARS OF EXPERIENCE
(Total numbers registered and numbers reporting salary and experience; six experience groups)

Field and years of experience	Number		Women as percent of total reporting salary and experience
	Total registered	Reporting salary and experience	
Economic theory	50	36	3.6
Under 2 years	3	2	5.9
2–4 years	12	6	4.3
5–9 years	6	3	1.5
10–14 years	5	5	2.6
15–19 years	8	7	4.2
20 years and over	16	13	5.2
Economic history	16	10	5.0
2–4 years	1	—	—
5–9 years	2	1	3.3
10–14 years	2	1	2.6
15–19 years	4	4	14.3
20 years and over	7	4	5.8
Economic systems, etc.	40	25	3.6
Under 2 years	2	1	4.3
2–4 years	8	4	4.6
5–9 years	10	5	3.4
10–14 years	66	4	2.7
15–19 years	5	5	4.6
20 years and over	9	6	3.3
Economic statistics	25	19	5.1
Under 2 years	2	1	5.6
2–4 years	4	2	2.9
5–9 years	8	6	7.4
10–14 years	2	1	1.5
15–19 years	3	3	6.3
20 years and over	6	6	6.6
Monetary-fiscal	60	36	4.7
Under 2 years	3	1	5.0
2–4 years	14	9	7.0
5–9 years	17	9	6.4
10–14 years	6	5	3.7
15–19 years	3	—	—
20 years and over	17	12	5.3

TABLE Q—(*Continued*)

Field and years of experience	Number		Women as percent of total reporting salary and experience
	Total registered	Reporting salary and experience	
nternational economics	35	20	4.6
Under 2 years	2	1	5.9
2–4 years	5	1	2.6
5–9 years	6	3	3.3
10–14 years	3	2	2.4
15–19 years	6	4	7.8
20 years and over	13	9	6.0
Business, finance, etc.	68	49	1.3
Under 2 years	1	—	—
2–4 years	7	6	1.9
5–9 years	12	8	1.2
10–14 years	10	9	1.2
15–19 years	7	5	.8
20 years and over	31	21	1.7
Industrial organization, etc.	25	18	2.8
Under 2 years	2	2	12.5
2–4 years	4	2	2.6
5–9 years	2	2	2.1
10–14 years	3	2	7.6
15–19 years	3	2	2.2
20 years and over	11	8	3.5
Labor economics	64	43	8.3
Under 2 years	2	2	18.2
2–4 years	5	1	2.0
5–9 years	6	3	3.9
10–14 years	10	6	6.7
15–19 years	6	6	6.7
20 years and over	35	25	12.5
Welfare, population, etc.	46	37	27.8
2–4 years	3	2	20.0
5–9 yerrs	3	2	11.1
10–14 years	7	55	22.7
15–19 years	3	3	15.0
20 years and over	30	25	41.7
Economics, other	12	6	6.1

TABLE R—WOMEN ECONOMISTS: NUMBER AND PERCENT OF TOTALS OF WOMEN AND OF MEN, BY SELECTED YEARS OF EXPERIENCE AND TYPE OF EMPLOYER
(Experience: Total, and 20 or more years)

Years of experience Type of employer	Number		Percent of total	
	Women	Men	Women	Men
All Years of Experience				
Total	446	10,892	100.0	100.0
Educational institutions	206	4,601	46.2	42.2
Federal government	79	1,134	17.7	10.4
Other government	23	250	5.2	2.3
Non-profit organizations	18	422	4.0	3.9
Industry and business	47	3,659	10.5	33.6
Other employers	16	426	3.6	3.9
Not employed	49	328	11.0	3.0
Employment not reported	8	72	1.8	.7
20 or more Years of Experience				
Total	181	3,539	100.0	100.0
Educational institutions	68	1,217	37.6	34.4
Federal government	50	437	27.6	12.3
Other government	14	78	7.8	2.2
Non-profit organizations	5	130	2.8	3.7
Industry and business	18	1,274	9.9	36.0
Other employers	4	171	2.2	4.8
Not employed	18	204	9.9	5.8
Employment not reported	4	28	2.2	.8

TABLE S—WOMEN ECONOMISTS: SALARIES BY ACADEMIC DEGREE
(Median, decile and quartile salaries)

Academic degree	Number reporting degree	Thousands of dollars of reported salary				
		Median	Lowest decile	Lower quartile	Upper quartile	Highest decile
Total	318	10.0	6.8	8.0	12.5	15.0
Ph.D.	129	10.3	7.5	8.8	13.0	15.0
Master's	138	8.8	6.0	7.1	12.4	15.2
Bachelor's	49	9.9	6.8	8.2	12.1	15.4
Less than bachelor's	2	—	—	—	—	—